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QUESTIONS SURROUNDING THE 'HOCKEY STICK' TEMPERATURE STUDIES: IMPLICATIONS FOR CLIMATE CHANGE ASSESSMENTS

HEARINGS

BEFORE THE

SUBCOMMITTEE ON OVERSIGHT AND
INVESTIGATIONS

OF THE

COMMITTEE ON ENERGY AND
COMMERCE
HOUSE OF REPRESENTATIVES

ONE HUNDRED NINTH CONGRESS

SECOND SESSION

JULY 19 AND JULY 27, 2006

Serial No. 109-128

Printed for the use of the Committee on Energy and Commerce

Available via the World Wide Web: http://www.access.gpo.gov/congress/house

U.S. GOVERNMENT PRINTING OFFICE

31-362PDF WASHINGTON : 2006

For sale by the Superintendent of Documents, U.S. Government Printing Office
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CONTENTS

Page

Hearings held:
- July 19, 2006 1
- July 27, 2006 603

Testimony of:
- Wegman, Dr. Edward J., Center for Computational Statistics, George Mason University 39
- North, Dr. Gerald R., Department of Atmospheric Sciences, Texas A&M University 52
- Karl, Dr. Thomas R., Director, National Climatic Data Center, National Oceanic and Atmospheric Administration, U.S. Department of Commerce 127
- Crowley, Dr. Thomas J., Nicholas Professor of Earth Science, Duke University 138
- von Storch, Dr. Hans, Director of Institute for Coastal Research, GKSS Research Center, Germany 215
- McIntyre, Stephen, Toronto, Ontario, Canada 236
- Mann, Dr. Michael E., Associate Professor and Director, Earth System Science Center, The Pennsylvania State University 640
- Christy, Dr. John R., Professor and Director, Earth System Science Center, NSSTC, University of Alabama in Huntsville 654
- Cicerone, Dr. Ralph J., President, National Academy of Sciences 674
- McIntyre, Stephen, Toronto, Ontario, Canada 682
- Gulledge, Dr. Jay, Senior Research Fellow, Pew Center on Global Climate Change 696
- Wegman, Dr. Edward J., Center for Computational Statistics, George Mason University 705

Additional material submitted for the record:
- North, Dr. Gerald R., Department of Atmospheric Sciences, Texas A&M University, response for the record 586
- Crowley, Dr. Thomas J., Nicholas Professor of Earth Science, Duke University, response for the record 585
- Mann, Dr. Michael E., Associate Professor and Director, Earth System Science Center, The Pennsylvania State University, response for the record 764
- Christy, Dr. John R., Professor and Director, Earth System Science Center, NSSTC, University of Alabama in Huntsville, response for the record 770
- Cicerone, Dr. Ralph J., President, National Academy of Sciences, response for the record 780
- McIntyre, Stephen, Toronto, Ontario, Canada, response for the record 784
- Wegman, Dr. Edward J., Center for Computational Statistics, George Mason University, response for the record 829

QUESTIONS SURROUNDING THE 'HOCKEY STICK' TEMPERATURE STUDIES: IMPLICATIONS FOR CLIMATE CHANGE ASSESSMENTS

WEDNESDAY, JULY 19, 2006

HOUSE OF REPRESENTATIVES,
COMMITTEE ON ENERGY AND COMMERCE,
SUBCOMMITTEE ON OVERSIGHT AND INVESTIGATIONS,
Washington, DC.

The subcommittee met, pursuant to notice, at 10:07 a.m., in Room 2123 of the Rayburn House Office Building, Hon. Ed Whitfield (Chairman) presiding.

Members present: Representatives Walden, Bass, Stearns, Burgess, Blackburn, Barton (ex officio), Stupak, Schakowsky, Inslee, Baldwin, Waxman, and Whitfield.

Staff present: Mark Paoletta, Chief Counsel for Oversight and Investigations; Peter Spencer, Professional Staff Member; Tom Feddo, Counsel; Matt Johnson, Legislative Clerk; Mike Abraham, Legislative Clerk; Ryan Ambrose, Legislative Clerk; David Vogel, Minority Research Assistant; Chris Knauer, Minority Investigator; Lorie Schmidt, Minority Counsel; and Edith Holleman, Minority Counsel.

MR. WHITFIELD. I call this hearing to order this morning.

Albert Gore's first movie, or documentary, entitled "An Inconvenient Truth" is the most recent of many topics in years and years of focus on the subject of global warming, and 95 percent of the American people certainly are familiar with the term "global warming" and they know basically what it means, I would think. However, 95 percent of the American people and certainly 95 percent of the Members of the U.S. Congress have not had the time to examine the data used by scientists, paleoclimatologists, and statisticians nor do they have the inclination to do so, to look at that data that is used to predict the probability that the temperature of one century is warmer or cooler than that of another century.

Now, the United Nations Intergovernmental Panel on Climate Change is the world body with most of the interest and does focus on this subject of global warming. And it is the body that most people look to on this subject. Now, for many years the Intergovernmental Panel on Climate Change used a chart that clearly shows the temperature from

1000 A.D. to about 1450 A.D., that the temperatures during that period were significantly warmer than the latter part of the 20th Century, or the late 1990s. Now, in 1998 and 1999, a paleoclimatologist, Dr. Michael Mann, with Raymond Bradley and Dr. Malcolm Hughes, introduced a new technique to develop more quantitative estimates of the nature of climate change since 1000 A.D. and concluded that the late 20th Century was the warmest in 1,000 years, that the warming during the late 1990s was the warmest in over 1,000 years. Now, as a result of that report, the IPCC incorporated the study with other data which eliminated the warming period for 1000 A.D. to 1450 A.D. and incorporated a new graph referred to as the "hockey stick" graph, which shows remarkable warming in the late 1990s. Now, when Chairman Barton and I wrote a letter asking that the Mann report be reviewed by some statisticians, there was a hue and cry around the country among many people in the news media that we were being totally political, that all we were trying to do was gut this issue that global warming is occurring. But I think quite sincerely that we have a responsibility when public policy decisions being made on reports like the Mann report and others have such a broad impact on so much of our society and certainly the Kyoto arguments were primarily based on this new chart, that the U.S. should be part of Kyoto. That was an important part of that. And so what we did was, we asked that Dr. Wegman and a team that he had review these data. Now, when we did that, Sherry Boehlert, who is a good Republican friend of ours and is Chairman of the Science Committee, was quite upset about it and he said I think you all are being political also, and he asked that we ask Dr. North, who is going to be a witness, and would like for him to be involved in this data analysis, and he is going to be a witness today also. But the real purpose of this is that this issue is so important that I think it is imperative that we hear from all sides and try to get some real understanding of the strengths and weaknesses of these reports.

Now, Dr. Wegman is going to testify today that the mathematics used by Mann is incorrect and wrong. Dr. North, I think on page five of his testimony, says that they have some concerns about it, the math. But the first witness today is going to be Dr. Edward Wegman, a statistician from George Mason University, and on his team was Dr. David Scott from Rice University and Dr. Yasmin Said from Johns Hopkins, and she is sitting behind him there. Dr. Wegman is Chairman of the National Academy of Sciences Committee on Applied and Theoretical Statistics, and at the committee's request he assembled this ad hoc committee of statisticians to examine the hockey stick studies and related articles and his committee report prepared for Chairman Barton and me and the committee and publicly released this Friday provides important findings for Congress and the public to consider about the soundness and openness of climate change research and assessment and I can tell you right now that his document has been peer reviewed also, and we will get into that later.

In addition to Dr. Wegman, we have Dr. Gerald North of Texas A&M University, who will testify on the first panel about the current state of historical temperature understanding. Dr. North chaired a recent National Research Council panel on historical temperature reconstructions and we look forward to hearing his perspective for improving climate change assessments. And to help us understand some particulars of the IPCC process, we will hear testimony on the second panel from Dr. Thomas Karl, who is a coordinating author of the chapter upon which Dr. Mann and his colleagues worked. Dr. Thomas Crowley of Duke University will be here and Dr. Hans von Storch, who traveled from Germany to be with us this morning. Both will provide their views concerning the questions about the hockey stick study as well as questions concerning data sharing, transparency and the IPCC process.

Finally, I would like to welcome Mr. Stephen McIntyre, who will testify about attempting to understanding just what was behind the hockey stick graphic promoted by the IPCC. His work is a testament to the value of open debate and scrutiny.

Now, I have talked about Dr. Mann and we invited Dr. Mann to be here today and he was unable to be here. We are extending another invitation for him to come and hope that maybe he will be here next week. Now, even though Dr. Mann could not come, he specifically asked us to request Dr. Crowley to testify on his behalf and Dr. Crowley is with us today from Duke University, and we look forward to his testimony. But as I said, the real purpose of this hearing is, let us just open the book. Let us look at everything. Let us look at the criticisms of all parties and see exactly where we are on this important issue of global climate change.

[The prepared statement of Hon. Ed Whitfield follows:]

PREPARED STATEMENT OF THE HON. ED WHITFIELD, CHAIRMAN, SUBCOMMITTEE ON OVERSIGHT AND INVESTIGATIONS

Good morning and welcome. We convene this hearing today to consider questions that begin with and surround the reliability of two particular studies of historical temperatures that gained an extraordinary level of public prominence a few years ago, and recently featured in former Vice President Al Gore's motion picture, "An Inconvenient Truth."

In 2001, the results of these studies were used to promote the view that the very recent average temperatures of the northern hemisphere were likely the warmest in 1,000 years. The temperature history results were portrayed in what is widely known as the 'hockey stick' graph, for its resemblance to the shape of a hockey stick. As a result, these studies are known as the "hockey stick" studies.

With its relatively long and even trend for 900 years and then sharp up-tick during the 20th Century, the “hockey stick” graph effectively undermined what had been the prevailing view that we had experienced periods of similar or even higher average temperatures in the past – such as when the Vikings inhabited Greenland.

The fact that the United Nation’s Intergovernmental Panel on Climate Change, or IPCC, prominently relied upon the graph lent the graph its apparent authority. The IPCC is an influential international body that conducts scientific assessments for use by policymakers.

The graph offered a simple and powerful message for the public and policymakers to understand. It was also a message that some say may have been based on faulty methodology. The “hockey stick” studies formed the basis for the IPCC finding in 2001 that the 1990s were likely the warmest decade of the millennium and 1998 likely the warmest year during that time. Some of today’s witnesses will describe in detail that the “hockey stick” studies were critically flawed and could not support the findings reached by these studies.

Had the ‘hockey stick’ studies remained in the niche of climate change journals, we would not be holding this hearing. Instead, we are here because the questions surrounding these studies relate directly to the strength of the findings in the first place. What does the “hockey stick” story say about the reliability of these studies for policymakers?

Last summer, Chairman Barton and I inquired into this matter after we learned that the lead author of these federally funded studies – Dr. Michael Mann -- to share the computer code he used to generate his results with researchers who sought to replicate the result of Mann’s studies. The researchers, one of whom will testify today, reportedly could not replicate his work based on what the study said. The researchers nevertheless identified several methodological and data problems with the work.

How critical were these problems identified by these researchers? Were the problems undetected because Dr. Mann assessed his own work in an IPCC report?

These are serious questions, and the answers contain broad implications for global policy on climate change. We should ensure that science is providing us with reliable, balanced, well-considered, and unbiased answers.

Today, our witnesses will help us address these critical questions.

I want to welcome, especially, Dr. Edward Wegman, a statistician with George Mason University, who will lead off the first panel this morning. Dr. Wegman is Chairman of the National Academy of Sciences Committee on Applied and Theoretical Statistics. At the Committee request, Dr. Wegman assembled an ad-hoc committee of statisticians to examine the hockey stick studies and related articles. His committee’s report, prepared for Chairman Barton and me and publicly released this past Friday, provides important findings for Congress – and the public – to consider about the soundness and openness of climate change research and assessments. The Wegman Committee not only identified fundamental flaws in the “hockey stick” studies, it also addressed the larger point that climate change studies, like any work with potentially large policy implications, must be subject to careful and broad scrutiny.

Dr. Wegman and his team performed their work completely independent of the Committee and without charge. I believe Dr. Wegman’s team has done a great public service and their work should help us improve how we discuss climate change when crafting policy.

Additionally, Dr. Gerald North, of Texas A&M University, will testify on the first panel about the current state of historical temperature understanding. Dr. North chaired a recent National Research Council panel on historical temperature reconstructions, and I look forward to hearing his perspective for improving climate change assessments.

To help us understand some particulars of the IPCC process, we’ll hear testimony on the second panel from Dr. Thomas Karl, who was a coordinating author of the chapter upon which Dr. Mann and his colleagues worked. Dr. Thomas Crowley, of Duke University, and Dr. Hans von Storch – who traveled from Germany to be with us this morning – both can provide their considered views concerning the questions about the "hockey stick" studies, as well as questions concerning data sharing, transparency, and the IPCC process.

Finally, I'd like to welcome Mr. Steven McIntyre. Mr. McIntyre will testify about attempting to understand just what was behind the hockey stick graphic promoted by the IPCC. His examination of the facts underlying the assessments' claims really initiated some of the important questions concerning the scrutiny provided by climate change assessments. His work is a testament to the value of open debate and scrutiny. His perseverance should be commended.

Let me add that we did invite Dr. Mann to this hearing, but his attorney explained that he was unavailable, on family vacation. Dr. Mann suggested Dr. Crowley could come in his place. We do hope to have Dr. Mann at a future hearing, however.

At the end of the day, the issues of climate change require open and objective discussion. Some of the work we'll consider today points to the value of policy decisions that are informed by sound science and objective advice.

I'll now yield to Mr. Stupak, our ranking member, for his opening statement.

MR. WHITFIELD. At this time, I would like to recognize Mr. Stupak of Michigan for his opening statement.

MR. STUPAK. Thank you, Mr. Chairman.

It is a little bewildering to me why the committee is holding its very first hearing on global warming to referee a dispute over a 1999 hockey stick graph of global temperatures for the past millennium. Mr. Chairman, in your opening statement you claim that Dr. Mann's hockey stick report of 1999 was the basis for the Kyoto Accord. According to my recollection, Kyoto was in 1997, so it could not have been the basis for the Kyoto Accord.

So as we will hear at this hearing today, global warming science has moved on since Dr. Mann put forth his study in 1999. Dr. Mann, who did this study, has made changes and even such diehard opponents as President Bush now actually admit that global warming exists and must be addressed. Congress is particularly ill-suited to decide scientific debates. There has been no attempt by this committee to obtain an unbiased view of the work done by Dr. Michael Mann, the author of the hockey stick research. Dr. Mann, who has done additional work with his methodology since 1999, is not even here to confront his critics because the Majority would not even postpone this hearing until Dr. Mann could be available. Moreover, it was known from the beginning that Dr. Mann used a new methodology and proxy material to reconstruct temperatures.

Paleoclimatologists, those who try to reconstruct ancient climates, are not working with instrumental measurements of temperature as we have today. Paleoclimatologists are looking at tree rings, ice cores, bore heads and historical records to attempt to determine what happened in an earlier time. That is all the research materials paleoclimatologists have and it is an admittedly imprecise science. It should not surprise us if the initial work in a new field can be improved. What should surprise us is that Dr. Wegman's report focuses on critiques of Dr. Mann's first work in 1998 and 1999, even though the field of large-scale temperature reconstruction has advanced since that time.

The Majority paid for a report to independently verify the critiques of Dr. Mann's 1999 research by a statistician but without any input from a climatologist. The Majority left it to the Science Committee to ask the National Academy of Sciences to do a full review of all the science represented. The Majority made no effort to verify whether the patterns in global temperatures detected in the Mann study were valid or coincided with conclusions of other researchers in global warming.

It is now 7 years since the original work was published and much additional work has been done by Dr. Mann and others. As we will hear from Dr. North, who chaired the NAS study, the patterns were verified with certainty for recent years but less certain for the years 1000 to 1600 A.D. That is to be expected because there is less data from this long ago era. Dr. Wegman has an eminent background in statistics and he believes that statisticians should be included in the research teams of all these studies because statisticians can make studies better. Perhaps they can. Dr. Wegman says Dr. Mann didn't center his data properly. Perhaps he didn't. But we note that Dr. Wegman's work is not yet published or peer reviewed so it is very difficult for us to evaluate his work. Dr. Wegman's criticism of Dr. Mann should have been interdisciplinary and include a statistician can also be said of Dr. Wegman's work. Dr. Wegman did not have a climate scientist on his team. However, Dr. Wegman has decided to go beyond his statistical expertise to hypothesize that Dr. Mann was allowed to publish and defend his work because of the small "social network" of paleoclimatologists who work with each other and protect each other. I want to emphasize that this is simply a hypothesis. Mr. Chairman, whatever the purpose of this hearing is, it is not to hypothesize about the impact of professional scientific relationships on research unless we have some hard objective evidence.

We in Washington know all about undue influence on government scientists. A political appointee at NASA just recently tried to keep James Hanson, a veteran atmosphere scientist, from discussing the dire consequences of global warming by threatening dire consequences to Mr. Hanson's employment status. The science content has been changed on NASA and other government websites because it didn't fit the Administration's world view. This fact ought to be of much more interest to this committee, the Oversight and Investigations Committee, than hypothesis about scientific social networking.

And with that, Mr. Chairman, I would yield back the balance of my time.

MR. WHITFIELD. Mr. Stupak, thank you. I also want to thank you for pointing out an incorrect statement that I made. I said something about the hockey stick being the impetus for Kyoto. Kyoto certainly started way before the hockey stick but the hockey stick graph did add impetus to the argument for the adoption of Kyoto, so I want to thank you for that. Also, I would point out that the committee did not pay Mr. Wegman for this report, we simply contacted him asking him to review it.

At this time I recognize the full committee Chairman, Mr. Barton.

CHAIRMAN BARTON. Thank you, Mr. Chairman. I have a written statement, I am going to use some of it, but I want to speak extemporaneously briefly based on what my good friend from Michigan, Mr. Stupak, just said.

The purpose of oversight and investigation is to do exactly that, to oversee the jurisdictional issues before this committee and when it seems to be called for to investigate issues that arise because of the oversight. There has been a disagreement for a number of years in the community at large about the issue of global warming. In this Congress, there has been a disagreement between the Chairman of the Science Committee and myself about that issue. That is normal and that is not anything that is a negative. But there were some statements made about a specific report by a number of people that basically use that report to come to the conclusion that global warming was a fact and that the 1990s was the hottest decade on record and that one year, 1998, was the hottest year in the millennium. Now, a millennium is a thousand years. That is a pretty bold statement. So Chairman Whitfield and myself decided, let us take this report that is the basis for many of these conclusions and has been circulated widely and once it is in the mainstream, it is stipulated that because of that, everything else follows and let us see if it can be replicated. Let us see if in fact the facts as purported in that report are in truth the facts.

Now, I have not seen Dr. Wegman until I walked in this room. I have not talked to him on the phone or in person or any of his collaborators. I may have seen Dr. North at Texas A&M since I went to Texas A&M. I don’t recall it but it is possible. He has got enough white in his hair that I could have been one of his students and I wouldn’t remember it, so I can’t stipulate that I have never met him but I can stipulate that I have never met Dr. Wegman. We asked to find some experts to try to replicate Dr. Mann’s work. Now, to their credit, when Dr. Wegman agreed to do it, he asked for no compensation. I don’t think we have even paid him for the fax paper that he has used. He picked some eminent statisticians in his field and they studied this thing. Had their report said Dr. Mann's data can be replicated, his conclusions are right on point, he is totally correct, we would have reported that, but that is not what they said. Now, I took statistics at Texas A&M and I also took them in graduate school. I made A's and B's, but I really didn't understand it but I kind of understand it. And according to Dr. Wegman, Dr. Mann made a fundamental error. He decentered the data. Now, to the average person, that doesn't mean squat. What does "decentered the data" mean? What it means apparently is, he moved it off center a little bit by enough that it really makes a difference and then using some statistical techniques that instead of looking at all the variables and in a complex system like climate you are going to have lots of variables, he chose one or two as the principal variables and used those to explain everything else, and Dr. Wegman and his colleagues who as far as I know have got no axe to grind, have said the Mann study is flat wrong. Now, it may be wrong just kind of unintentionally. Dr. Wegman doesn't say there is any intent to deceive but he says it is flat wrong. Now, if that is not the purpose of the Oversight Subcommittee of the Energy and Commerce Committee that has got jurisdiction over energy and environmental policy for the United States of America, then I don't know what this subcommittee should be doing.

So I want to thank Dr. Wegman and his colleagues for giving us an unvarnished, flat out non-political report. Now, admittedly, that report is going to be used probably for political purposes but that is not what he did, and I want to thank Dr. North for the work that he did in this document. Now, it is a lot thicker than Dr. Wegman's document, and Dr. North and his colleagues have kind of looked at the same subject and they have come to a somewhat little--they are little bit more, I don't want to use the technical term wishy-washy but they are kind of on both sides of it, but even Dr. North's report says that the absolute basic conclusion in Dr. Mann's work cannot be guaranteed. This report says it is plausible. Lots of things are plausible. Dr. Wegman's report says it is wrong.

Now, what we are going to do after today's hearing, we are going to take Dr. Wegman's report, and if my friends on the Minority want to shop it to their experts, so be it. We are going to put it up there, let everybody who wants to, take a shot at it. Now, my guess is that since Dr. Wegman came into this with no political axe to grind, that it is going to stand up pretty well. If Dr. Mann and his colleagues are right, their conclusion may be right--Dr. Mann's conclusion may be right but you can't verify it from his statistics in his model so if Dr. Mann's conclusion is right, it is incumbent upon him and his colleagues to go back, get the math right, get the data points right, get the modeling right. That is what science is about.

So I want to thank you, Mr. Chairman, for holding this hearing. I am planning to participate fully and extensively. I have got a whole series of questions. I stayed up half the night studying all the various documents so I hope that by the end of today we can shed some light on a subject that is very, very important to the future economic and health consequences for this country. Thank you, sir.

[The prepared statement of Hon. Joe Barton follows:]

PREPARED STATEMENT OF THE HON. JOE BARTON, CHAIRMAN, COMMITTEE ON ENERGY AND COMMERCE

Thank you, Chairman Whitfield. Today's hearing on the hockey stick temperature studies will show why we need to question the quality of climate assessments for policy makers.

This Committee frequently confronts some of our Nation's most consequential public policy questions affecting the quality of human health, our economy, and our environment. However, no issue we deal with has more potential to affect the American people than climate change.

Meanwhile, the compounding costs to the U.S. economy posed by some proposals for reducing greenhouse gas emissions could rock our economy, drive manufacturing off-shore, and spike domestic consumer energy costs.

That is why we need to be sure that we have a solid factual basis for whatever decisions we make in this area.

The report we are about to receive indicates that the social and statistical underpinnings of key climate-change work are prone to produce error.

I look forward to hearing from all of our witnesses because we have important work to accomplish today. I would especially like to thank Dr. Edward Wegman who, on his own time and his own expense, assembled a pro bono committee of statisticians to provide us with independent and expert guidance concerning the hockey stick studies and the process for vetting this work.

Dr. Wegman and his committee have done a great public service. Their report, with clear writing and measured tone, has identified significant issues concerning the reliability of some of the climate change work that is transmitted to policymakers and characterized as well scrutinized. The Wegman Committee report will be the centerpiece of today's hearing.

These 'hockey stick' studies were the linchpin for what became widely acclaimed as the *consensus* view of the earth's temperature history during the past thousand years. It was presented as part of the leading climate assessment for public policy makers around the world – the United Nation's Intergovernmental Panel on Climate Change, or IPCC.

Both good science and good public policymaking demand that scientific work withstand independent and impartial scrutiny. Information that is not scientifically sound is just not acceptable. Indeed, it appears that some of the authors of the IPCC assessment dealing with global temperature history were not independent or impartial. They also happened to be the authors of the hockey stick studies, themselves. The researchers then declined to provide the information necessary to replicate their work, a fundamental failure in reliable science.

The "hockey stick" studies were supported by Federal grants and were central to a prominent finding in an influential assessment. In my view, if Congress is going to make policy decisions based on the authority of climate change assessments, we cannot fail to wonder how they have been formulated. Asking questions is at the core of what we do.

Our central question is: Can we count on hockey stick studies? That answer from Dr. Wegman and his panel appears to be, “No.” And it doesn’t appear to be a matter of overlooking the researchers’ written caveats about their particular work; rather, the Wegman panel has identified a fundamental error of methodology. If that finding holds up, it will highlight a mistake that lay dormant for years as a closed network of supportive colleagues saw and heard what it wanted. It took scientists outside the network to identify the core problems, both in the studies and in the IPCC assessment.

Congress is in the business of making policy decisions that affect the lives of real people. Science provides us with the answers to many policy questions, and we need to trust it. I do trust science, and I trust it most when it is transparent, open to question, and eager to explain. When research is secretive, automatically and aggressively defensive, and self-reinforcing, it becomes easy to distrust.

As Chairman of the Energy and Commerce Committee, which holds a key role in any policy making relating to climate change, I believe it is incumbent on this Committee must ensure that the very best information is available to make its decisions.

Caveats and uncertainty are facts of life, and not only in science. We deal with complicated science and research-based decisions and uncertainty in every area of our jurisdiction. Some of the most troubling work we confront – on bioterror or radiological risks for example – present very tough and complicated issues for us to assess.

Good science is built on healthy skepticism, and good scientists don’t hide from questions. They invite them. Asking questions to establish the validity of scientific studies – especially those with enormous policy implications – is why we are here today. The caveats and uncertainty are never going to be eliminated, but we would like to know whether the facts or caveats contained in these sophisticated climate assessments have been adequately and independently scrutinized.

Heads-I-win, tails-you-lose science can produce any answer that is desired, but that’s hardly the way to make multi-billion-dollar decisions. This is a vitally important matter. When we deal with global warming, we need to know that the underlying studies constitute reliable science. The taxpayers depend on it. My grandchildren depend on it. The planet depends on it.

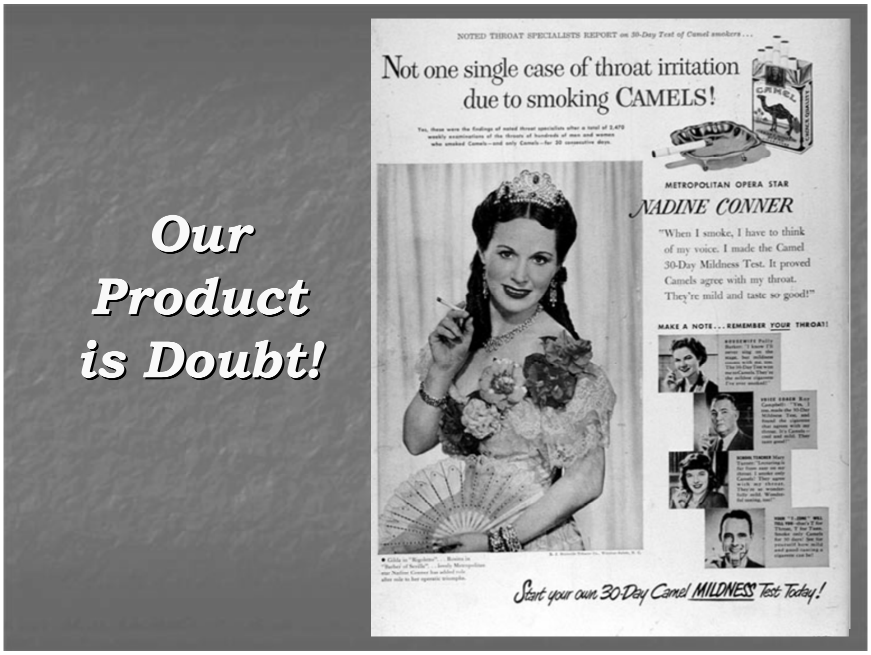
I want to extend my thanks to all the witnesses for appearing today, and I look forward to their testimony.

MR. WHITFIELD. Thank you, Mr. Chairman. At this time I recognize Mr. Inslee of Washington.

MR. INSLEE. Thank you, Mr. Chairman.

America is fully capable of dealing with global warming but not if Congress engages in snipe hunts, arguments about how many statisticians can dance on the head of a pin rather than figuring out what our energy policy should be to get a handle on global warming.

Now, why are we in this exercise for doubt? I refer you to the first slide I have, which is a memo from the tobacco industry when they were fighting the clear, unalloyed science that tobacco was bad for you. Here is a memo from one of their people: “Doubt is our product.” And those who decide that America should stay quiescent, do nothing about global warming, doubt is their product.



Our Product is Doubt!

NOTED THROAT SPECIALISTS REPORT on 30-Day Test of Camel smokers . . .

Not one single case of throat irritation due to smoking CAMELS!

Yes, these were the findings of noted throat specialists after a total of 2,470 weekly examinations of the throats of hundreds of men and women who smoked Camels—and only Camels—for 30 consecutive days.

METROPOLITAN OPERA STAR

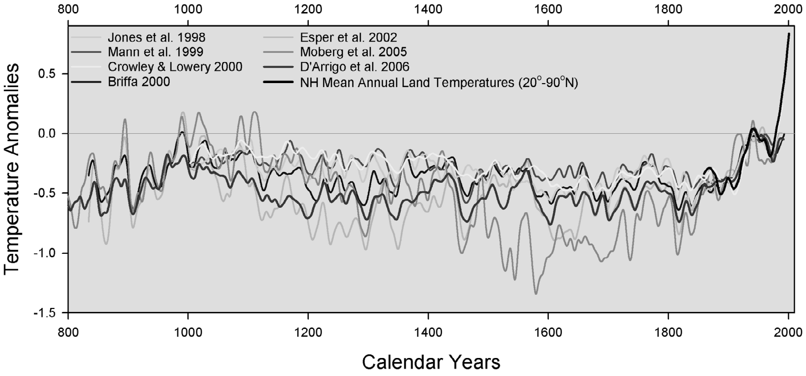
NADINE CONNER

"When I smoke, I have to think of my voice. I made the Camel 30-Day Mildness Test. It proved Camels agree with my throat. They're mild and taste so good!"

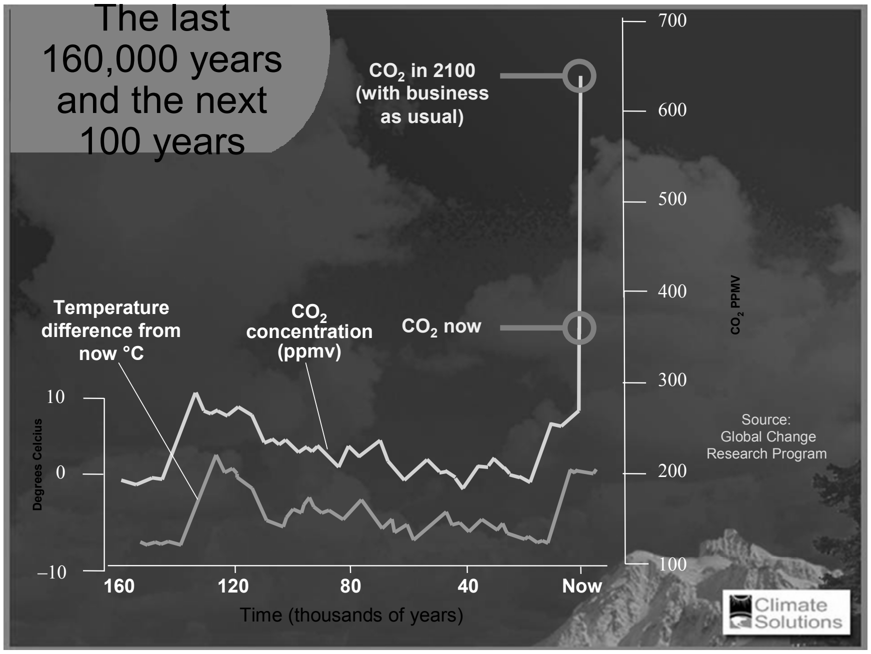
MAKE A NOTE . . . REMEMBER YOUR THROAT!

Start your own 30-Day Camel MILDNESS Test Today!

Next slide.



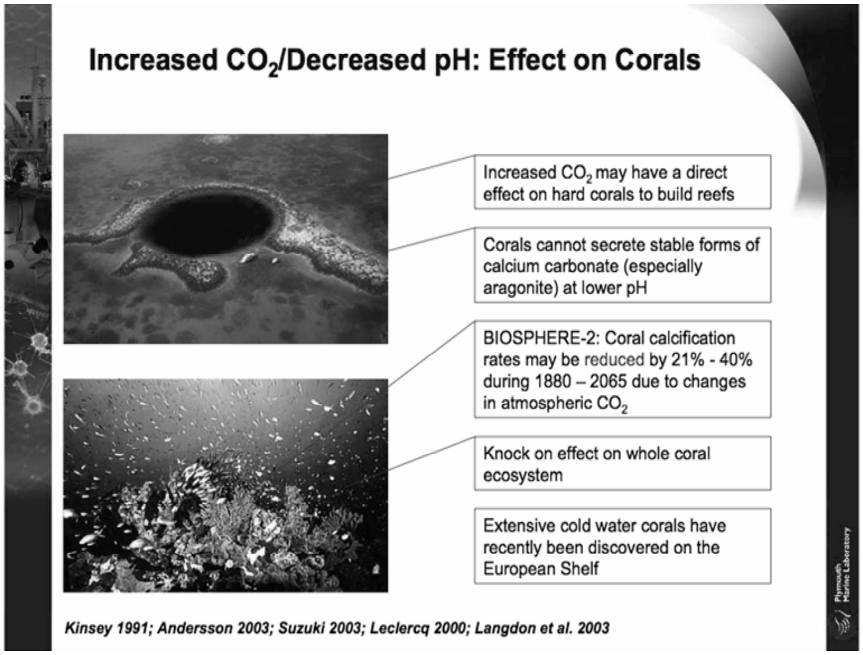
Why should we deal with this? What we are going to find out today, I hope, we can spend weeks debating the statistics behind one particular study but what we will find is that every single study ever that has looked at proxy data for temperature has indicated we are in a unique circumstance and carbon dioxide is going through the roof which you will see from these studies, multiple of which are on this slide. Next slide.



The last 160,000 years and the next 100 years

Source: Global Change Research Program

What we find now is that CO_2 is going through the roof. No one in this room will say otherwise. The first bottom circle is where we are today. It is higher CO_2 levels than any time in the last 160,000 years. Every single scientist in the world agrees to that fact, and by 2100 the circle on top, it will be almost twice as it has ever been in the last 200,000 years. Every single scientist in the world agrees to that fact, and because CO_2 drives climate, because it drives temperature, we ought to get out of this posture of the ostrich and assume the posture of the eagle to do something about global warming. Next slide, please.


Plymouth Marine Laboratory

Increased CO_2/Decreased pH: Effect on Corals

Increased CO_2 may have a direct effect on hard corals to build reefs

Corals cannot secrete stable forms of calcium carbonate (especially aragonite) at lower pH

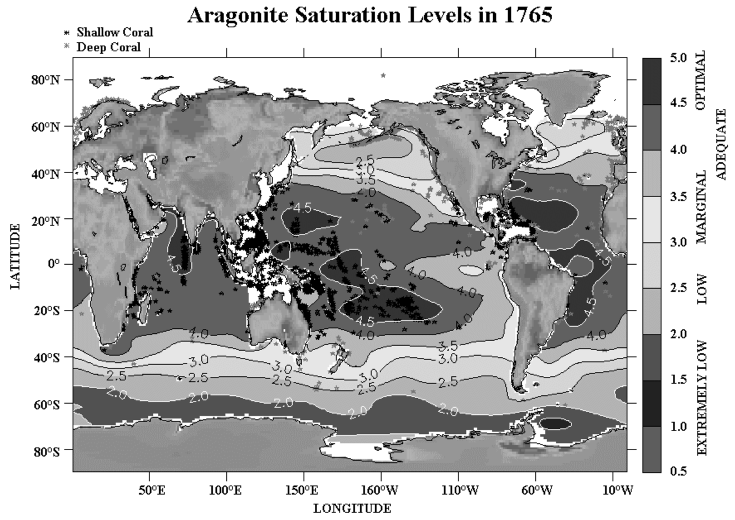
BIOSPHERE-2: Coral calcification rates may be reduced by 21% - 40% during 1880 – 2065 due to changes in atmospheric CO_2

Knock on effect on whole coral ecosystem

Extensive cold water corals have recently been discovered on the European Shelf

Kinsey 1991; Andersson 2003; Suzuki 2003; Leclercq 2000; Langdon et al. 2003

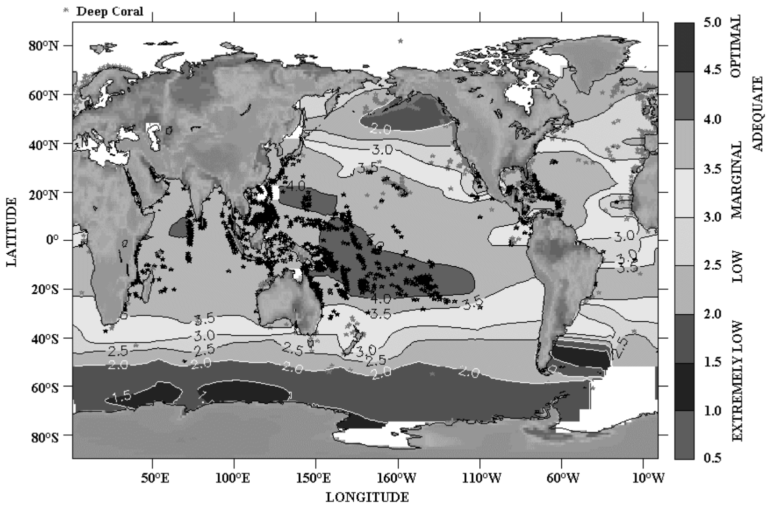
I want to point out something that is very important in today's discussion. We can spend years debating what the temperature was on July 18, 972, but what we ought to know is that our putting CO_2 into the atmosphere is destroying the world's oceans regardless of the temperature. The new science shows that the CO_2 that we put in the atmosphere is acidifying the oceans. The oceans have 23 percent more hydrogen ions that create acidic conditions than any time ever that we know of in human history, at least. Next slide, please.


Aragonite Saturation Levels in 1765

Saturation levels provided by Orr et. al. Submitted

The result of that is that when the oceans become more acidic, it becomes much more difficult for any life including plankton, coral reefs, clams, oysters, you name it to form shells including plankton, which is the basis of the entire food chain of all the protein we get out of the oceans. Next slide, please.

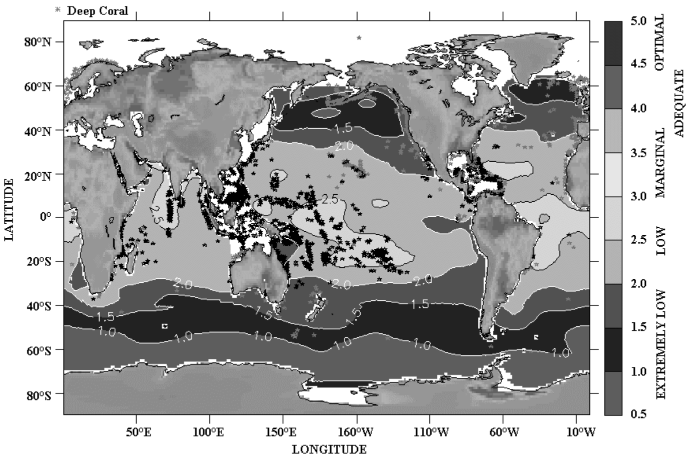
Aragonite Saturation Levels in 1994



Saturation levels provided by Orr et. al. Submitted

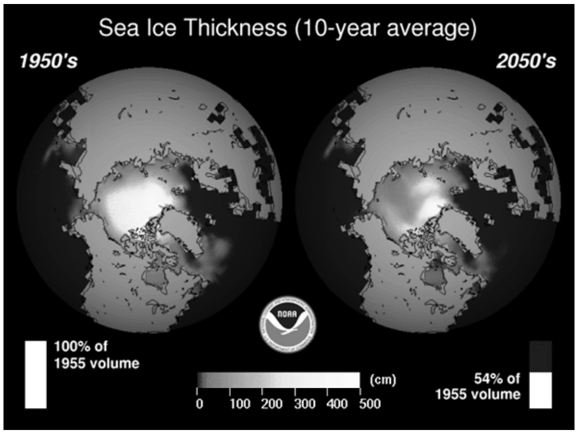
What this shows is the pH level of acidity is changing. Next slide, please.

Aragonite Saturation Levels in 2099

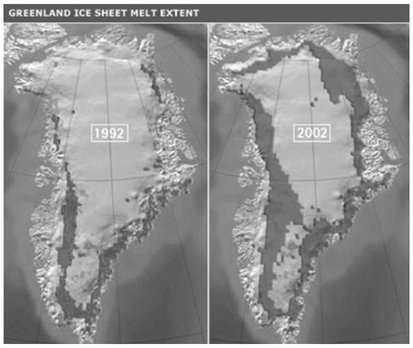


Saturation levels provided by Orr et. al. Submitted

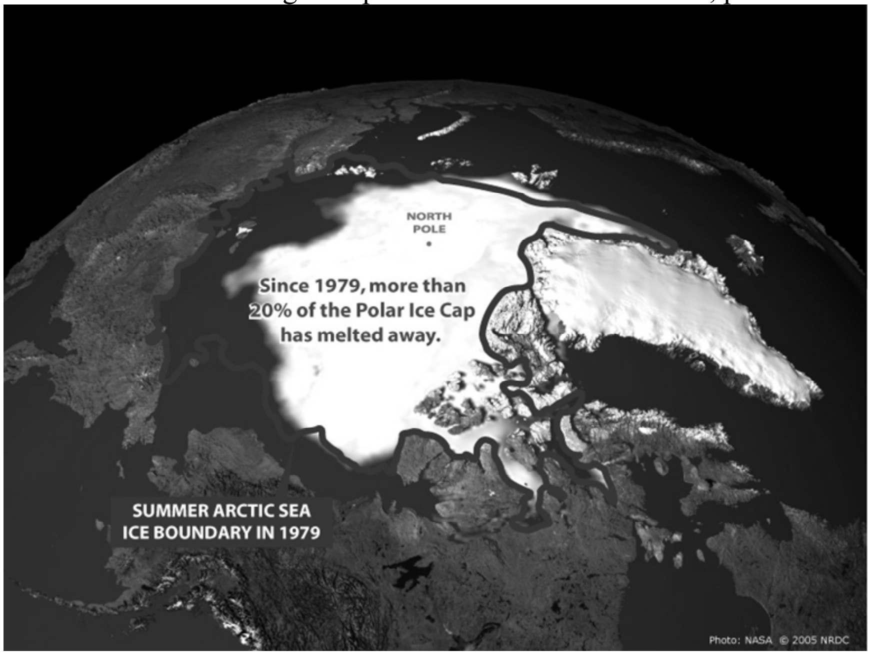
So that by the year 2099, conditions in the ocean may not support any coral reefs healthy anywhere in the world. This doesn't have anything to do with Dr. Mann's report. Even if temperatures did not change one-half a degree, the oceans are becoming acidic that may not support the protein that we depend on in the world if we don't act and if this committee continues to act like an ostrich. Next slide, please.



Why are Americans rejecting this doubt they see with their own eyes? Polar icecaps shrunk in density--next slide, please--in the last 12 years.



Greenland is melting at unprecedented rates. Next slide, please.


NORTH POLE
Since 1979, more than 20% of the Polar Ice Cap has melted away.
SUMMER ARCTIC SEA ICE BOUNDARY IN 1979
Photo: NASA © 2005 NRDC

The polar icecap has shrunk 20 percent in the summer. The red line shows where it used to be. The white is where it is now. Next slide, please. We have run out of slides. Well, maybe I ought to talk then.

This is very disturbing to me that when the entire world scientific community has reached a conclusion with high levels of certainty that carbon dioxide is going to astrospheric levels, unprecedented in world history, and that when we know beyond a shadow of a doubt the levels of carbon dioxide ultimately will drive temperature changes to areas we do not want to see, that instead of really engaging Congressional talent in figuring out how to deal with this problem, we try to poke little pinholes in one particular statistical conclusion of one particular study where the overwhelming evidence is that we have to act to deal with this global challenge.

It is not fitting for this Congress, America that should lead the technology that drives the energy future of the world, to sit here to ask these fine statisticians to go into mind-numbing detail about whether this particular year was hotter than it was in 980. I don't care whether this year or yesterday was the hottest day. It was pretty hot here yesterday, but I don't care whether it may have been hotter in 980. What I care about is whether there will be snow in the mountains for my kids and grandkids to ski on 50 years from now, and there is not going to be unless this Congress pulls its head out of the sand and acts.

So I look forward to the day that we have a Congress that will adopt the position that we need to deal with technology rather than statistical recreations of the tobacco industry's effort to create doubt. Thank you.

MR. WHITFIELD. Mr. Bass.

MR. BASS. Thank you, Mr. Chairman, and thank you for holding this hearing.

I want to start by saying that in my opinion, there is absolutely nothing inappropriate about the subject of this hearing, and although the data may be mind-numbing, nonetheless there are those--I am probably not one of them--who really get into going through the data and the details and so forth to try to figure out what the problem is. Ultimately, the issue underlying the hearing today and any others that we have is not going to be about math, it is going to be about the effect of the extraction of enormous quantities of hydrocarbons from the middle of the Earth and from underground and the combustion of those hydrocarbons and the resultant impact that that has, if any, on the climate of the world.

Now, in another life when I used to sell architectural panel products for buildings, I was often asked by a customer whether or not the panel that I was trying to sell passed the ASTM, American Society for Testing Materials, E84 test, and I always used to respond because, of course, we couldn't afford to have that test conducted, I used to say well, it hasn't but I subjected it to what I called the elephant foot test and I built--every fall I burned a huge pile of brush in my field on the farm I live on and one year I just took one of the panels that I planned to sell and I threw it on top of the pile and it sat there for 30 minutes and nothing happened. Is that satisfactory? Well, we can spend I think a productive period of time talking about the basis upon which the data was developed to determine the Mann report or the Wegman report or Dr. North's report and so forth, but ultimately I think we need to recognize that there is a problem and anyone who denies the existence of any problem associated with the release of these hydrocarbons I think really needs--I want to be friendly about this--really needs to rethink that premise. There is something going on and I think finding out what that something is and then trying to debate a policy whereby we address that issue is constructive.

So I want to thank my friend from Kentucky for holding this hearing and I look forward to hearing the witnesses' testimony, and I yield back.

MR. WHITFIELD. Thank you, Mr. Bass. At this time I recognize Mr. Waxman of California.

MR. WAXMAN. Thank you very much, Mr. Chairman.

The party that is in the majority selects the Chairman of the committees and the subcommittees and they can decide what priorities ought to be given to different issues and what hearings are to be called. Now, in the past 12 years, we have had study after study after study raising genuine concern about global warming and climate change. The Energy and Commerce Committee is a committee that has legislative jurisdiction over this issue. So for the past 12 years this committee has a very amazing record on this issue. This is only the second hearing in 12 years. The first one was to look at the very intricate issue of modeling on predictions of climate change and this one is to look at studies from 1998 and 1999 to see whether those studies are refuted by the work of the two gentlemen before us today. We have not held a hearing looking at what is the overwhelming scientific consensus that global warming is real and is caused by humans. We have not focused on some of the important recent scientific news on global warming such as a study showing that climate change is causing increased wildfires in the American West or the recent studies that show that global warming is leading to more intense hurricanes.

The committee could go a step further by examining the practical solutions that could begin to reduce our greenhouse gas emissions, and if the committee leadership wanted to conduct important and nonpartisan oversight, it could investigate why a former employee of ExxonMobil operated out of the Bush White House to sow doubt in government publications on global warming. Instead, this committee is doing what the deniers of global warming would have us do, ignore all the important questions and divert ourselves to a ridiculous effort to discredit a climate scientist and two studies he published eight years ago.

Chairman Barton began this dubious investigation in June of 2005 when he sent a letter demanding the funding for every study that had ever been conducted by Dr. Michael Mann, demanding he turn over all of the data for all their research and made over burdensome and intrusive requests. The *Washington Post* accused our Chairman of conducting a witch hunt. The Chairman of the Science Committee, Sherwood Boehlert, called the investigation “misguided” and “illegitimate.” Well, oftentimes when we have scientific disputes we ask the National Academy of Sciences to review the matter. Instead of asking them--even though they offered their services to help resolve controversy--the Academy wasn’t called on by this committee but by Representative Boehlert’s committee and the Academy issued its report last month and they found that they largely upheld the findings of Dr. Mann.

So I have to submit that I don’t find this hearing to be one about truth. It is about sowing doubt and spreading disinformation, and I chaired all those committees over the years where I heard from tobacco executives who always insisted on having their scientists come in and say it is only coincidental that more cancers and other diseases seem to afflict smokers but there is no causal relationship. Not only is this hearing not legit in trying to deal with an important issue, it isn’t even fair. We are going to hear people attacking Dr. Mann but we are not going to have Dr. Mann here to confront the accusations against him. That is not science where you hear only one side. Science is hearing both sides, looking at the evidence, reaching conclusions based on the evidence. Dr. Mann was willing to testify before the committee but his schedule would not be accommodated. Global warming is an incredibly serious problem and this is not a serious hearing.

I would submit that if you have doubts, fine, but prudent people would start doing something in case your doubts on the Republican side of the aisle are wrong. We would start taking measures to reduce these greenhouse gas emissions that seem to be causing enormous damage to our planet and a threat to human life. Instead, we are looking at reports from 8 years ago and trying to debunk them. That is not an indication to me, that and the 12 years of inaction by this committee, that there is any interest on behalf of the Republican leadership to come to terms with what is not a partisan issue at all but one that is a very important issue for us to address.

[The prepared statement of Hon. Henry Waxman follows:]

THE PREPARED STATEMENT OF THE HON. HENRY WAXMAN, A REPRESENTATIVE IN CONGRESS FROM THE STATE OF CALIFORNIA

Today, the Subcommittee holds only the second hearing on global warming in the Energy and Commerce Committee since the Republicans took over the House of Representatives in 1995. With so many important aspects to global warming and twelve years of virtual inaction, there's a lot of important work for the Committee to do.

It could start by highlighting the overwhelming scientific consensus that global warming is real and is caused by humans. Or it could focus on some of the important recent scientific news on global warming, such as the study showing that climate change is causing increased wildfires in the American West or the recent studies that show that global warming is leading to more intense hurricanes.

The Committee could go a step further by examining the practical solutions that can begin to reduce our green house gas emissions. And if it wanted to conduct important and non-partisan oversight, it could investigate why a former employee of ExxonMobil operated out of the Bush White House to sow doubt in government publications on global warming.

Instead, the Committee is doing exactly what the big oil companies hope for it to do...it ignores the important questions and diverts to a ridiculous effort to discredit a climate scientist and a study he published eight years ago.

Chairman Barton began this dubious investigation when he wrote Dr. Michael Mann and several other researchers in June 2005. He demanded to know the source of funding for every study they had ever conducted, demanded they turnover all of the data for all of their research, and made other burdensome and intrusive requests.

The Washington Post accused Chairman Barton of conducting a witch hunt. The Chairman of the House Science Committee Sherwood Boehlert called the investigation “misguided” and “illegitimate.” And the nation’s premiere science organizations quickly condemned the investigation. The American Association for the Advancement of Science wrote to Chairman Barton stating that his letters “give the impression of a search for some basis on which to discredit these particular scientists and findings, rather than a search for understanding.”

The National Academy of Sciences also weighed in, stating that Chairman Barton’s approach was “intimidating” to researchers and offering the services of the Academy to help resolve the controversy.

Ironically, it wasn’t Chairman Barton who took the Academy up on its offer. Instead, Rep. Boehlert requested the Academy report that was released last month. The Academy largely upheld the findings of Dr. Mann.

This hearing isn’t about finding the truth. It’s about sowing doubt and spreading disinformation. The closest parallel is the decades-long campaign of the tobacco industry to deny that nicotine is addictive and cigarettes cause cancer.

And the hearing isn’t even fair. Today we’re going to attack the work of Dr. Mann, but we’re not going to give Dr. Mann a chance to confront the accusations against him. Dr. Mann was willing to testify before the Committee, but his schedule was not accommodated and so he is going to be tried in absentia.

Global warming is an incredibly serious problem, but this is not a serious hearing. It’s a diversion and a delaying tactic. And – worst of all – it is a missed opportunity to begin the process of protecting our children from the catastrophic effects of global warming.

I know that the Chairman of this Subcommittee has never accepted the science about global warming. To bolster his argument over the years, he has repeatedly brought to the attention of the Committee, the views of Gregg Easterbrook and his book, “A Moment on the Earth.”

So, I just want to make sure that the Chairman is aware of Mr. Easterbrook's op-ed from May 26, 2006, in which Mr. Easterbrook announces that he has changed from "a skeptic to a convert." He says that it is "case closed," and that a strong scientific consensus shows that global warming "is a real phenomenon posing real danger."

I am glad that Mr. Easterbrook has revisited his views and corrected them accordingly. I hope the Chairman is willing to do the same.

MR. WHITFIELD. Dr. Burgess, you are recognized.

MR. BURGESS. Thank you, Mr. Chairman, for the recognition. I thank the Ranking Member for pointing out that partisanship has no place in this debate and I hope we won't see it again this morning.

I will point out just for the record that Dr. Mann has been invited to appear before this committee before this hearing this morning. He couldn't be here. Apparently he is on vacation that couldn't be interrupted and maybe he can be here next week, and if he can be here next week, we will certainly be grateful to hear from him, but fortunately we do have his number one colleague, Dr. Crowley, on our second panel and I am grateful for that as I am sure the Minority is as well.

Again, I thank the Chairman for the recognition and I want to thank all of our witnesses for taking their valuable time to be with us here today. I know there are many other productive activities you could have been doing. And we have already heard from our friends on the other side of the room that there does indeed currently exist an international consensus that global warming exists and that human beings have caused it. They didn't say so but I would further extrapolate that it is Americans that have caused it and it is probably one American in particular and he lives in the White House. But I think it is fair to point out that no such consensus exists.

The Earth has been heating and cooling for millions of years. There have been big ice ages, little ice ages and it is fair to say that in between those two cooling events it probably even got a little warm. The Earth's climate is cyclical and we have only been paying attention during the past few hundred years. With the cyclical nature of the Earth's climate, it is plausible to say that the Earth's temperatures would be on the rise today regardless of what humans did or didn't do. Thirty-five years ago, I was a freshman in a geology class and we learned how the Earth itself was spun off as a hot ball of gases and gradually cooled and it was postulated that the Earth had been cooling ever since and indeed perhaps Armageddon would come one day not as a fire or as a flood but as we cooled into that last ice age. Now we have global warming staring us in the face.

I am not saying we should completely dismiss fears of global warming as an inaccurate science. I think that it merits thoughtful and serious debate and we owe the subject matter thoughtful and serious debate. Part of my problem with the whole process is, that it seems that the cleaner we make our energy generation capability, and indeed we have cleaned our energy generation capability over the years, and the Ranking Member can take considerable credit for that with legislation that he has passed, but now we want to come up against an obstacle that nothing can come out of those pipes, we have already taken out the VOX, the NOX, the SOX, the POX, the TOX. Now it is the carbon dioxide and water that are coming out of those smokestacks that has to be stopped, and it is interesting that later today--we have a mechanism to stop the carbon dioxide from coming out of those stacks and later today we are having a hearing in the Energy and Air Quality Subcommittee of this same Energy and Commerce Committee on the Yucca Mountain nuclear waste repository. One of the reasons why Yucca Mountain is so important is because of the increasing importance of nuclear power in our national fuel mix as an emissions-free, carbon-free-emissions source of power.

In fact, I would submit that along with the passage of the Clean Air Act in the past few decades, perhaps one of the greatest missed opportunities--if the Clean Air Act was an enacted opportunity, one of the great missed opportunities was abandonment of nuclear power in the late 1970s and allowing other countries to get ahead of us in that regard so now that our dependence on foreign oil--and we knew in the 1974 embargo that dependence on foreign oil was not a good foreign policy strategy and yet for whatever reason we have lagged with development of nuclear fuel, so I am grateful we are having that hearing later on today.

It is false to presume that a consensus exists today or that human activity has been proven to cause global warming, and that is the crux of this hearing. What we are here today to discuss is the broader issue of the use of sound statistical analysis and the peer review process through the lens of the hockey stick temperature studies, but the focus of our hearing today is to examine the statistical analysis and methodology used when evaluating the influential report on global warming written by Dr. Mann. As the U.S. Congress and even the international policymaking bodies look to the scientific community to provide information and analysis, it is especially important to make certain that the processes are in place to ensure that we are using sound and unbiased science that has undergone rigorous peer review process.

I would point out that simply turning off the electrical generation plants that provide the air conditioning back in my district would not be a viable option. I would submit that the good people of California got upset when some people in Texas turned off their electrical generation plants a few years ago. I don’t see that as a viable option. Should we move to other methods? Perhaps, but we need to do so in a sound and scientific manner.

With that, Mr. Chairman, I will yield back.

MR. WHITFIELD. At this time I recognize Ms. Baldwin of--okay, Ms. Schakowsky.

MR. STUPAK. Mr. Chairman, before we do that, if I may, I would like to put into the record a letter from Georgetown University Law Center Institute for Public Representation explaining why Dr. Mann cannot be here on such short notice from the committee and other dates he was available to testify. I would like to put that in the record, a follow-up of the statements that he is on vacation, which is not true.

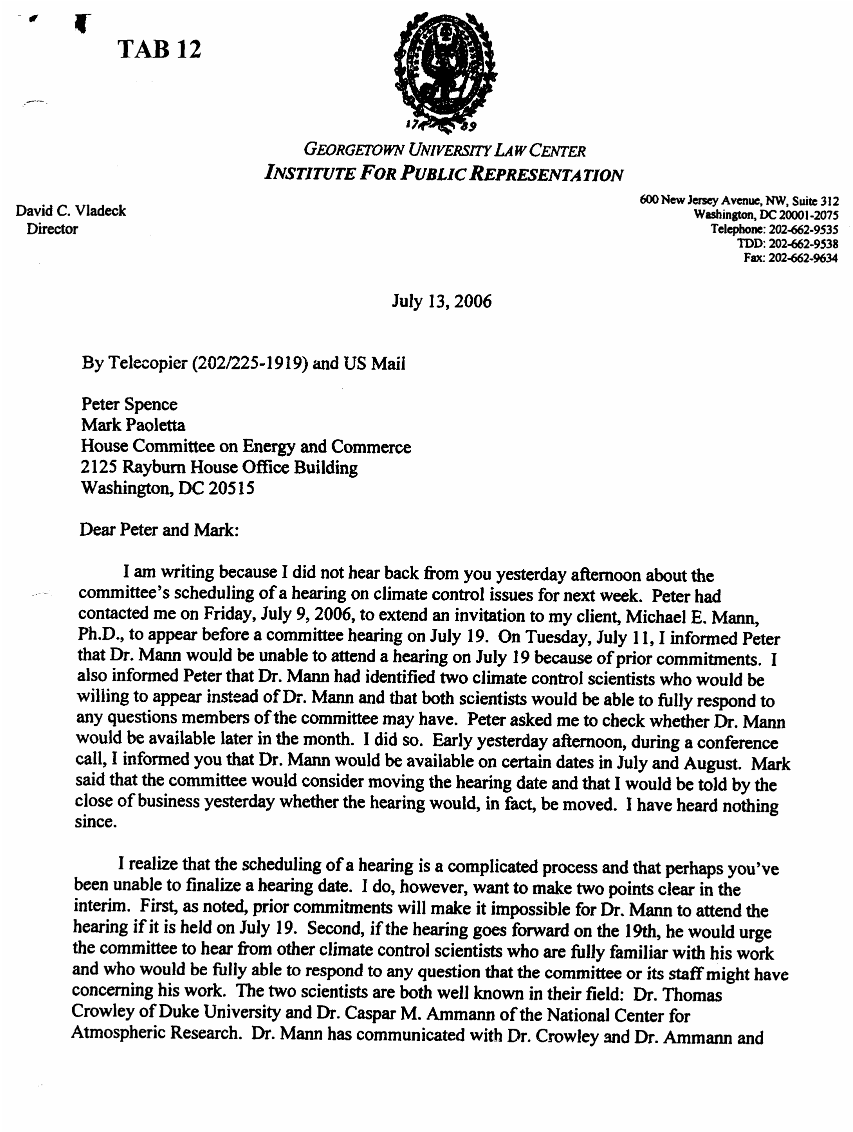
MR. WHITFIELD. We would be happy to do so unless--

MR. STUPAK. Thank you.

MR. WHITFIELD. --there is objection. Is there objection to this going in? Thank you.

MR. STUPAK. This letter of July 19 was provided actually by fax to Mr. Spencer and Mr. Paoletta.

[The information follows:]



TAB 12

GEORGETOWN UNIVERSITY LAW CENTER
INSTITUTE FOR PUBLIC REPRESENTATION

David C. Vladeck
Director

600 New Jersey Avenue, NW, Suite 312
Washington, DC 20001-2075
Telephone: 202-662-9535
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Fax: 202-662-9634

July 13, 2006

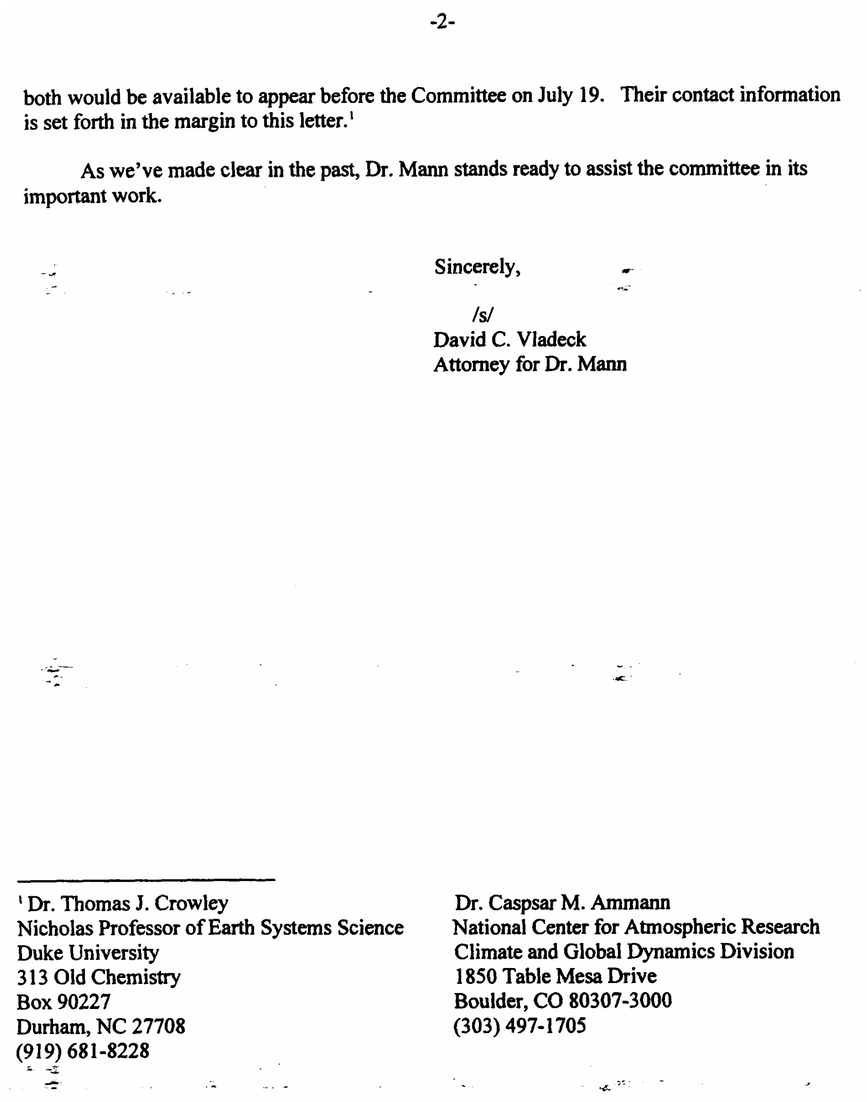
By Telecopier (202/225-1919) and US Mail

Peter Spence
Mark Paoletta
House Committee on Energy and Commerce
2125 Rayburn House Office Building
Washington, DC 20515

Dear Peter and Mark:

I am writing because I did not hear back from you yesterday afternoon about the committee's scheduling of a hearing on climate control issues for next week. Peter had contacted me on Friday, July 9, 2006, to extend an invitation to my client, Michael E. Mann, Ph.D., to appear before a committee hearing on July 19. On Tuesday, July 11, I informed Peter that Dr. Mann would be unable to attend a hearing on July 19 because of prior commitments. I also informed Peter that Dr. Mann had identified two climate control scientists who would be willing to appear instead of Dr. Mann and that both scientists would be able to fully respond to any questions members of the committee may have. Peter asked me to check whether Dr. Mann would be available later in the month. I did so. Early yesterday afternoon, during a conference call, I informed you that Dr. Mann would be available on certain dates in July and August. Mark said that the committee would consider moving the hearing date and that I would be told by the close of business yesterday whether the hearing would, in fact, be moved. I have heard nothing since.

I realize that the scheduling of a hearing is a complicated process and that perhaps you've been unable to finalize a hearing date. I do, however, want to make two points clear in the interim. First, as noted, prior commitments will make it impossible for Dr. Mann to attend the hearing if it is held on July 19. Second, if the hearing goes forward on the 19th, he would urge the committee to hear from other climate control scientists who are fully familiar with his work and who would be fully able to respond to any question that the committee or its staff might have concerning his work. The two scientists are both well known in their field: Dr. Thomas Crowley of Duke University and Dr. Caspar M. Ammann of the National Center for Atmospheric Research. Dr. Mann has communicated with Dr. Crowley and Dr. Ammann and



both would be available to appear before the Committee on July 19. Their contact information is set forth in the margin to this letter.[1]

As we've made clear in the past, Dr. Mann stands ready to assist the committee in its important work.

Sincerely,

/s/
David C. Vladeck
Attorney for Dr. Mann

---

[1] Dr. Thomas J. Crowley
Nicholas Professor of Earth Systems Science
Duke University
313 Old Chemistry
Box 90227
Durham, NC 27708
(919) 681-8228

Dr. Caspsar M. Ammann
National Center for Atmospheric Research
Climate and Global Dynamics Division
1850 Table Mesa Drive
Boulder, CO 80307-3000
(303) 497-1705

MR. WHITFIELD. Ms. Schakowsky.

MS. SCHAKOWSKY. Thank you, Mr. Chairman.

I am glad that we are holding a hearing on global climate change although I am disappointed in the actual substance of this, and I have a statement for the record that is prepared but I would like to just say a couple of things extemporaneously about this issue which I care so much about.

I guess I would ask about this particular hearing in some ways is, what is the point? I think that there are certain agreements that all of the scientific community would adhere to, and one is that climate change, the question being how much does human activity contribute to that, but the climate change is definitely happening, that the Earth is warming right now and there is a large and robust body of science that documents that, that even in the Middle Ages it could have been as warm as now although that is not clear at all, that the temperature is going up and that climate change impacts are being observed now and are projected to be of enormous consequence, enormous consequence. If the snow in the Himalayas melts, which provides water for I think close to a billion people, this is of great concern.

As a grandmother, I am concerned that my grandchildren may never see or know about a polar bear in the wild and that the coral reefs are disappearing. The fact that we are seeing stronger hurricanes and tornadoes and that there is drought and flood and hunger and displacement as a consequence, these are things that we know about, and so the question is, even if human activity is not the principal cause of global warming, which most scientists do believe that is the case, but even if it weren't but we are simply contributing to it, why wouldn't we be focusing on now how human activity could reduce the impact of global warming, how we could help to stem the tide of these devastating consequences that will hurt all of humanity. Why wouldn't we be focusing on that instead of trying to discredit a report that is only one piece of the evidence that establishes that we are in the midst of a tremendous change that is going to impact the possibility of life as we know it on this planet.

We don't have to be talking about the kinds of devastating changes in lifestyle that Americans won't accept. Instead, because of our ingenuity, always being on the cutting edge of technology and change, we can manage the changes that are needed in order to sustain life on this planet. It just makes no sense to me--I mean, we will talk about it and we will get into it how the Mann statistics that are going to be discredited actually weren't used in his final report and we can go into all the details back and forth about the scientific evidence but it seems to me that this is a waste of time, that what we ought to be talking about is how are we going to confront what everyone knows is a real problem, and if human activity can be changed in some way to ameliorate that problem, for the life of me I can't understand why all of us together in a bipartisan way wouldn't want to do that.

I have a young person in my district who really is absolutely obsessed with the issue of global warming. He is a junior high school student. His mother is worried about him because he worries about it so much. To me, the answer isn't explaining to him oh, be happy, don't worry, this isn't really an issue, there is nothing you can do about it. The answer is, we need to tell young people, the next generation, my grandchildren, that there are things that we can do today, and so I plead with my colleagues on both sides of the aisle, let us get down to solutions, not discrediting one tiny piece of the mass of evidence that says that we are in trouble right now and that literally billions of people, all the people are on our plant, will suffer if we don't get down to the business of finding a solution, so I thank you, Mr. Chairman, for the time.

MR. WHITFIELD. Thank you. Mr. Stearns, you are recognized for 5 minutes.

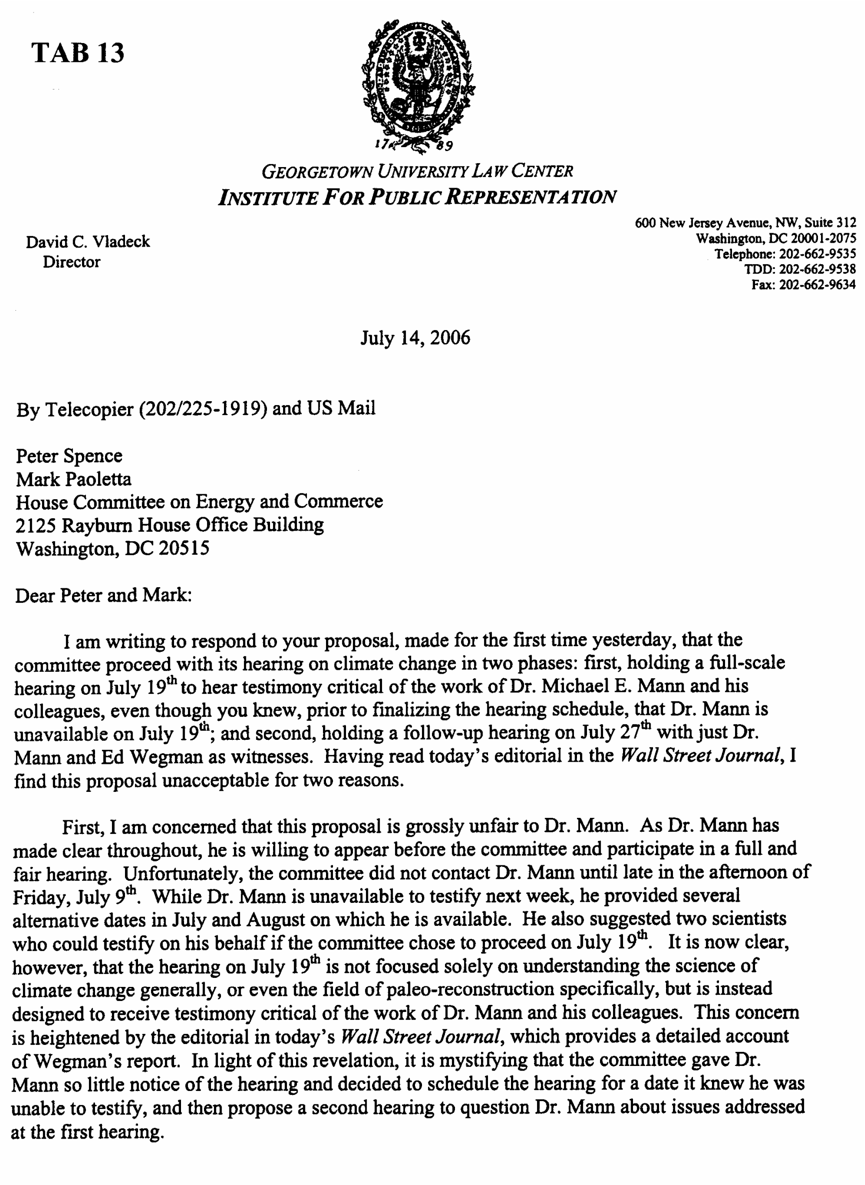
MR. STEARNS. Thank you, Mr. Chairman. I think Mr. Stupak put in the letter of July 14 from Mr. Mann's lawyer. I would like unanimous consent to put in the letter of July 13 that preceded that, which if without objection I would like that--

MR. STUPAK. Well, I guess I will have to object until we see it. Can we at least see it?

MR. STEARNS. Oh, sure, sure. Yes. You put a letter that came after the first letter and I thought it would be appropriate if we include that letter too since that is a day earlier in which he said he could not make our committee and for whatever reason he couldn't make it and in fact he suggested that if we do have this hearing, that we should have Dr. Thomas J. Crowley, and indeed we took his advice and we got Dr. Crowley. He is going to be on the second panel, so we took Dr. Mann's advice, we got the people he wanted, and I am sure, Mr. Chairman, other people had to cut their vacation short to be here, perhaps even Dr. North did. This is a time when a lot of us are taking vacations, not necessarily Members of Congress who are into a campaign mode but the rest of you perhaps are doing that, and I can understand that, but the letter Mr. Stupak put in said that he would not even show up on the 27th. The letter I am putting in says he won't show up today. Unfortunately, his lawyer from the Georgetown University Law Center keeps talking about July--I think in his letter--I don't have it in front of me but he has a typographical error in both letters in which he cites Friday, July 9. In all calendars, July 9 is not a Friday.

MR. WHITFIELD. They are not objecting to the letter.

[The information follows:]



TAB 13

GEORGETOWN UNIVERSITY LAW CENTER
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July 14, 2006

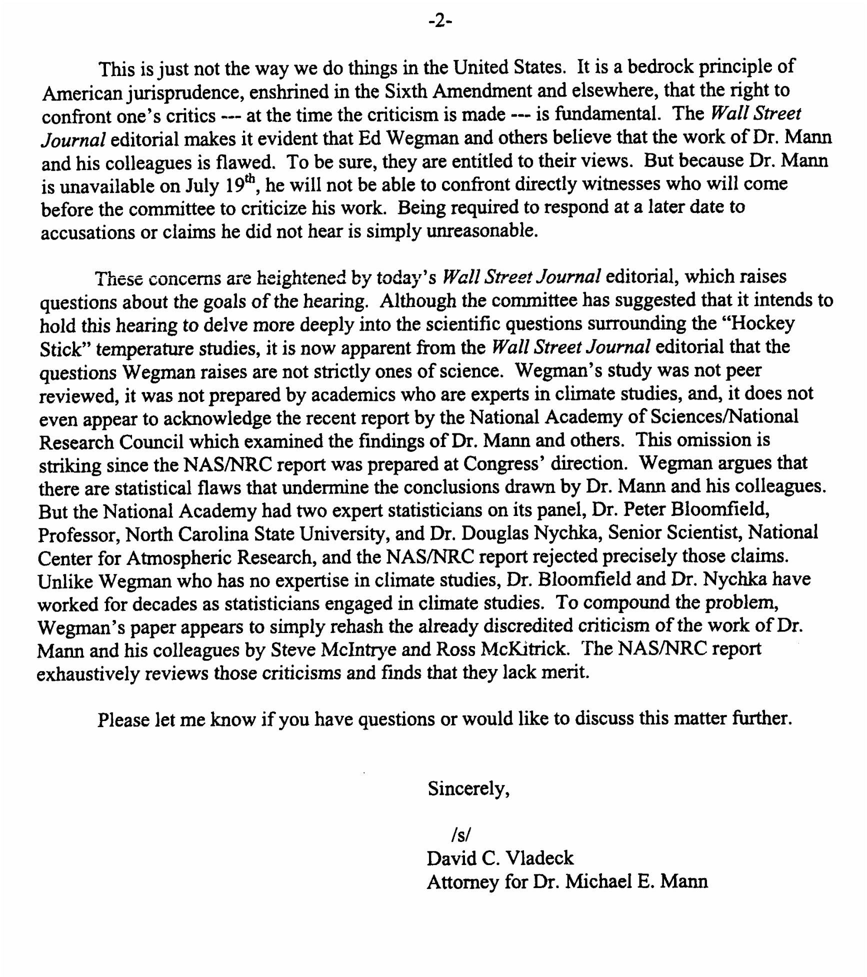
By Telecopier (202/225-1919) and US Mail

Peter Spence
Mark Paoletta
House Committee on Energy and Commerce
2125 Rayburn House Office Building
Washington, DC 20515

Dear Peter and Mark:

I am writing to respond to your proposal, made for the first time yesterday, that the committee proceed with its hearing on climate change in two phases: first, holding a full-scale hearing on July 19th to hear testimony critical of the work of Dr. Michael E. Mann and his colleagues, even though you knew, prior to finalizing the hearing schedule, that Dr. Mann is unavailable on July 19th; and second, holding a follow-up hearing on July 27th with just Dr. Mann and Ed Wegman as witnesses. Having read today's editorial in the *Wall Street Journal*, I find this proposal unacceptable for two reasons.

First, I am concerned that this proposal is grossly unfair to Dr. Mann. As Dr. Mann has made clear throughout, he is willing to appear before the committee and participate in a full and fair hearing. Unfortunately, the committee did not contact Dr. Mann until late in the afternoon of Friday, July 9th. While Dr. Mann is unavailable to testify next week, he provided several alternative dates in July and August on which he is available. He also suggested two scientists who could testify on his behalf if the committee chose to proceed on July 19th. It is now clear, however, that the hearing on July 19th is not focused solely on understanding the science of climate change generally, or even the field of paleo-reconstruction specifically, but is instead designed to receive testimony critical of the work of Dr. Mann and his colleagues. This concern is heightened by the editorial in today's *Wall Street Journal*, which provides a detailed account of Wegman's report. In light of this revelation, it is mystifying that the committee gave Dr. Mann so little notice of the hearing and decided to schedule the hearing for a date it knew he was unable to testify, and then propose a second hearing to question Dr. Mann about issues addressed at the first hearing.



This is just not the way we do things in the United States. It is a bedrock principle of American jurisprudence, enshrined in the Sixth Amendment and elsewhere, that the right to confront one's critics --- at the time the criticism is made --- is fundamental. The *Wall Street Journal* editorial makes it evident that Ed Wegman and others believe that the work of Dr. Mann and his colleagues is flawed. To be sure, they are entitled to their views. But because Dr. Mann is unavailable on July 19th, he will not be able to confront directly witnesses who will come before the committee to criticize his work. Being required to respond at a later date to accusations or claims he did not hear is simply unreasonable.

These concerns are heightened by today's *Wall Street Journal* editorial, which raises questions about the goals of the hearing. Although the committee has suggested that it intends to hold this hearing to delve more deeply into the scientific questions surrounding the "Hockey Stick" temperature studies, it is now apparent from the *Wall Street Journal* editorial that the questions Wegman raises are not strictly ones of science. Wegman's study was not peer reviewed, it was not prepared by academics who are experts in climate studies, and, it does not even appear to acknowledge the recent report by the National Academy of Sciences/National Research Council which examined the findings of Dr. Mann and others. This omission is striking since the NAS/NRC report was prepared at Congress' direction. Wegman argues that there are statistical flaws that undermine the conclusions drawn by Dr. Mann and his colleagues. But the National Academy had two expert statisticians on its panel, Dr. Peter Bloomfield, Professor, North Carolina State University, and Dr. Douglas Nychka, Senior Scientist, National Center for Atmospheric Research, and the NAS/NRC report rejected precisely those claims. Unlike Wegman who has no expertise in climate studies, Dr. Bloomfield and Dr. Nychka have worked for decades as statisticians engaged in climate studies. To compound the problem, Wegman's paper appears to simply rehash the already discredited criticism of the work of Dr. Mann and his colleagues by Steve McIntrye and Ross McKitrick. The NAS/NRC report exhaustively reviews those criticisms and finds that they lack merit.

Please let me know if you have questions or would like to discuss this matter further.

Sincerely,

/s/
David C. Vladeck
Attorney for Dr. Michael E. Mann

MR. STEARNS. Okay. Good. All right. Well, I was just talking to make sure Mr. Stupak had plenty of time to read it so that I could go forward.

You know, I think almost everybody in this room and perhaps everybody on this oversight committee would agree that there is global warming of some kind. The question is, is it sinusoidal, that is, are we looking at warming today in which there was warming like this or similar to this in the Middle Ages and have we seen a warming and a cooling much like a sinusoidal wave, and so we are trying to look at Dr. Mann's analysis and we are trying to say, is he absolutely right that we have this hockey stick effect that is just flat and then suddenly comes up.

Now, we have Dr. Wegman's analysis concludes that Dr. Mann's work cannot support the claim that the 1990s were the warmest decade in the millennium. I mean, that is what he is saying. Some people are questioning Dr. Mann, his quantitative analysis, and that is fine. He could be right, he could be wrong. Now, Dr. North, in looking through his testimony which he is going to give, he sort of confirms what I think is possible, that this warming and cooling is a sinusoidal wave and that in fact, let me just read what Dr. North says in his testimony. He says that it is plausible that the Northern Hemisphere was warmer during the last few decades of the 20th Century than during any comparable period over the preceding millennium. That is what he says. However, the substantial uncertainties currently present in the quantitative assessment, same thing that Dr. Wegman says, of large-scale surface temperature changes prior to about 1600 A.D. lower our confidence in this conclusion compared to the high level of confidence we place in the Little Ice Age cooling and 20th Century warming. So we have two distinguished individuals who are professionals in their fields indicating that it is not absolutely true that Dr. Mann is correct in his analysis and Dr. North went on to say even less confidence can be placed in the original conclusions by Dr. Mann.

So, I mean, for anybody on the other side to say this not a legitimate hearing is incorrect. We have taken people that Dr. Mann wanted and we put them on here as witnesses. We have asked Dr. Mann to come to this hearing. We have asked him to come to the 27th. He won't come. He has hired a lawyer to spar with our people to say why he won't come. By golly, if he really is interested in solving this problem, I would cut my vacation short and whatever he is doing to say I will be here because I think in the interest of science, I would like to have an open hearing and talk about it. So I think, one, it is a legitimate hearing. Two, we have offered Dr. Mann two opportunities and yet his lawyer has indicated he won't show up. So this is a very important issue but I think overall, all of us here are trying to understand this and we would agree that there is probably global warming. What we want to know is, is this sinusoidal or is this something that is aberrational.

Let me conclude by saying that yes, we should have further inquiries into this matter. Perhaps as a result of this hearing we will. Temperature studies and the effect of climate change, all these are very important to our very existence. So Mr. Chairman, I commend what you are doing and I commend the other side too to keep an open mind here and to hear Dr. Wegman and to hear Dr. North and to read their opening statements where you will see they have less confidence and they certainly have as much credibility on this matter as Dr. Mann, and I am just so sorry, so sorry that Dr. Mann is not showing up today, he is not showing up on the 27th, and at this point I am not clear, Mr. Chairman, when you will get him. Thank you.

MR. WHITFIELD. Ms. Baldwin, you are recognized for 5 minutes.

MR. STUPAK. Mr. Chairman, may I ask unanimous consent that the letter that we received from Dr. Mann's lawyer indicating he would like to come at the same time these witnesses are here be entered into the record.

MR. WHITFIELD. It has been.

MR. STUPAK. Oh, it has been? Oh, okay. Thank you.

MR. WHITFIELD. We have had two letters introduced into the record from his lawyer, both.

MS. BALDWIN. Thank you, Mr. Chairman. It doesn't take much more than a quick walk outside today to know that the thermometer has reached dangerously high levels and government heat alerts are abounding these days but this summer is not unique. Each year summers are growing warmer and warmer and so are the winters, falls, and springs. Of the 20 hottest years on record, 19 have occurred in the 1980s or later. 2005 was one of the hottest years on records and so far 2006 has set record levels for its high temperatures.

Unfortunately, despite overwhelming scientific evidence that our planet is warming at dramatic rates, no political consensus for bold action has followed and that is the problem. Politicians ignore sound science showing evidence that the Earth is warming at an unprecedented rate, that carbon dioxide levels are rising, and that human activities are largely the cause. But beyond ignoring sound science, they are doing other disturbing things. I see political interference in science these days. In fact, time after time, sound science has been censored in order to maintain a political agenda. Here are just a few examples.

In 2003, the EPA was ordered by the White House to delete critical sections relating to climate change from its report on the environment. In 2005, the White House insisted upon weakening language relating to the impact of global climate change in a document that served as the basis of negotiations during the G8 Summit, and just a few months ago the Administration tried to silence a NASA scientist from talking about the need to reduce greenhouse gases linked to global warming. I could point to many other examples, some on this topic, some outside, but it is a disturbing trend indeed.

With all these examples, it only becomes more clear that false logic will not bring us closer to an understanding of the scientific truth. The truth is alarming. Sea levels are rising. Glaciers are melting and storms are becoming more intense, and the result is the near extinction of animals such as polar bears, the compromising of coastal ecosystems, and the threatening of human life as heat waves become prevalent and disease-carrying insects grow more abundant.

Mr. Chairman, I often speak about America's need to take bold action and the importance of us leading the world on environmental issues. Now is the time for us to show our commitment for if we do nothing, we risk an uncertain and unstable future. So I ask, what are the consequences if the cynics and naysayers and keepers of the status quo are wrong? We have a moral and an ethical obligation to act and I just hope that today we will take some steps in that right direction.

Thank you, Mr. Chairman. I yield back the balance of my time.

MR. WHITFIELD. Thank you, Ms. Baldwin. At this time I recognize Mrs. Blackburn.

MRS. BLACKBURN. Thank you, Mr. Chairman. Thank you for holding the hearing. Thank you to the staff for the preparation work they have done and to our witnesses appearing before us to comment on the matter. We thank you for being here. We are concerned about it. I do think it is prudent to address the issue and we are seeking information. We thank you for being here to supply some.

The ability to obtain and analyze the data and the methods that a scientist uses to form a theory about the universe is central to science. For hundreds of years society has placed the utmost importance on the scientific method to validate theories which is predicated on the ability to replicate and verify a scientist's work. If the work cannot be replicated and verified by independent experts, then that work's conclusions become more speculation and possibly some will say it should be open to classification as outright scientific dishonesty.

Last year Chairman Barton inquired into the background of some recent climate change studies that had been held by scientific portions of the scientific community as proof of drastic global warming. Now, I am old enough to remember that as a teen in the late 1960s, I sat in science classes and in a geology class and I was warned of a returning and impending ice age. By the time I reached my current age, the world was going to be covered in ice, North America would have a 9-month winter, our food supplies would be short, and I would be freezing to death all the time. Well, I guess times changed or maybe that old group of scientists had some kind of political interference in favor of the new group of scientists who now want the Earth to warm up.

Now, after some independent analysis it seems that all scientists could possibly be misled on some of their issues. Both the National Academy of Sciences and Dr. Wegman's committee analyzed the hockey stick report by Dr. Mann that has become the poster child for proof of global warming. The committees came to the conclusion that Dr. Mann's hockey stick report failed verification tests and did not employ proper statistical methods. Also, it appears that Dr. Mann is part of a social network or could be part of a social network of climate scientists who almost always use the same data sets and review each other's works. There is a contention that they would dismiss critics who had legitimate concerns, rarely used statistical experts for the data they used in their reports, and make it very difficult for reviewers to obtain background data and analysis. These revelations point to the lack of independent peer review and how it is practically impossible to replicate or verify Dr. Mann's work by those not affiliated with the network of scientists, so we are looking forward to hearing about that work today. Could it be that this particular work violates the principles of the scientific method and should be dismissed until it meets the basic qualifications? Could that have been some of what happened to the Ice Age return theory of the 1960s?

Climate is affected by numerous causes that interact with each other. When a scientific paper comes to a conclusion about climate, its results must be able to be replicated and shown to have direct causation and not merely correlation. If these steps cannot be done, then making conclusive statements of how one cause changes the climate is unwarranted and not real science.

Now, there is strong evidence that the Earth has warmed about half a degree Fahrenheit from 1900 to 1940 but this is widely attributed to an increase in solar activity during those years and there are indications that the Earth warmed another half degree Fahrenheit from 1940 until the present but that much of this warming occurred in the past 7 years, and if you look at the surface record in the satellite data, it is pretty clear and possible that this warming is mostly due to the 1998 El Nino, so for the past hundred years the Earth has warmed about one degree and you can make the cause that it was not caused by human activity but by natural events. Possibly that is what happened to the return of that old Ice Age.

Mr. Chairman, if one looks at the data in an objective manner, I believe that one would conclude that the Earth's climate is not in serious danger or not standing at the edge of a precipice. Maybe our focus should be first on getting the information. Maybe our focus should not be on environmentalism. Maybe the focus should be on common-sense conservatism. I would challenge my colleagues on the other side to approach this issue to learn the truth about the Earth's climate, not to form an agenda.

I am looking forward to our witnesses in the hearing today. I yield back.

MR. WHITFIELD. Thank you, Mrs. Blackburn. I think that concludes the opening statements so we will proceed to the first panel of witnesses, and I would say to you, Dr. North and Dr. Wegman, that this committee is holding an investigative hearing, and when doing so we do have the practice of taking testimony under oath, and I would ask you, do either of you have any objection to testifying under oath?

Now, Dr. Wegman, accompanying you today is one of the statisticians that worked with your three-person panel, and would you introduce her? Although it is my understanding she is not going to be testifying but she is from Johns Hopkins, I believe.

DR. WEGMAN. That is correct. It is Dr. Yasmin Said. Dr. Said actually did a tour at Johns Hopkins but has just won a very prestigious National Institutes of Health postdoctoral fellowship and she will be with us in George Mason for the next 3 years.

MR. WHITFIELD. And although she is not going to testify, you may consult with her. Dr. Wegman, if you and Dr. North would stand up, I would like to swear you in. Of course, under the rules of the House and the rules of the committee, you are also entitled to legal counsel and I am assuming you don't need legal counsel today, but if you do--

DR. WEGMAN. Hopefully not.

MR. WHITFIELD. If you would raise your right hand.

[Witnesses sworn.]

MR. WHITFIELD. Thank you, very much. You are now both under oath, and Dr. Wegman, you are recognized for your opening statement, and I would say to both of you, I know both of you have rather lengthy documents that we appreciate your preparing and those will be entered into the record in their entirety, and if you all could keep your statements to 5 to 7 minutes or so, we would appreciate that. Dr. Wegman, you are recognized.

STATEMENTS OF DR. EDWARD J. WEGMAN, CENTER FOR COMPUTATIONAL STATISTICS, GEORGE MASON UNIVERSITY; AND DR. GERALD R. NORTH, DEPARTMENT OF ATMOSPHERIC SCIENCES, TEXAS A&M UNIVERSITY

DR. WEGMAN. Thank you, sir. I would like to begin by circumscribing the substance of our report. We were asked to provide independent verification by statisticians of the critiques of the statistical methodology found in the papers of Drs. Michael Mann, Raymond Bradley, and Malcolm Hughes published respectively in *Nature* in 1998 and in *Geophysical Research Letters* in 1999. These two papers have commonly been referred to as MBH98 and MBH99. The critiques have been made by Stephen McIntyre and Ross McKitrick published in *Energy and Environment* in 2003 and again in *Energy and Environment* and in *Geophysical Research Letters* in 2005. We refer to these are MM02, 05a and 05b, respectively.

We were also asked about the implications of our assessment. We were not asked to assess the reality of global warming and indeed this is not an area of our expertise. We do not assume any position with respect to global warming except to note in our report that the instrumented record of global average temperature has risen since 1850 according to the MBH99 chart by about 1.2 degrees Centigrade, and in the NAS panel report chaired by Dr. North, about six-tenths of a degree Centigrade in several places in that report.

Our panel is composed of myself, Edward Wegman at George Mason University, David W. Scott at Rice University, and as mentioned, Yasmin Said at the Johns Hopkins University. This ad hoc panel has worked on a pro bono basis. We have received no compensation, not even taxi fare, and no financial interest and we have no financial interest in this.

Can we see slide one, please? In figure 1, we have a document, a chart that came out of Dr. Bradley's book on paleoclimatology, and sort of indicates the kind of things that are used as proxy data in paleoclimatology. One thing I would like to point out in particular that is important I think for understanding this area is the things that are indicated--if you look--

MR. WHITFIELD. Dr. Wegman, we need for you to use your mic. I know it is going to be difficult but we could not hear you when you turned around there.

DR. WEGMAN. I will refrain from doing that. The point of this graphic is that there are many factors that affect all of the proxies that are used in paleoclimate temperature reconstruction, and without carefully teasing out those effects, the tree rings, the ice cores, and so on, are not by, in and of themselves totally temperature records.

So MBH98 and 99 use several proxy indicators to measure global climate change. Primarily these include historical records, tree rings, ice cores, and coral reefs. More details of the proxies are given in our report and mentioned in the written testimony.

Could we go to figure 2, please? Some examples of tree ring proxy series are given in figure 2. Most of the proxy series for these tree rings show little structure but the last two show the characteristic hockey stick shape. The principal component-like methodology in MBH98 and 99 preferentially emphasizes these shapes as we shall see. Principal component analysis methodology is at the core of the MBH98 and 99 analysis methodology. Principal component analysis is a statistical methodology often used for reducing data sets with many variables into data sets with fewer but composite variables. The time series proxy data involved are transformed into their principal components where the first principal component is intended to explain most of the variation present in the data variables. Each of the subsequent principal components explains less and less of the variation. In the methodology of MBH98 and 99, the first principal component is used in the temperature reconstruction.

Could we have figure 3, please? The two principal methods for temperature reconstructions have been used. CFR, climate field construction is used in MBH98/99 although that terminology was not used formally until 2005, I believe, and the other is CPS, climate-plus-scale methodology. The CFR is essentially the principal component-based analysis and the CPS is a simple averaging of climate proxies. The controversy of the MBH98/99 method lies in that the proxies are incorrectly centered on the mean period of 1902 to 1995, rather than on the whole time period. The proxy data exhibiting the hockey stick are actually decentered low. The updated MBH98/99 reconstruction is given in figure 3. This fact that the proxies are centered low is apparent in figure 3 because for most of the thousand years the reconstruction is below zero. This is temperature anomaly. Because the hockey stick proxies are centered too low, they will exhibit a large effective variance, allowing the method to exhibit a preference for selecting them as the first principal component. The net effect of decentering the proxy data in MBH98 and MBH99 is to produce the hockey stick shape. Centering on the overall mean is a critical factor in using principal component methodology properly.

Could we have figure 4, please? To illustrate this, we consider the North American tree series and apply the MBH98 methodology. The top panel shows the result from decentering. The bottom panel shows the result when the principal components are properly centered. The centering does make a significant difference in the reconstruction, and as you see, while the top panel illustrates the temperature rise or purported temperature rise in the last 100 years or so, the bottom panel when properly centered does not have this temperature rise.

Could we go to figure 5? To further illustrate this, we digitized the temperature profile published in the IPCC 1990 report and we did apply both the CFR and the CPS methods to them. The data used here are 69 unstructured noise pseudo-proxy series with only one copy of the 1990 profile. The upper left panel illustrates the PC1 with proper centering. In other words, no structure is shown. The other three panels indicate what happens when using principal components with an increasing amount of decentering. Again, the single series begins to overwhelm the 69 other pure noise series. Cleary, this decentering has a big effect.

It is not clear that Dr. Mann and his associates realized the error in their methodology at the time of publication but our re-creation supports the critique of the MBH99 methods.

As commentary, in general we found the writing in MBH98 and 99 to be somewhat obscure and incomplete and the criticisms by MM03/05a and 05b to be valid. The reasons for setting 1902-1995 as the calibration period presented in the narrative of MBH98 sounds plausible on the surface and the error may be easily overlooked by someone not trained in statistical methodology. We note that there is no evidence that Dr. Mann or any of the other authors in the paleoclimate studies have significant interactions with mainstream statisticians.

Because of this apparent isolation, we decided to attempt to understand the paleoclimate community by exploring the social network of authorships in the temperature reconstruction area. We found that at least 43 authors have direct ties to Dr. Mann--and this should be figure 6, please; thank you--have direct ties to Dr. Mann by virtue of coauthored papers with him. Our findings from this analysis suggest that authors in this area of the relatively narrow field of paleoclimate studies are closely connected. Dr. Mann has an unusually large reach in terms of influence. He is the coauthor with every one of these people which are indicated by the black edge borders on the top and the side of this graph. In particular, he has a close connection with Drs. Jones, Bradley, Hughes, Briffa, Rutherford, and Osborne and those are indicated by the solid block on the upper left-hand corner.

This area of social networks is based off a graph theoretic representation, and if we go to figure 7, we can see the graph theoretic representation. Because of these close connections, independent studies may not be as independent as they appear to be on the surface. Although we have no direct data on the functioning of peer review within the paleoclimate community but, with me having 35 years of experience with peer review in both journals as well as evaluation of research proposals, peer review may not have been as independent as would generally be desirable.

Could we have figure 8, please? Figure 8 is a graphic that depicts a number of papers in the paleoclimate reconstruction area together with some of the proxies used. We note that many of the proxies are shared. Some of the same data also suggests a lack of independence.

The MBH98/99 work has been sufficiently politicized that this committee can hardly reassess their public positions without losing credibility. Overall, our community believes that the MBH98/99 assessment that the decade of the 1990s was likely the hottest decade in the millennium and that 1998 was likely the hottest year in the millennium cannot be supported by their analysis because of the mathematical flaws.

We have some recommendations which flowed out of our analysis. Recommendation one: Especially when massive amounts of public monies and human lives are at stake, academic work should have a more intense level of scrutiny and review. It is especially the case that authors of policy-related documents like the IPCC report should not be the same people as those that constructed the academic papers.

We believe that federally funded research agencies should develop a more comprehensive and concise policy on disclosure. All of us writing this report have been federally funded. Our experience with Federal funding agencies has been that they do not generally articulate clear guidelines to the investigators as to what must be disclosed. Federally funded work, including code, should be made available to other researchers upon reasonable request, especially if the intellectual property has no commercial value. Some consideration should be granted to the data collectors to have exclusive use of their data for 1 or 2 years prior to publication but data collected under Federal support should be made publicly available.

Recommendation three: With clinical trials for drugs and devices to be approved for human use by the FDA, review and consultation with statisticians is expected. Indeed, it is standard practice to include statisticians in the application for approval process. We judge this to be a good policy when public health and also when substantial amounts of monies are involved--for example, when there are major policy decisions to be made based on statistical assessments. In such cases, evaluation by statisticians should be standard practice. The evaluation phase should be a mandatory part of all grant applications and funded accordingly.

Finally, recommendation four; emphasis should be placed on the Federal funding of research related to a fundamental understanding of the mechanisms of climate change. Funding should focus on interdisciplinary teams and avoid narrowly focused disciple research. That is a general comment and by interdisciplinary teams, I mean including teams that involve what I like to call the enabling sciences such as mathematics, computer science, and statistics. Thank you, sir.

[The prepared statement of Dr. Edward J. Wegman follows:]

PREPARED STATEMENT OF DR. EDWARD J. WEGMAN, CENTER FOR COMPUTATIONAL STATISTICS, GEORGE MASON UNIVERSITY

I would like to begin by circumscribing the substance of our report. We were asked to provide an independent verification by statisticians of the critiques of the statistical methodology found in the papers of Drs. Michael Mann, Raymond Bradley and Malcolm Hughes published respectively in *Nature* in 1998 and in *Geophysical Research Letters* in 1999. These two papers have commonly been referred to as MBH98 and MBH99. The critiques have been made by Stephen McIntyre and Ross McKitrick published in *Energy and Environment* in 2003 and in *Energy and Environment* and in *Geophysical Research Letters* in 2005. We refer to these as MM03, MM05a, and MM05b respectively. We were also asked about the implications of our assessment. We were not asked to assess the reality of global warming and indeed this is not an area of our expertise. We do not assume any position with respect to global warming except to note in our report that the instrumented record of global average temperature has risen since 1850 according to the MBH 99 chart by about 1.2º centigrade. In the NAS panel Report chaired by Dr. North, .6º centigrade is mentioned in several places.

Our panel is composed of Edward J. Wegman (George Mason University), David W. Scott (Rice University), and Yasmin H. Said (The Johns Hopkins University). This Ad Hoc Panel has worked pro bono, has received no compensation, and has no financial interest in the outcome of the report.

Archive	**Minimum Sampling Interval**	**Temporal Range (order:yr)**	**Potential Information Derived**
Historical records	day/hr	$\sim 10^3$	T, P, B, V, M, L, S
Tree rings	yr/season	$\sim 10^4$	T, P, B, V, M, S, $\mathbf{C_A}$
Lake sediments	yr to 20 yr	$\sim 10^4$-10^6	T, B, M, P, V, C_W
Corals	yr	$\sim 10^4$	C_W, L, T, P
Ice cores	yr	$\sim 5 \times 10^4$	T, P, C_A, B, V, M, S
Pollen	20 yr	$\sim 10^5$	T, P, B
Speleothems	100 yr	$\sim 5 \times 10^5$	C_W, T, P
Paleosols	100 yr	$\sim 10^6$	T, P, B
Loess	100 yr	$\sim 10^6$	P, B, M
Geomorphic feat.	100 yr	$\sim 10^6$	T, P, V, L, P
Marine sediments	500 yr	$\sim 10^7$	T, C_W, B, M, L, P

Characteristics of Natural Archives

T = temperature
C = chemical composition of air or water
V = volcanic eruptions
L = sea level
P = precipitation, humidity, water balance
B = information on biomass, vegetation patterns
M = geomagnetic field variations
S = solar activity

After Bradley and Eddy (1991)

FIGURE 1

MBH98, MBH99 use several proxy indicators to measure global climate change. Primarily, these include historical records, tree rings, ice cores, and coral reefs. More details of proxies are given in the report and mentioned in the written testimony. [The width and density of tree rings vary with climatic conditions (sunlight, precipitation, temperature, humidity, and carbon dioxide and nitrogen oxides availability), soil conditions, tree species, tree age, and stored carbohydrates in the trees. The width and density of tree rings are dependent on many confounding factors, making isolation of the climatic temperature signal uncertain. It is usually the case that width and density of tree rings are monitored in conjunction in order to more accurately use them as climate proxies. Ice cores are the accumulation of snow and ice over many years that have recrystallized and have trapped air bubbles from previous time periods. The composition of these ice cores, especially the presence of hydrogen and oxygen isotopes, provides a picture of the climate at the time. The relative concentrations of the heavier isotopes in the condensate indicate the temperature of condensation, allowing for ice cores to be used in global temperature reconstruction. In addition to the isotope concentration, the air bubbles trapped in the ice cores allow for measurement of the atmospheric concentrations of trace gases, including greenhouse gases carbon dioxide, methane, and nitrous oxide.]

Sample Proxy Series

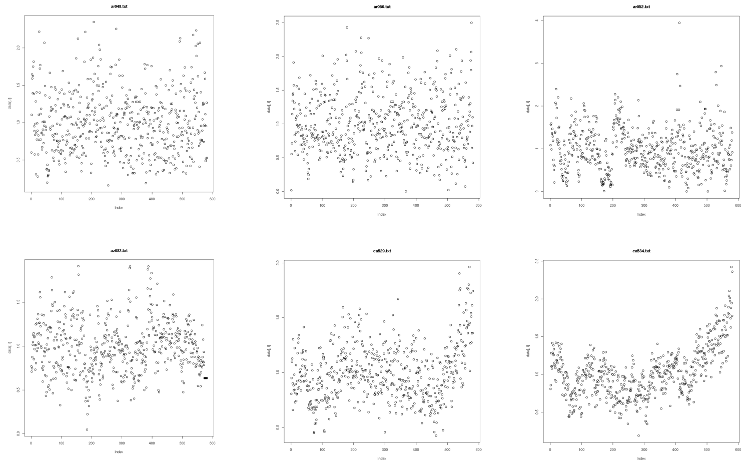
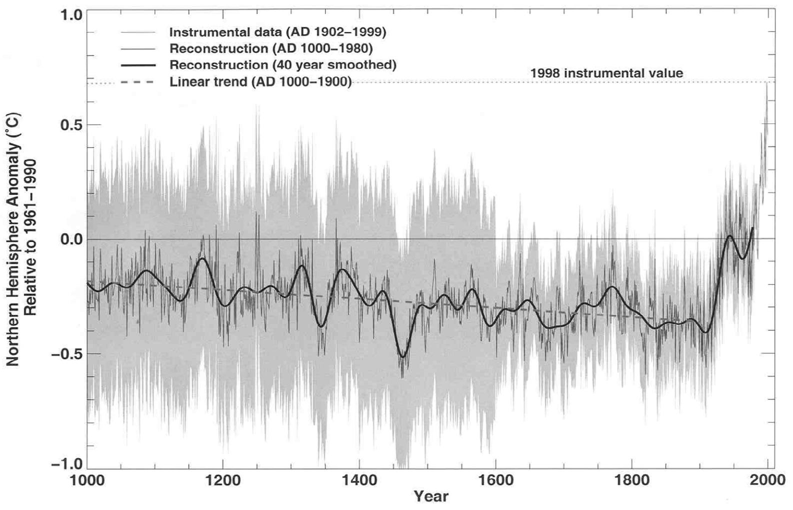


FIGURE 2

Some examples of tree ring proxy series are given in Figure 2. Most of the proxy series show little structure, but the last two show the characteristic ‘hockey stick’ shape. The principal component-like methodology in MBH 98/99 preferentially emphasizes these shapes as we shall see.

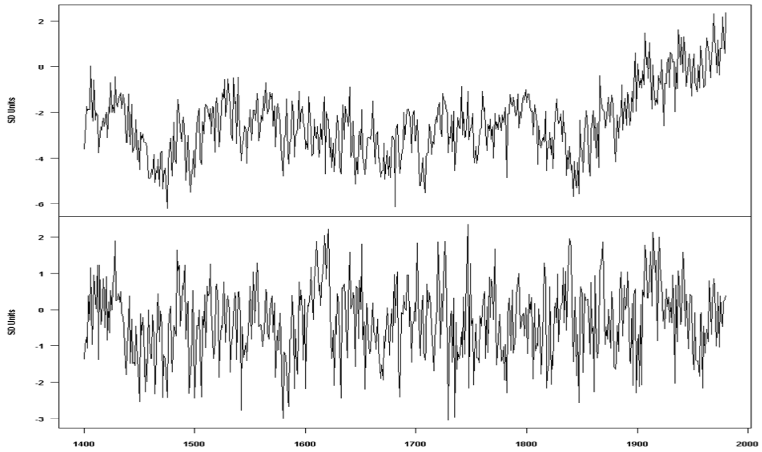
Principal component analysis methodology is at the core of the MBH98/99 analysis methodology. Principal component analysis is a statistical methodology often used for reducing datasets with many variables into datasets with fewer, but composite variables. The time series proxy data involved are transformed into their principal components, where the first principal component is intended to explain most of the variation present in the data variables. Each subsequent principal component explains less and less of the variation. In the methodology of MBH98/99, the first principal component is used in the temperature reconstruction.



Last Updated in August 2000 by Ian Macadam.

FIGURE 3

Two principal methods for temperature reconstructions have been used; CFR (climate field construction used in MBH98/99) and CPS (climate-plus-scale). The CFR is essentially the principal component based analysis and the CPS is a simple averaging of climate proxies. The controversy of the MBH98/99 methods lies in that the proxies are incorrectly centered on the mean of the period 1902-1995, rather than on the whole time period. The proxy data exhibiting the hockey stick shape are actually decentered low. The updated MBH99 reconstruction is given in Figure 3. This fact that the proxies are centered low is apparent in Figure 3 because for most of the 1000 years, the reconstruction is below zero. Because the 'hockey stick' proxies are centered too low, they will exhibit a larger effective 'variance', allowing the method to exhibit a preference for selecting them as the first principal component. The net effect of this decentering using the proxy data in MBH98 and MBH99 is to produce a 'hockey stick' shape. Centering on the overall mean is a critical factor in using the principal component methodology properly.



Top Panel is the MBH98 reconstruction
Bottom Panel is the centered PCA reconstruction

FIGURE 4

To illustrate this, we consider the North America Tree series and apply the MBH98 methodology. The top panel shows the result from the de-centering. The bottom panel shows the result when the principal components are properly centered. Thus the centering does make a significant difference to the reconstruction.

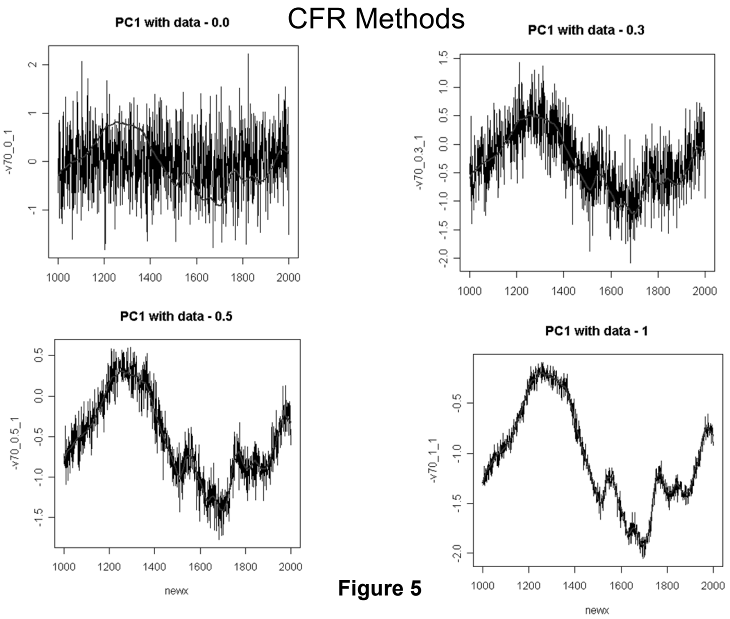


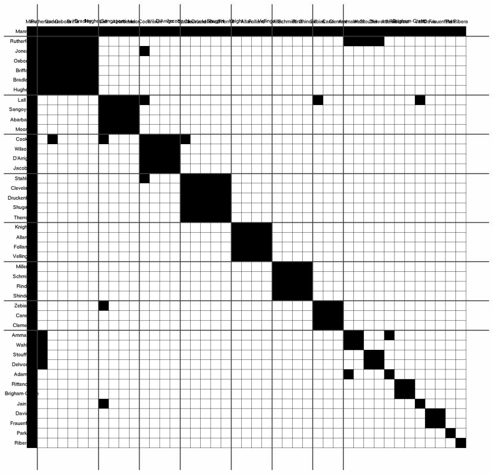
Figure 5

To further illustrate this, we digitized the temperature profile published in the IPCC 1990 report and applied both the CFR and the CPS methods to them. The data used here are 69 unstructured noise pseudo-proxy series and only one copy of the 1990 profile. The upper left panel illustrates the PC1 with proper centering. In other words, no structure is shown. The other 3 panels indicate what happens using principal components with an increasing amount of de-centering. Again, the single series begins to overwhelm the other 69 pure noise series. Clearly, these have a big effect.

It is not clear that Mann and associates realized the error in their methodology at the time of publication. Our re-creation supports the critique of the MBH98 methods.

In general, we found the writing in MBH98 and MBH99 to be somewhat obscure and incomplete and the criticisms by MM03/05a/05b to be valid. The reasons for setting 1902-1995 as the calibration period presented in the narrative of MBH98 sounds plausible, and the error may be easily overlooked by someone not trained in statistical methodology. We note that there is no evidence that Dr. Mann or any of the other authors in paleoclimate studies have had significant interactions with mainstream statisticians.

Because of this apparent isolation, we decided to attempt to understand the paleoclimate community by exploring the social network of authorships in temperature reconstruction.



Mann-Rutherford-Jones-Osborn-Briffa-Bradley-Hughes

FUGURE 6

We found that at least 43 authors have direct ties to Dr. Mann by virtue of coauthored papers with him. Our findings from this analysis suggest that authors in the area of this relatively narrow field of paleoclimate studies are closely connected. Dr. Mann has an unusually large reach in terms of influence and in particular Drs. Jones, Bradley, Hughes, Briffa, Rutherford and Osborn.

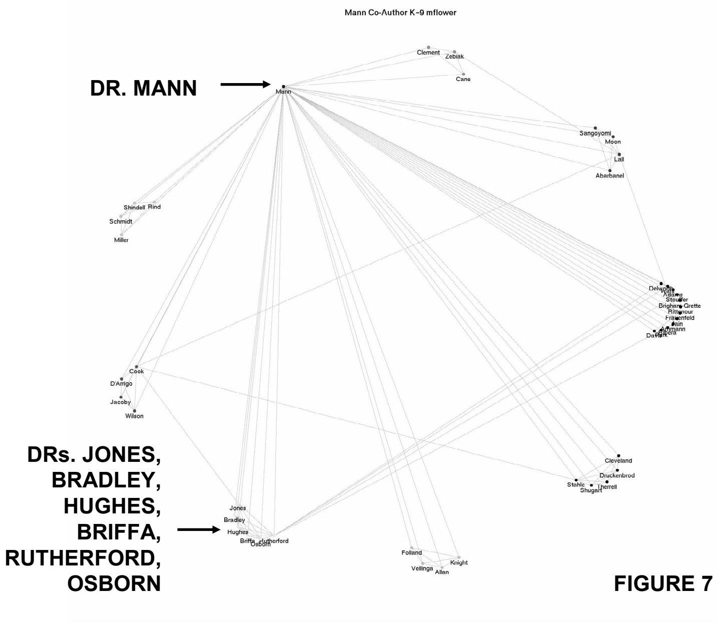
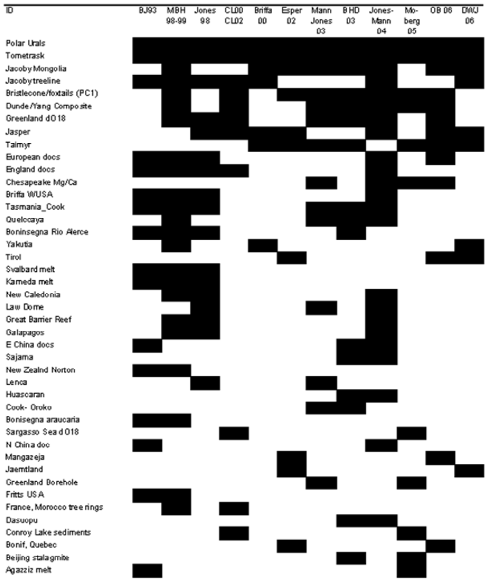


FIGURE 7

Because of these close connections, independent studies may not be as independent as they might appear on the surface. Although we have no direct data on the functioning of peer review within the paleoclimate community, but with 35 years of experience with peer review in both journals as well as evaluation of research proposals, peer review may not have been as independent as would generally be desirable.



Bradley & Jones (1993)

Mann, Bradley, Hughes (1998,1999)

Jones et al. (1998)

Crowley & Lowery (2000)

Briffa (2000)

Esper (2002)

Mann & Jones (2003)

Bradley, Hughes, Diaz (2003)

Jones & Mann (2004)

Moberg et al. (2005)

Osborn & Briffa (2006)

D'Arrigo, Wilson, Jacoby (2006)

FIGURE 8

Figure 8 is a graphic that depicts a number of papers in the paleoclimate reconstruction area together with some of the proxies used. We note that many of the proxies are shared. Using the same data also suggests a lack of independence.

The MBH98/99 work has been sufficiently politicized that this community can hardly reassess their public positions without losing credibility. Overall, our committee believes that the MBH99 assessment that the decade of the 1990s was the likely the hottest decade of the millennium and that 1998 was likely the hottest year of the millennium cannot be supported by their analysis.

Recommendations

Recommendation 1. Especially when massive amounts of public monies and human lives are at stake, academic work should have a more intense level of scrutiny and review. It is especially the case that authors of policy-related documents like the IPCC report, *Climate Change 2001: The Scientific Basis*, should not be the same people as those that constructed the academic papers.

Recommendation 2. We believe that federally funded research agencies should develop a more comprehensive and concise policy on disclosure. All of us writing this report have been federally funded. Our experience with funding agencies has been that they do not in general articulate clear guidelines to the investigators as to what must be disclosed. Federally funded work including code should be made available to other researchers upon reasonable request, especially if the intellectual property has no commercial value. Some consideration should be granted to data collectors to have exclusive use of their data for one or two years, prior to publication. But data collected under federal support should be made publicly available.

Recommendation 3. With clinical trials for drugs and devices to be approved for human use by the FDA, review and consultation with statisticians is expected. Indeed, it is standard practice to include statisticians in the application-for-approval process. We judge this to be a good policy when public health and also when substantial amounts of monies are involved, for example, when there are major policy decisions to be made based on statistical assessments. In such cases, evaluation by statisticians should be standard practice. This evaluation phase should be a mandatory part of all grant applications and funded accordingly.

Recommendation 4. Emphasis should be placed on the Federal funding of research related to fundamental understanding of the mechanisms of climate change. Funding should focus on interdisciplinary teams and avoid narrowly focused discipline research.

MR. WHITFIELD. Thank you, Dr. Wegman, and Dr. North, you are recognized for your opening statement.

DR. NORTH. Thank you, Mr. Chairman. Before I begin, I would like to introduce Peter Bloomfield from North Carolina State University, who is a professor of statistics there, and he was on our committee, the NAS committee, and so I will use him if I need to during the course of--

MR. WHITFIELD. Welcome, Dr. Bloomfield.

DR. NORTH. Good morning, Mr. Chairman, and members of the committee. My name is Jerry North. I am a professor of atmospheric sciences at Texas A&M University and it is nice to see one Aggie here. He said he took some statistics there and I suspect he knows more than he is letting on today. And I served as the Chairman of the National Research Council's committee on surface temperature reconstruction for the last 2,000 years.

My comments today will highlight the findings of our committee's recently released report. Its aim was to asses the state of scientific efforts to reconstruct surface temperature records for the Earth over the last few thousand years, and to comment on the implications of these efforts for our understanding of global climate change. Surface temperature reconstructions are only one of many lines of evidence supporting the conclusion that the climate is warming in response to human activities. These long records give context and perspective to the issue but they are not the primary evidence. In fact, human-induced climate change is quite real.

First some background. Widespread thermometer records only the last 150 years or so. To extrapolate deeper into the past, scientists have learned to use proxy evidence such as tree rings, corals, ocean and lake sediments, ice cores, glacier records, boreholes, and historical documents. To give one example, the advances and retreats of glaciers can tell us whether the climate has been warmer or cooler on the average at that location. Starting in the 1990s, scientists began combining proxy evidence for many locations in an effort to estimate temperature changes averaged over broad geographic regions for the last few thousand years.

Much attention has been concentrated on papers published by Michael Mann, Raymond Bradley, and Malcolm Hughes in 1998 and 1999. This is partly because the authors concluded that the Northern Hemisphere was warmer during the late 20th Century than at any time during the past millennium. In addition, it was illustrated with a simple graphic, the so-called hockey stick curve, that was featured prominently in the 2001 Intergovernmental Panel on Climate Change report, and you have seen that graphic.

Our committee examined the scientific literature in great depth, considered written and oral remarks from experts representing a broad range of perspectives. We reached five major conclusions. Number one, the warming of about one degree Fahrenheit during the 20th Century is real. No one doubts it.

Number two: Besides the rapid warming in the 20th Century, two other features appear to be common in the records, a cool period centered in A.D. 1700 called the Little Ice Age and a warm period around 1000 known as the Medieval Warm Period, details about the latter being much less certain.

Number three: It can be said with a high level of confidence that global mean surface temperature was higher during the last few decades of the 20th Century than during any comparable period since 1600. This statement is justified by the consistency of the evidence from a wide variety of geographically diverse proxies. If we could put that graphic up. That one. That is the only one I will show. So here is the kind of diverse evidence that I would like to just mention. These are different curves from different investigators. Most of them have come out after the Mann et al. work, and some of them don't rely on the statistical techniques at all. The boreholes, for example, come from the direct physics, no calibration with the instrumental temperatures, and the same is true for the glacier length records.

Number four: Less confidence can be placed in large-scale surface temperature reconstructions from A.D. 900 to 1600. We find that temperatures at many, but not all, locations were higher during the last 25 years than during any period of comparable length since A.D. 900, but the uncertainties increase substantially as one moves backward in time through this period and are not yet fully quantified. Now, the way we tried to illustrate that on this graphic is by showing a sort of darkening graying as you go back, and one of my colleagues on the committee says well, as you go back beyond the year 1600, things get a little murkier, so the amount of the kinds of data that we have and so on are much less certain. We don't understand all of the interrelations and so forth, so I can go into that in more detail if you need it.

And number five, very little confidence can be assigned to statements concerning the average surface temperatures prior to about A.D. 900, so we just don't know enough about that period.

Now, the basic conclusion of the 1999 paper by Mann and his colleagues was that the late 20th Century warmth in the Northern

Hemisphere was unprecedented during at least the last 1,000 years. This conclusion has substantially been supported by an array of evidence, but substantial uncertainties remain for the period before about 1600, and I can give you some illustrations of other ways of looking at the problem later if that should come up in questions. Our main disagreements with the Mann 98/99 papers are related to the assertions about warmth of individual decades and individual years. We don't subscribe to that kind of definition of the problem. We also question some of their statistical methodology, in fact, some of the same claims that were put forward by Dr. Wegman and you will hear some later as well.

However, our reservations with some aspects of the original papers by Mann and colleagues should not undermine the fact that the climate is warming and will continue to warm as a result of human activities. In fact, the scientific consensus regarding human-induced climate warming, global warming, would not be substantively altered if the global mean surface temperature 1,000 years ago was found to be as warm as it is today although there is evidence that this really is a very exceptional period that we are in now, and again, I can come back to that during questions. This is because we don't know enough about the driving forces of the climate over that long period.

During the last 150 years, we have considerable evidence about the increase in greenhouse gas concentrations and we know a lot about the other things that tend to nudge the climate system as well. By the way, a lot has been learned about climate in the last 30 or 40 years. I mean, it is a very rapidly changing field and we have all the giant computers and satellites now at our disposal to help us. So we know a lot more about this than we did 30 years ago. And in the last quarter century, when warming was particularly steep, we also have good data on the sun because for the last 25 years we have been measuring the sun very, very accurately from outside the atmosphere using satellites. Aerosols--we have a very good idea of how the dust and tiny particles in the atmosphere have been changing over the last 25 years and probably 50, both of which--both of these two drivers of climate change, the sun and the aerosols, really are negligible compared to the forcing from greenhouse gases.

Moreover, climate models can only reproduce the warming of the 20th Century when greenhouse gases are included. Our knowledge of the driving forces over the last several thousands of years is not yet good enough to go back beyond this recent period, so that is the reason that that early data doesn't really close or finish off the story.

So now in conclusion, our committee finds that large-scale surface temperature reconstructions contribute to climate research, they are important, and that they contain meaningful climate signals. Our confidence in the reconstructions becomes stronger when multiple independent lines of evidence point to the same general result such as the warmth of the last few decades of the 20th Century relative to the last 400 years. Further research, especially in the collection of additional proxy evidence, would help to reduce the uncertainties and allow us to make more definitive conclusions over longer time periods.

I thank you for your attention, and I would be happy to answer any questions, and I may call on Dr. Bloomfield to help me.

[The prepared statement of Dr. Gerald R. North follows:]

PREPARED STATEMENT OF DR. GERALD R. NORTH, DEPARTMENT OF ATMOSPHERIC SCIENCES, TEXAS A&M UNIVERSITY

Good morning, Mr. Chairman and members of the Committee. My name is Jerry North. I am the Harold J. Haynes Endowed Chair in Geosciences and a Distinguished Professor in the Department of Atmospheric Sciences at Texas A&M University, and I served as chair of the National Research Council's Committee on Surface Temperature Reconstructions for the Last 2,000 Years. The National Research Council is the operating arm of the National Academy of Sciences, National Academy of Engineering, and the Institute of Medicine, chartered by Congress in 1863 to advise the government on matters of science and technology.

My comments today will highlight the key findings of our committee's recent report on Surface Temperature Reconstructions for the Last 2,000 Years, which was prepared in response to a request from Chairman Boehlert of the House Committee on Science. Our task was to assess the state of scientific efforts to reconstruct surface temperature records for the Earth over the past few hundred to few thousand years, and to comment on the implications of these efforts for our understanding of global climate change. I would like to stress, in advance, that surface temperature reconstructions are only one of multiple lines of evidence supporting the conclusion that the climate is warming in response to human activities, and they are not the primary evidence. In fact, man-made climate change is quite real. However, our report did not examine projections of future climate change in any detail, and it does not make policy recommendations.

Let me begin by briefly explaining how scientists go about reconstructing surface temperatures for the past few millennia. Because widespread thermometer measurements are only available for the last 150 years or so, scientists estimate temperatures in the more distant past by analyzing what we call proxy evidence. Common proxies include tree rings, corals, ocean and lake sediments, cave deposits, ice cores, glacier records, boreholes, and historical documents. To give one example, the annual growth rings in a tree can tell us about the climate at that location when the growth occurred -- a thicker ring indicates better growing conditions than a narrow ring. Likewise, the advances and retreats of glaciers around the world provide evidence of warming and cooling.

Starting in the 1990s, scientists began using sophisticated methods to combine proxy evidence from many different locations in an effort to estimate surface temperature changes averaged over broad geographic regions during the last few hundred to few thousand years. These large-scale surface temperature reconstructions have enabled researchers to estimate past temperature variations over the Northern Hemisphere or even the entire globe, often with time resolution as fine as decades or even individual years.

The papers that have attracted the most attention in this research area were written by Michael Mann, Raymond Bradley and Malcolm Hughes in 1998 and 1999. Dr. Mann and his colleagues used a new methodology to combine data from a number of sources to estimate temperatures in the Northern Hemisphere for the last six centuries, and then for the last 1,000 years. This research received wide attention, in part because the authors concluded that the Northern Hemisphere was warmer during the late 20th century than at any other time during the past millennium, and also because it was illustrated with a simple graphic, the so-called hockey stick curve, that was featured prominently in the 2001 Intergovernmental Panel on Climate Change report.

Our committee was composed of 12 members with expertise in a range of fields including climate modeling, statistics, climate change and variability, and each of the types of proxies commonly used for climate reconstructions. The committee took multiple steps to accomplish its charge. First, we hosted a two-day workshop in March 2006 and invited numerous speakers from all perspectives in the debate to participate. We examined the scientific literature in great depth, and considered written input from many sources. The evidence we considered included large-scale surface temperature reconstructions from a number of different research teams, as well as local and regional proxy evidence collected from a number of locations around the world. We also looked at the instrumental record and examined various statistical considerations.

Let me summarize five key conclusions we reached after reviewing the evidence:

1. The instrumentally measured warming of about 1°F during the 20th century is also reflected in borehole temperature measurements, the retreat of glaciers, and other observational evidence, and can be simulated with climate models.

2. Large-scale surface temperature reconstructions yield a generally consistent picture of temperature trends during the preceding millennium, including relatively warm conditions centered around A.D. 1000 (identified by some as the "Medieval Warm Period") and a relatively cold period (or "Little Ice Age") centered around 1700.

3. It can be said with a high level of confidence that global mean surface temperature was higher during the last few decades of the 20th century than during any comparable period during the preceding four centuries. This statement is justified by the consistency of the evidence from a wide variety of geographically diverse proxies.

4. Less confidence can be placed in large-scale surface temperature reconstructions for the period A.D. 900 to 1600. Presently available proxy evidence indicates that temperatures at many, but not all, individual locations were higher during the past 25 years than during any period of comparable length since A.D. 900. The uncertainties

increase substantially backward in time through this period and are not yet fully quantified.

5. Very little confidence can be assigned to statements concerning the hemispheric mean or global mean surface temperature prior to about A.D. 900.

The main reason that our confidence in large-scale surface temperature reconstructions is lower before A.D. 1600 and especially before A.D. 900 is the relative scarcity of precisely dated proxy evidence. Other factors limiting our confidence in surface temperature reconstructions include the relatively short length of the instrumental record, the fact that all proxies are influenced by many climate variables, and the possibility that the relationship between proxy data and local surface temperatures may have varied over time. All of these considerations introduce uncertainties that are difficult to quantify.

Overall, the committee finds that efforts to reconstruct temperature histories for broad geographic regions using multiproxy methods are an important contribution to climate research and that these large-scale surface temperature reconstructions contain meaningful climatic signals. The individual proxy series used to create these reconstructions generally exhibit strong correlations with local environmental conditions, and in most cases there is a physical, chemical, or physiological reason why the proxy reflects local temperature variations. Our confidence in the results of these reconstructions becomes stronger when multiple independent lines of evidence point to the same general result, as in the case of the Little Ice Age cooling and the 20th century warming.

The basic conclusion of the 1999 paper by Dr. Mann and his colleagues was that the late 20th century warmth in the Northern Hemisphere was unprecedented during at least the last 1,000 years. This conclusion has subsequently been supported by an array of evidence that includes both additional large-scale surface temperature reconstructions and pronounced changes in a variety of local proxy indicators, such as melting on icecaps and the retreat of glaciers around the world, which in many cases appear to be unprecedented during at least the last 2,000 years.

Based on the analyses presented in the original papers by Mann et al. (1998, 1999) and this newer supporting evidence, the committee finds it plausible that the Northern Hemisphere was warmer during the last few decades of the 20th century than during any comparable period over the preceding millennium. However, the substantial uncertainties currently present in the quantitative assessment of large-scale surface temperature changes prior to about A.D. 1600 lower our confidence in this conclusion compared to the high level of confidence we place in the Little Ice Age cooling and 20th century warming. Even less confidence can be placed in the original conclusions by Mann et al. (1999) that "the 1990s are likely the warmest decade, and 1998 the warmest year, in at least a millennium" because the uncertainties inherent in temperature reconstructions for individual years and decades are larger than those for longer time periods, and because not all of the available proxies record temperature information on such short timescales. We also question some of the statistical choices made in the original papers by Dr. Mann and his colleagues. However, our reservations with some aspects of the original papers by Mann et al. should not be construed as evidence that our committee does not believe that the climate is warming, and will continue to warm, as a result of human activities.

Large-scale surface temperature reconstructions are only one of multiple lines of evidence supporting the conclusion that climatic warming is occurring in response to human activities, and they are not the primary evidence. The scientific consensus regarding human-induced global warming would not be substantively altered if, for example, the global mean surface temperature 1,000 years ago was found to be as warm as it is today. This is because reconstructions of surface temperature do not tell us *why* the climate is changing. To answer that question, one would need to examine the factors, or forcings, that influence the climate system. Prior to the Industrial Revolution, the primary climate forcings were changes in volcanic activity and in the output of the Sun, but the strength of these forcings is not very well known. In contrast, the increasing concentrations of greenhouse gases in the atmosphere over the past century are consistent with both the magnitude and the geographic pattern of warming seen by thermometers.

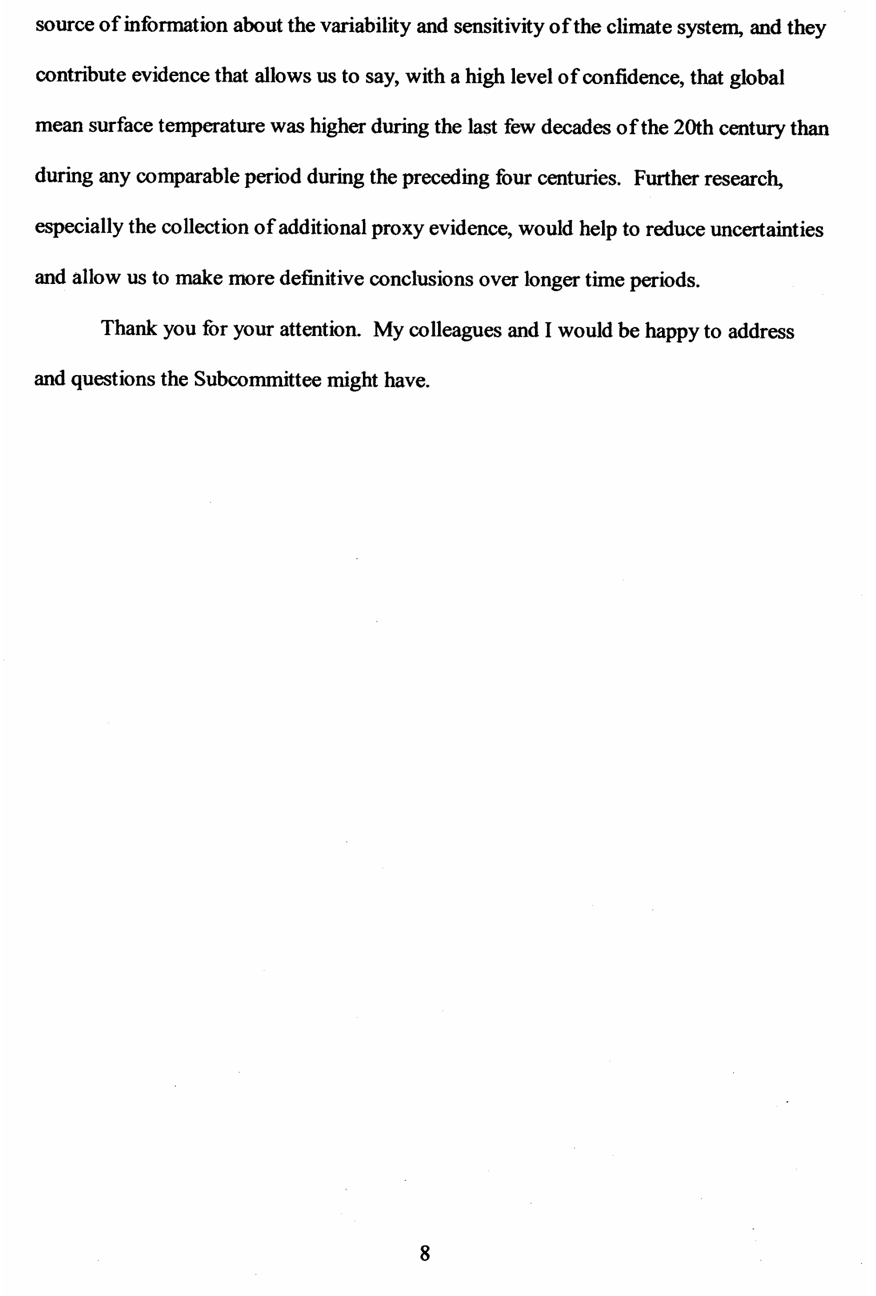
One significant part of the controversy on this issue is related to data access. The collection, compilation, and calibration of paleoclimatic proxy data represent a substantial investment of time and resources, often by large teams of researchers. The committee recognizes that access to research data is a complicated, discipline-dependent issue, and that access to computer models and methods is especially challenging because intellectual property rights must be considered.

Our view is that all research benefits from full and open access to published datasets and that a clear explanation of analytical methods is mandatory. Peers should have access to the information needed to reproduce published results, so that increased confidence in the outcome of the study can be generated inside and outside the scientific community. Paleoclimate research would benefit if individual researchers, professional societies, journal editors, and funding agencies continued their efforts to ensure that existing open access practices are followed.

So where do we go from here? Large-scale surface temperature reconstructions have the potential to further improve our knowledge of temperature variations over the last 2,000 years, particularly if additional proxy evidence can be identified and obtained from areas where the coverage is relatively sparse and for time periods before A.D. 1600 and especially before A.D. 900. It would also be helpful to update proxy records that were collected decades ago, in order to develop more reliable calibrations with the instrumental record.

New analytical methods, or more careful use of existing ones, may also help circumvent some of the existing limitations of surface temperature reconstructions performed using multiple proxy data. Efforts to improve our understanding of how solar output and volcanic activity have varied over the past few thousand years are also important. Finally, because some of the most important potential consequences of climate change are linked to changes in regional circulation patterns, hurricane activity, and the frequency and intensity of droughts and floods, regional and large-scale reconstructions of changes in other climatic variables, such as precipitation, over the last 2,000 years would provide a valuable complement to those made for temperature.

In summary, as science has made progress over the past few years, we have learned that large-scale surface temperature reconstructions are important tools in our understanding of global climate change. Surface temperature reconstructions are a useful



source of information about the variability and sensitivity of the climate system, and they contribute evidence that allows us to say, with a high level of confidence, that global mean surface temperature was higher during the last few decades of the 20th century than during any comparable period during the preceding four centuries. Further research, especially the collection of additional proxy evidence, would help to reduce uncertainties and allow us to make more definitive conclusions over longer time periods.

Thank you for your attention. My colleagues and I would be happy to address and questions the Subcommittee might have.

MR. WHITFIELD. Dr. North, thank you and Dr. Wegman both for your testimony, and Dr. North, now, you are a Ph.D. Are you a climatologist or--

DR. NORTH. I have a Ph.D. in physics from the University of Wisconsin.

MR. WHITFIELD. From the University of Wisconsin.

DR. NORTH. Yes.

MR. WHITFIELD. A wonderful school.

DR. NORTH. Yes. It is a wonderful school.

MR. WHITFIELD. Almost as good as Texas A&M.

DR. NORTH. Well, comparable.

MR. WHITFIELD. Now, have you had the opportunity to review Dr. Wegman's and his associates--

DR. NORTH. Yes, I did receive it a few days ago so I don't think I have read it in the detail that I should but I have been able to look through it.

MR. WHITFIELD. And you all don't know each other? You are not friends or--

DR. NORTH. No, I met him at our briefing a couple of weeks ago just for a handshake.

MR. WHITFIELD. Well, I was wondering if you might just take a minute or two to summarize your--as a professional in this area and your experience in this area. What is your reaction to their report?

DR. NORTH. Well, I think that on many things we are in agreement. The studies that--I mean, the examination they did of the statistical procedures and the Mann et al. papers is not the way we would--that I would have done it in hindsight, especially now looking back. It is not the way I would have done it. I don't think there is anything dishonest about it or anything like that, but I think that the analyses that the Wegman group did really were--some of those were examined by the statisticians on our committee and I don't think that we are in any great disagreement about it. Let me just mention this, that the criticisms don't mean that the MBH claims were wrong. They just mean that the MBH claims are not convincing by themselves. So if you pull together other information, then that does change the view a bit.

MR. WHITFIELD. Now, Dr. Wegman, I am not a statistician but obviously a statistician is where you look at data and from that data you try to look at the probability of something happening or not happening and whatever. Is that just in a rough layman's term what statistics is all about, or give me your definition of statistics or a statistician.

DR. WEGMAN. Well, I think a statistician generally tries to look at data and represent the meaning, the inferences that are available from that data as straightforwardly and honestly as possible.

MR. WHITFIELD. Now, Dr. North said that his group had reviewed your document and that they agreed with much of what you said and you have indicated that one of your primary concerns about the Mann document is the center point that was utilized in his hockey stick graph. Would you elaborate on that a little bit?

DR. WEGMAN. Yes. They used the period from 1902 to 1995, which was the instrumented temperature record that they used, so they used that period to calibrate the proxy data. They centered their overall proxy data on that period, 1902 to 1995, and of course temperature was rising in that period, so when you center on that period, you push the rest of the proxy data below the axis. That has, as I mentioned, the net effect of increasing the variance and making the principal component methodology pick out that kind of shape. So it preferentially attempts to fit those kind of shapes in the first principal component.

MR. WHITFIELD. And it does establish this hockey stick showing a rapidly increasing--

DR. WEGMAN. That is essentially the mechanism that creates the hockey stick. If you do the--as I showed in the one graph, if you do the centering properly, the hockey stick disappears.

MR. WHITFIELD. Now--

CHAIRMAN BARTON. Would the Chairman yield on that point?

MR. WHITFIELD. Yes.

CHAIRMAN BARTON. Dr. Wegman, you say when you center it properly. Put in layman's terms that those of us that are not statisticians, what does that mean, centering it properly?

DR. WEGMAN. Thank you for asking. The principal components analysis methodology requires that the data be centered on the mean of the overall series, so if you are doing reconstructions, let us say, back to year 1000, 1000 to 2000, then you should center on the average value of the proxy series for the period 1902 to--

CHAIRMAN BARTON. In which there is better data. I mean, they--there could be a plausible reason why they did what they did, the more accurate data, they are more certain of it?

DR. WEGMAN. Well, they are more certain of the temperature data but the net effect of the decentering is to preferentially pick out these--

CHAIRMAN BARTON. But what they should have done was if they are going to measure from one 1000 to 2000, they should have used all the data points and came up with the mean and centered wherever that mean was?

DR. WEGMAN. That is correct, yes.

CHAIRMAN BARTON. Thank you.

MR. WHITFIELD. And I think the reason that is important is that when you make a categorical statement that the 1990s were the warmest decade in a millennium and that 1998 was the warmest year in the 1,000, I mean, it is difficult to make a statement like that categorically if the centering is not correct. Would you agree with that?

DR. WEGMAN. Yes, I agree.

MR. WHITFIELD. And I think that is the whole basis of this hearing because this hockey stick--all of us are concerned about global warming but I do think we have an obligation and responsibility--everyone has latched onto this hockey stick and almost created a panic in a way, and maybe we should be panicked, but I think it is important that we understand how the hockey stick came about, and that is what we are talking about today. Now, Dr. North, do you agree with Dr. Wegman's centering analysis or not?

DR. NORTH. I do. I think that he is right about that. However, you know, we have to be careful here and not throw the baby out with the water.

MR. WHITFIELD. Right.

DR. NORTH. Because there have been other analyses, papers published after the Mann papers in which people just took a simple average. Dr. Crowley wrote a paper just a short time after that in which he didn't use the principal component analysis at all. He got essentially the same answer. And so--

MR. WHITFIELD. Is that what we refer to as the CPS analysis?

DR. NORTH. I don't know what the initials--but he just took the average instead of dealing with the data the way one does it in the principal component analysis, so what I am arguing, and some other people have also done this same, there have been many studies later that don't use principal component analysis and the ones that I showed you, it is not there now--

MR. WAXMAN. Mr. Chairman.

DR. NORTH. They don't all use principal component analysis.

MR. WHITFIELD. Yes?

MR. WAXMAN. Will you yield to me? I am just wondering if Dr. Wegman is familiar with Dr. Crowley's way of handling the statistics and if he thinks that the conclusions are suspect in the Crowley study.

DR. WEGMAN. Well, let me say that simple averaging of proxies, depending on how the proxies are selected, can yield the same kind of results. In fact, if--I don't know if you can put up my backup slide, backup figure number 2, the backup figure number 2 shows--

MR. WHITFIELD. Well, are we putting this graph up? Where is this graph? Okay. There we go. Okay.

DR. WEGMAN. This is using the CPS, simple averaging proxy methodology, just like the principal components, and by doing the simple averaging of proxies appropriately selected, you can reconstruct the same shape that you had with the principal component-type methodology. So it is possible depending on how you approach this.

MR. WHITFIELD. So you can do a lot of things, just depending upon what data you use, what the centering is and so forth?

DR. WEGMAN. Exactly.

MR. WHITFIELD. Now, let me just ask both of you one question quickly. My time has been used by other people.

MR. WAXMAN. I would like to ask unanimous consent that the chairman be given two additional minutes, but are you critical--because that was my question--are you critical of his methodology in reaching the same conclusion?

DR. WEGMAN. I am saying that it is quite possible to use the CPS, the averaging methodology, and come to the same conclusion that Dr. Mann had. I am not saying he did that because I haven't studied his paper in such detail as to be willing to say that.

MR. WHITFIELD. Well, let me just ask you on this whole issue of scientific analysis and scientific collaboration and so forth, you mentioned this social networking, for lack of a better term. I mean, like any other profession, scientists, statisticians, they deal with each other, they know each other, they write articles together and so forth. But how serious is this issue of bodies making scientific reports and getting into a pattern of talking to the same people all the time about the same thing and they all have the same views? Is that a significant problem or not?

DR. WEGMAN. I think it potentially can be. It would be naive to think that there are not competing social networks within a discipline area. Sometimes the competing social networks keep each other in check. In the statistical arena, for example, there is a group of people who view themselves as classical statisticians. There is a group of people who view themselves as Bayesian statisticians. As one of our reviewers said, Heaven help you if you get a reviewer from a competing social network.

MR. WHITFIELD. Okay.

DR. WEGMAN. And I think it would be naive to think that these things don't exist. They exist in peer review journals, they exist in reviews of proposals submitted to the NSF and other organizations.

MR. WHITFIELD. Would you like to make a comment about this whole issue, Dr. North?

DR. NORTH. Well, I would be pleased to. There are several matters here. Social networking, it does seem to me to be a little bit of a problem to pick out that this young scientist got busy and found himself 43 coauthors. I think a lot of us would look at that and say my, he is quite a charismatic young man who has gone out and found himself 43 collaborators. That is something that I would probably look very favorable on if I were considering him for tenure. And so there is that. Now, do people collaborate and think similarly? Of course they do. But, you know, if you look back at the history of, say, quantum mechanics in the early 1920s, it was Einstein, Bohr, Heisenberg, all these people. I am sure if you did a similar analysis, you would probably find something very like that, but in fact these guys hated each other. I mean, they were very, very competitive. And if you look at the 43 authors, I am sure that not all of them like to go out and have a beer together. This is pretty competitive business, and I will tell you, if somebody can find a way to knock down someone else's theory, that is their road to recognition and fame. We all do that. That is part of the game and we really enjoy that part of the game. So yes and no.

MR. WHITFIELD. All right. Thank you. My time has expired and I will recognize Mr. Stupak.

MR. STUPAK. Mr. Chairman, because of time constraints, I am letting Mr. Waxman go now and I will catch the next round.

MR. WAXMAN. Thank you very much, Mr. Stupak and Mr. Chairman. That was an interesting analysis, Dr. North. We are sometimes sheltered by our own politics but it looks like academics have their politics.

DR. NORTH. They do.

MR. WAXMAN. And I guess we should take that into consideration, but I don't think we doubt all science because experts agree with each other or that they are competing with each other. Is that--

DR. NORTH. That is correct. You know, the process works. You know, as they say, it is a little like making sausage. You have heard that one.

MR. WAXMAN. On June 7, 2005, 11 National Science academies issued a joint statement calling on world leaders "to acknowledge that the threat of climate change is clear and increasing" and in their joint statement, the science academies of Brazil, Canada, China, France, Germany, India, Italy, Japan, Russia, the United Kingdom, and the United States declared, "There is now strong evidence that significant global warming is occurring." They also stated that it is likely that most of the warming in recent decades can be attributed to human activities. Mr. Chairman, I would like to ask unanimous consent that this statement from the premiere scientific institutions be placed in the record.

MR. WHITFIELD. Without objection.

[The information follows:]

TAB 19

Joint science academies' statement: Global response to climate change

Climate change is real

There will always be uncertainty in understanding a system as complex as the world's climate. However there is now strong evidence that significant global warming is occurring[1]. The evidence comes from direct measurements of rising surface air temperatures and subsurface ocean temperatures and from phenomena such as increases in average global sea levels, retreating glaciers, and changes to many physical and biological systems. It is likely that most of the warming in recent decades can be attributed to human activities (IPCC 2001)[2]. This warming has already led to changes in the Earth's climate.

The existence of greenhouse gases in the atmosphere is vital to life on Earth – in their absence average temperatures would be about 30 centigrade degrees lower than they are today. But human activities are now causing atmospheric concentrations of greenhouse gases – including carbon dioxide, methane, tropospheric ozone, and nitrous oxide – to rise well above pre-industrial levels. Carbon dioxide levels have increased from 280 ppm in 1750 to over 375 ppm today – higher than any previous levels that can be reliably measured (i.e. in the last 420,000 years). Increasing greenhouse gases are causing temperatures to rise; the Earth's surface warmed by approximately 0.6 centigrade degrees over the twentieth century. The Intergovernmental Panel on Climate Change (IPCC) projected that the average global surface temperatures will continue to increase to between 1.4 centigrade degrees and 5.8 centigrade degrees above 1990 levels, by 2100.

Reduce the causes of climate change

The scientific understanding of climate change is now sufficiently clear to justify nations taking prompt action. It is vital that all nations identify cost-effective steps that they can take now, to contribute to substantial and long-term reduction in net global greenhouse gas emissions.

Action taken now to reduce significantly the build-up of greenhouse gases in the atmosphere will lessen the magnitude and rate of climate change. As the United Nations Framework Convention on Climate Change (UNFCCC) recognises, a lack of full scientific certainty about some aspects of climate change is not a reason for delaying an immediate response that will, at a reasonable cost, prevent dangerous anthropogenic interference with the climate system.

As nations and economies develop over the next 25 years, world primary energy demand is estimated to increase by almost 60%. Fossil fuels, which are responsible for the majority of carbon dioxide emissions produced by human activities, provide valuable resources for many nations and are projected to provide 85% of this demand (IEA 2004)[3]. Minimising the amount of this carbon dioxide reaching the atmosphere presents a huge challenge. There are many potentially cost-effective technological options that could contribute to stabilising greenhouse gas concentrations. These are at various stages of research and development. However barriers to their broad deployment still need to be overcome.

Carbon dioxide can remain in the atmosphere for many decades. Even with possible lowered emission rates we will be experiencing the impacts of climate change throughout the 21st century and beyond. Failure to implement significant reductions in net greenhouse gas emissions now, will make the job much harder in the future.

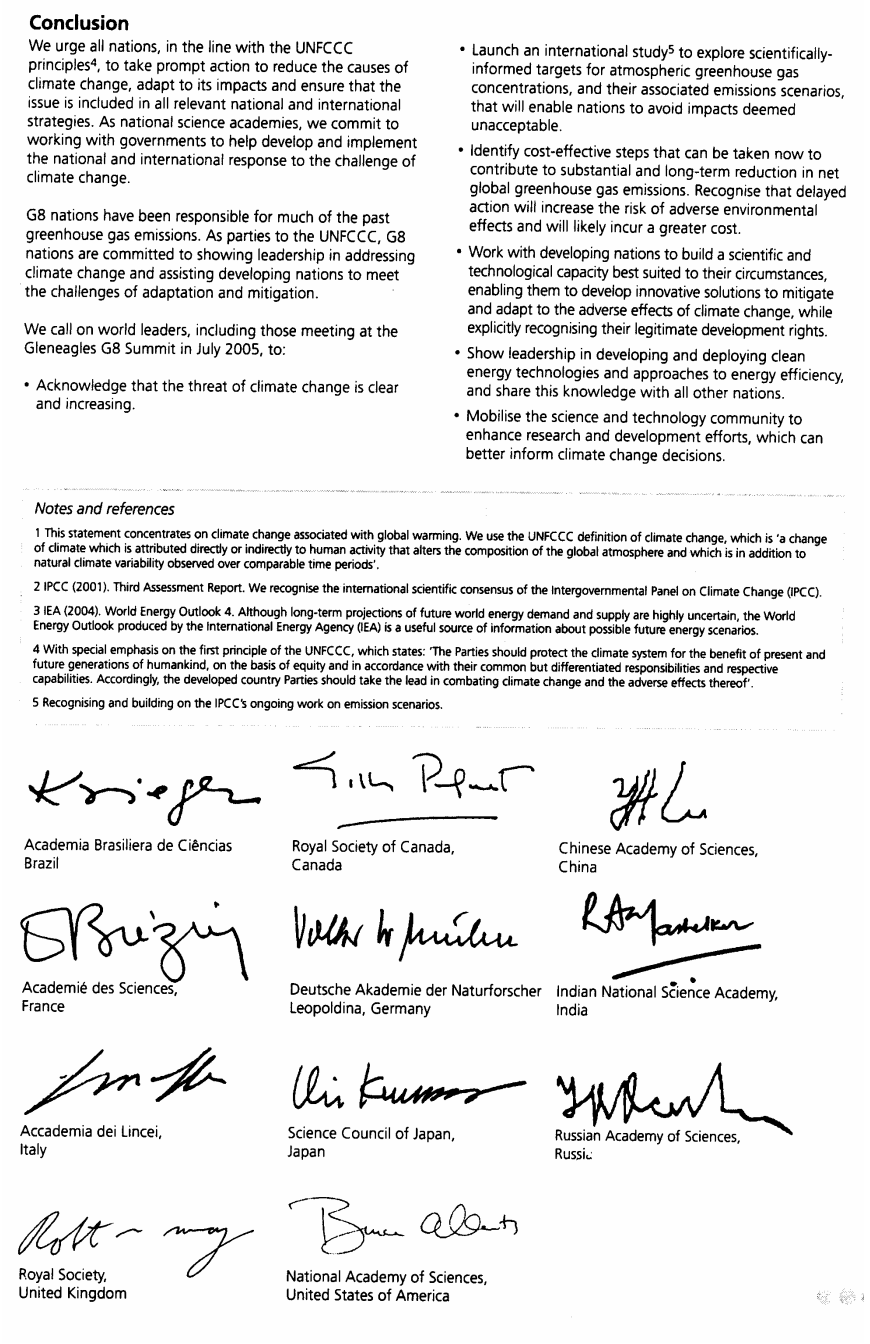
Prepare for the consequences of climate change

Major parts of the climate system respond slowly to changes in greenhouse gas concentrations. Even if greenhouse gas emissions were stabilised instantly at today's levels, the climate would still continue to change as it adapts to the increased emission of recent decades. Further changes in climate are therefore unavoidable. Nations must prepare for them.

The projected changes in climate will have both beneficial and adverse effects at the regional level, for example on water resources, agriculture, natural ecosystems and human health. The larger and faster the changes in climate, the more likely it is that adverse effects will dominate. Increasing temperatures are likely to increase the frequency and severity of weather events such as heat waves and heavy rainfall. Increasing temperatures could lead to large-scale effects such as melting of large ice sheets (with major impacts on low-lying regions throughout the world). The IPCC estimates that the combined effects of ice melting and sea water expansion from ocean warming are projected to cause the global mean sea-level to rise by between 0.1 and 0.9 metres between 1990 and 2100. In Bangladesh alone, a 0.5 metre sea-level rise would place about 6 million people at risk from flooding.

Developing nations that lack the infrastructure or resources to respond to the impacts of climate change will be particularly affected. It is clear that many of the world's poorest people are likely to suffer the most from climate change. Long-term global efforts to create a more healthy, prosperous and sustainable world may be severely hindered by changes in the climate.

The task of devising and implementing strategies to adapt to the consequences of climate change will require worldwide collaborative inputs from a wide range of experts, including physical and natural scientists, engineers, social scientists, medical scientists, those in the humanities, business leaders and economists.



Conclusion

We urge all nations, in the line with the UNFCCC principles[4], to take prompt action to reduce the causes of climate change, adapt to its impacts and ensure that the issue is included in all relevant national and international strategies. As national science academies, we commit to working with governments to help develop and implement the national and international response to the challenge of climate change.

G8 nations have been responsible for much of the past greenhouse gas emissions. As parties to the UNFCCC, G8 nations are committed to showing leadership in addressing climate change and assisting developing nations to meet the challenges of adaptation and mitigation.

We call on world leaders, including those meeting at the Gleneagles G8 Summit in July 2005, to:

- Acknowledge that the threat of climate change is clear and increasing.
- Launch an international study[5] to explore scientifically-informed targets for atmospheric greenhouse gas concentrations, and their associated emissions scenarios, that will enable nations to avoid impacts deemed unacceptable.
- Identify cost-effective steps that can be taken now to contribute to substantial and long-term reduction in net global greenhouse gas emissions. Recognise that delayed action will increase the risk of adverse environmental effects and will likely incur a greater cost.
- Work with developing nations to build a scientific and technological capacity best suited to their circumstances, enabling them to develop innovative solutions to mitigate and adapt to the adverse effects of climate change, while explicitly recognising their legitimate development rights.
- Show leadership in developing and deploying clean energy technologies and approaches to energy efficiency, and share this knowledge with all other nations.
- Mobilise the science and technology community to enhance research and development efforts, which can better inform climate change decisions.

Notes and references

[1] This statement concentrates on climate change associated with global warming. We use the UNFCCC definition of climate change, which is 'a change of climate which is attributed directly or indirectly to human activity that alters the composition of the global atmosphere and which is in addition to natural climate variability observed over comparable time periods'.

[2] IPCC (2001). Third Assessment Report. We recognise the international scientific consensus of the Intergovernmental Panel on Climate Change (IPCC).

[3] IEA (2004). World Energy Outlook 4. Although long-term projections of future world energy demand and supply are highly uncertain, the World Energy Outlook produced by the International Energy Agency (IEA) is a useful source of information about possible future energy scenarios.

[4] With special emphasis on the first principle of the UNFCCC, which states: 'The Parties should protect the climate system for the benefit of present and future generations of humankind, on the basis of equity and in accordance with their common but differentiated responsibilities and respective capabilities. Accordingly, the developed country Parties should take the lead in combating climate change and the adverse effects thereof'.

[5] Recognising and building on the IPCC's ongoing work on emission scenarios.

Academia Brasiliera de Ciências
Brazil

Royal Society of Canada,
Canada

Chinese Academy of Sciences,
China

Academié des Sciences,
France

Deutsche Akademie der Naturforscher
Leopoldina, Germany

Indian National Science Academy,
India

Accademia dei Lincei,
Italy

Science Council of Japan,
Japan

Russian Academy of Sciences,
Russia

Royal Society,
United Kingdom

National Academy of Sciences,
United States of America

MR. WAXMAN. Dr. North, I would like to begin with you. Do you agree with the statement of these premiere institutions that there is now strong evidence that significant global warming is occurring and that it is likely that most of the warming can be attributed to human activities?

DR. NORTH. Yes, I do.

MR. WAXMAN. And Dr. North, the national science academies also state that the scientific understanding of climate change is sufficiently clear to justify nations taking prompt action. They say it is important that we take cost-effective steps now to reduce our emissions or else it will be more costly to act in the future. Again, do you agree with that statement?

DR. NORTH. Well, now you are stepping a little bit beyond my role here. I will talk about the science but what we ought to do is somebody else's business.

MR. WAXMAN. I am concerned that some are going to hear about Dr. Wegman's statistical criticism of the early Mann study and somehow conclude that global warming is still an open question. In order to put the overall importance of this issue in context, I would like to ask you about some of the other evidence of global warming. Are the Mann studies the basis for the ice core studies that give us data going back hundreds of thousands of years?

DR. NORTH. No.

MR. WAXMAN. Are the Mann studies the basis for the recorded atmospheric temperature records that we have maintained for the last 150 years?

DR. NORTH. No.

MR. WAXMAN. Dr. Crowley is going to testify later today that although the Mann study was influential in the IPCC's 2001 assessment, the studies, which demonstrated that the instrumental record and the models could not be reconciled without an anthropogenic greenhouse influence, were even more influential. Were those studies based on the Mann studies?

DR. NORTH. I don't think so. I am sorry. I didn't hear everything you said.

MR. WAXMAN. Well, Dr. Crowley is going to tell us that--

DR. NORTH. He will talk about that, sure.

MR. WAXMAN. --although the Mann study was influential with the IPCC's 2001--

DR. NORTH. Well, it was part of the report. It was a part of the report.

MR. WAXMAN. Right.

DR. NORTH. But as I have said, it is only one of several lines of evidence that are used in drawing those conclusions.

MR. WAXMAN. And so therefore you have further studies that seem to come to similar conclusions?

DR. NORTH. There are other studies, and they were shown on the graphic that I showed you.

MR. WAXMAN. And they weren't based on the Mann studies, were they?

DR. NORTH. They were not based on the Mann studies. Now, there are cases where they use the same data so there is some correlation and that is what I think Dr. Wegman referred to and that is correct. See, there is only a limited amount of data, so--

MR. WAXMAN. In 2005, two research teams led by scientists at the Scripps Institution for Oceanography and NASA's Goddard Institute for Space Studies published studies in *Science* magazine that concluded that not only is the Earth's air and land warming, but the oceans are warming as well and that heating has penetrated more than 1,000 feet into the ocean's depth. Jim Hanson, director of the NASA Goddard Institute for Space Studies and the lead author of one of the studies, called these findings "the smoking gun of global warming." Dr. North, are these studies in any way based on the Mann 1998 and 1999 studies?

DR. NORTH. No, not at all.

MR. WAXMAN. In July 2005, *Nature* magazine published a study by Dr. Kerry Emanuel of M.I.T. who found that the destructive power of hurricanes is increasing along with ocean temperatures. Dr. Emanuel found that the total destructive potential of hurricanes has increased markedly during the last 30 years. While natural cycles in the pattern of ocean circulation likely played a role, Dr. Emanuel attributes at least part of the increase to global warming. Just last month the publication *Geophysical Research Letters* published a new study by Dr. Kevin Trenberth and Dr. Dennis Shea of the National Center for Atmospheric Research which concludes that global warming fueled hurricane intensity in the waters of the tropical North Atlantic in 2005, while natural cycles were only a minor factor. Dr. North, are these papers by Dr. Emanuel, Dr. Trenberth, and Dr. Shea in any way based upon Mann's 1998 and 1999 studies?

DR. NORTH. No, no.

MR. WAXMAN. Drs. Mears and Wentz published an article in *Science* magazine in August 2005 that resolves a longstanding conflict in the global warming debate. For years global warming naysayers, based on the work of Dr. John Christy at the University of Alabama, have argued that satellite data showed that the Earth's atmosphere was warming far slower than the Earth's surface. These scientists reanalyzed the raw satellite data and found that the lower atmosphere is actually warming slightly faster than the surface in agreement with the theory and models. These scientists found that the previous analysis of the satellite data had inaccurately corrected for changes in the satellite's measurement time resulting from the decay of their orbit. Dr. Christy has now acknowledged his mistake and has adjusted his data series, making it much more consistent with other results. Dr. North, is the Mears and Wentz study in any way based on Mann's 1998 and 1999 studies?

DR. NORTH. Absolutely not. Dr. Christy was actually on our committee, by the way.

MR. WAXMAN. He was on--

DR. NORTH. He was on the NAS committee.

MR. WAXMAN. Finally, if we were to--

DR. NORTH. If I may just add one thing. You know, just because a paper is published, it goes out for the community. People--the wolves attack, and this particular study by Spencer and Christy took many years before the error was finally found. It doesn't mean these guys are villains. It is just that--

MR. WAXMAN. If you knew that Dr. Mears--

DR. NORTH. --they did their best. It took years to find that mistake.

MR. WAXMAN. If you knew that those two scientists were friends with--

MR. WHITFIELD. Would the gentleman excuse me one minute? Did you say it took many years before the error was discovered?

DR. NORTH. Before the error in the Spencer-Christy study using satellite data was found. It was a good-faith effort on their part but it turned out to be wrong.

MR. WAXMAN. If you knew that these gentlemen were friends with Dr. Mann, would that make you suspect their work?

DR. NORTH. I have no idea whether they know him.

MR. WAXMAN. Finally, if we were to sweep away the Mann studies and forget that they existed, would that in any way erode the validity of any of the studies I just mentioned?

DR. NORTH. I do not think it would.

MR. WAXMAN. Would there still be--

DR. NORTH. We wouldn't--

MR. WAXMAN. Would there still be a scientific consensus that global warming is happening, it is being caused by humans and that some people think it is time to act now?

DR. NORTH. Yes, I think there would be.

MR. WAXMAN. And Dr. North, my point in asking you about these other studies is simply to illustrate how wrong it would be for anyone to draw sweeping conclusions from a statistical criticism of one or two studies from 8 years ago. Unfortunately, the Republican majority on this committee has been completely content to sit back and ignore global warming. They ignored it while President Bush frayed our relationships with our international allies over global warming. They ignored it while the committee crafted an energy policy that exacerbates global warming and they continue to ignore it as evidence piles up about the severity of the situation. Instead, we spend our time attacking climate researchers who have infuriated the oil lobby by contributing to our knowledge of this issue, and apparently that is the one thing that the Majority simply cannot ignore. My time is just about expired, and we have a vote on the House floor. I thank the witnesses for their testimony and Dr. North for responding to my questions.

MR. WHITFIELD. Mr. Chairman, we have 8 minutes to vote on the floor. Would you like to start your questions and come--

CHAIRMAN BARTON. I would recommend that we recess and let us go vote, give our witnesses a chance to have a personal convenience break and then come back.

MR. WHITFIELD. We have two votes on the floor. The first vote will be over in about 10 minutes and then we will have another one, so we will reconvene at about 12:15.

[Recess.]

MR. WHITFIELD. At this time I recognize the Chairman for his 10 minutes of questions.

CHAIRMAN BARTON. Thank you, Mr. Chairman. I appreciate the courtesy and I appreciate our witnesses here today. My first question is a personal question to you, Dr. Wegman, and it is not normally one that I would even think about asking but there has been some attempt to portray you as a pawn of this committee or me personally. I am told that you voted for Vice President Gore for president in the year 2000. Is that correct?

DR. WEGMAN. That is correct.

CHAIRMAN BARTON. So you are by no means a radical, wild-eyed, hard core, right wing Republican?

DR. WEGMAN. No, sir.

CHAIRMAN BARTON. Okay. How often, if ever, have you been in Texas?

DR. WEGMAN. I was in Texas in hill country a few weeks ago but I have been to Houston a few times, interacting in my social network with David Scott.

CHAIRMAN BARTON. But you are not--you and I until this morning have had no phone calls, no e-mails, no--

DR. WEGMAN. I didn’t even know what you looked like until--

CHAIRMAN BARTON. Which is a blessing for you, right?

DR. WEGMAN. No, sir.

CHAIRMAN BARTON. All right. Now, let me ask you, Dr. North, obviously you and I went to--I attended the school where you have been an illustrious professor for a number of years and I asked you during the break if you and I had met and you said that we had met on an airplane once.

DR. NORTH. We had a 2-minute--a 30-second conversation.

CHAIRMAN BARTON. So you and I have had some personal interaction, but that is it. Again, there is no real association in terms of continuing basis or anything. When Mr. Waxman was here, he was asking some questions of you, Dr. North, about headlines that had occurred and papers that had been issued that state the possibility or the probability that global warming is real and it is caused by humans, and it is your personal opinion that global warming is real and that a large part of the reason it is real is because of human emissions of greenhouse gases. That is a fair statement of yours? You need to push that button, put your microphone on. Let the record show that he said yes. But we have some headlines here that have been purported to be because of global warming. Dr. North, one of them is that more frogs are dying as the planet warms. Are you aware of that?

DR. NORTH. I have heard of it.

CHAIRMAN BARTON. You have heard of that. How about because of global warming, irrigation fuels warmer temperatures in California's central valley, are you aware of that?

DR. NORTH. I have not heard of that one.

CHAIRMAN BARTON. Okay. How about the irony of global warming, more rain, less water?

DR. NORTH. I am familiar with that idea. I don't know if I have seen that headline.

CHAIRMAN BARTON. Global warming could sour the wine industry?

DR. NORTH. I don't--

CHAIRMAN BARTON. Poison ivy grows faster, bigger, more irritating?

DR. NORTH. No, I don't--

CHAIRMAN BARTON. Global warming weakens trade winds. Global warming's next casualty, igloo. Global warming could overwhelm storm drains. Strange things happening to Pacific coast marine life. Global warming might create lopsided planet. Global warming makes seas less salty. Space ring could shade Earth and stop global warming. My point is, a lot of people are jumping on the global warming bandwagon and there is no question it is serious, there is no question that eminent people like yourself believe the causality of human emissions. I don't have a problem with that. I mean, you pointed out in your testimony what science is supposed to be about. My problem is that everybody seems to think that it is automatically a given and that we shouldn't even debate the possibility of it and we probably shouldn't debate the causes of it, and I think that is wrong. That is one of the reasons that we are holding this hearing.

I want to put up the digitized temperature curve number 2 that Dr. Wegman was referring to. We determined that you couldn't prove the hockey stick by using the data points, Dr. Wegman concluded that, and so Mr. Waxman said well, that is okay but there are other studies and one of them is the study of a methodology that was not using the methodology that Dr. Mann used, and that is--it is kind of an S curve and--that is not?

DR. NORTH. Figure number 2 is the one that--

CHAIRMAN BARTON. That one right there. Now, in that curve there, Dr. North, the highest point looks to me to be about the year 1300. Would you agree with that?

DR. NORTH. Well, that is what it shows on that graphic.

CHAIRMAN BARTON. Okay. But it is definitely higher than the 1900s.

DR. NORTH. Higher than--I think that curve goes up to the middle of the 20th Century although I am not sure.

CHAIRMAN BARTON. But it is obvious--I am not saying that is the truth, okay, but I am saying, if that is a justification for global warming in that particular study, which I believe is purported to be a Crowley study, that is using average temperatures, that that particular graph shows the warmest period was somewhere between 1100 and 1400. Is that correct?

DR. NORTH. Well, that is what the curve shows. I cannot tell you where that one actually came from. We used a graphic like that in our report just to give some perspective about how people thought the curve looked 15 year ago, 16 years ago, so we used a graphic like that. I believe you have replotted it here.

CHAIRMAN BARTON. Now--

DR. WEGMAN. Let me be precise. This is a curve from the IPCC 1990 report.

DR. NORTH. Sixteen years ago.

CHAIRMAN BARTON. Okay. And let us go to the study--there is a comparison in Dr. Wegman's testimony of the Mann report and I believe this curve. There are two--keep going. There are two documents--yes--no, not that one.

DR. NORTH. Number 4 and 5, I think.

CHAIRMAN BARTON. Well, my question is, something happened between the chart that we up here that showed the early 1300s being the warmest period and Dr. Mann's study that obviously shows the 20th Century, and my question is, what changed in the modeling or the methodology or the data set? Because Dr. Mann wipes out that early warming period. It is just not there.

DR. NORTH. Is that for me?

CHAIRMAN BARTON. It could be for either one of you.

DR. NORTH. Well, there is more data available 10 years later than there was in that first report. In fact, I have a feeling that that first report--I hope you will ask Crowley that later because I think he will know more about it than--

CHAIRMAN BARTON. Well, is it now the consensus of the majority of the scientific community that this early warming period just didn't exist?

DR. NORTH. No, I think that there is good evidence that such a medieval warm period did exist, however, it may not have existed at the same time at different locations on the Earth, and I could give you some information about that. For example, if you look in Greenland, there was a very distinct warming period in that time around--between 1000 and 1200. In fact, there were colonies of people who lived there from Denmark and their civilization disappeared there. They went back to Denmark or died out, I am not sure which.

CHAIRMAN BARTON. But I mean, it is striking--

DR. NORTH. So there is evidence, historical and so on, that--

CHAIRMAN BARTON. It is on page 15 of your report, and I have the prepublication copy. You have the figure 03 at the top and then you have the figure 04 at the bottom. Oh three is a schematic description of global warming that is the IPCC report of 1990 and then the 04 figure is the Mann graph, and it is just striking to me that there is no correlation between the two, or very little.

DR. NORTH. Oh, actually, if you look at the gray area in the Mann graph, that is the area where the curve could fall with some reasonable probability. That is their error margin.

CHAIRMAN BARTON. Let me ask you--

DR. NORTH. If you look at the family of our curves that I showed in our graphic, the family of curves that were derived by using several different methods and different sources, you find that that family of curves really does fall pretty close to where the gray is here, especially if you put margins of error on each of those comparable to these.

CHAIRMAN BARTON. Let me ask you--

DR. NORTH. And we would dispute how accurately Mann and company did that.

CHAIRMAN BARTON. I understand that.

DR. NORTH. That is another matter.

CHAIRMAN BARTON. I understand that. It looks like my time is expired, so I want to ask one more question. Dr. North, do you dispute the conclusions or the methodology of Dr. Wegman's report?

DR. NORTH. No, we don't. We don't disagree with their criticism. In fact, pretty much the same thing is said in our report. But again, just because the claims are made, doesn't mean they are false.

CHAIRMAN BARTON. I understand that you can have the right conclusion and that it not be--

DR. NORTH. It happens all the time in science.

CHAIRMAN BARTON. Yes, and not be substantiated by what you purport to be the facts but have we established--we know that Dr. Wegman has said that Dr. Mann's methodology is incorrect. Do you agree with that? I mean, it doesn't mean Dr. Mann's conclusions are wrong, but we can stipulate now that we have--and if you want to ask your statistician expert from North Carolina that Dr. Mann's methodology cannot be documented and cannot be verified by independent review.

DR. NORTH. Do you mind if he speaks?

CHAIRMAN BARTON. Yes, if he would like to come to the microphone.

MR. BLOOMFIELD. Thank you. Yes, Peter Bloomfield. Our committee reviewed the methodology used by Dr. Mann and his coworkers and we felt that some of the choices they made were inappropriate. We had much the same misgivings about his work that was documented at much greater length by Dr. Wegman.

MR. WHITFIELD. If I may interrupt just one minute. We didn't swear you in so I want you to swear now that the testimony you gave was the truth.

[Witness sworn]

MR. WHITFIELD. Thank you.

CHAIRMAN BARTON. I would like to submit for our record an e-mail that was received, and I would be more than willing to share it with the Minority if they have not seen it before. They have it? It is an e-mail from Yasmin Said to Peter Spencer and it says, "To whom it may concern: I have read the reports of Chairman Barton and Chairman Whitfield entitled "ad hoc committee report on the hockey stick global climate reconstruction by Edward J. Wegman, David Scott, and Yasmin H. Said" and what follows this work of Wegman, Scott, and Said is simply referred to as Report. The assessment of previous results given in the Report is correct. The Report is entirely correct in stating that the most rudimentary additive model, the model of a simple temperature signal with superimposed noise, is not adequate to describe the complex relationships involved in climate dynamics. There is no physical process found in nature that does not involve feedback in one form or another to regulate the action of the system. The statistical methods and models described in the report use more variables and make possible the construction of more elaborate reconstructions that allow feedback and interactions. The report represents the correct way to proceed. It is especially important to bring the professional statistical community into the picture in order to assure that a sound analytical foundation is secured in the continuing development of this program. Sincerely, Enders A. Robinson, member of the National Academy of the USA, fellow of the European Academy of Scientists, professor emeritus and the Maurice Ewing and J. Lamar Rozelle, Chair, Department of Earth and Environment, Columbia University." And I yield back.

MR. WHITFIELD. Thank you. At this time I recognize Mr. Inslee.

MR. STUPAK. Wait a minute. Did we accept this e-mail that was read into the record, or what?

MR. WHITFIELD. Well, he asked for unanimous consent if you all--do you have an objection to it?

MR. STUPAK. Well, let us object for now. We will ask some questions of it later.

MR. WHITFIELD. They object to it being entered until they clarify a few things with that.

CHAIRMAN BARTON. But they had the document. I don't want them to accept it if they have not seen it. I was told that they had seen it.

MR. WHITFIELD. We were told that you all had it last night but is that not--

CHAIRMAN BARTON. But certainly we don't want to put anything in that hadn't been cleared. Mr. Chairman, they have every right to object if they haven't seen it.

MR. WHITFIELD. Well, while they are discussing it, Mr. Inslee, why don't you proceed with your questions.

MR. INSLEE. Thank you. Dr. Wegman, can you cite to us the first three laws of thermodynamics?

DR. WEGMAN. Probably not.

MR. INSLEE. And you shouldn't be ashamed of that because you are a statistician, not a physicist.

DR. WEGMAN. That is correct.

MR. INSLEE. But it is important for us to talk about that in the context of some things I want to ask you. Because I believe reviewing the literature, and I spent some time doing this, it is beyond any reasonable doubt that there is a strong worldwide scientific consensus that human activities are putting carbon dioxide and other global warming pollutants in the air in a way that is changing our climate in fundamental ways. I want to ask you some questions about your testimony here today. I want to refer you to a chart that is up on the screen to your left, and it shows concentrations of carbon dioxide in the Earth's atmosphere going back 160,000 years and basically what it shows is that the concentrations now which are in the lower right-hand circle are higher than they have been in any time in the last 160,000 years. They also show that those concentrations of carbon dioxide will go up approximately doubling in the next century by the year 2100 unless this Congress pulls its head out of the sand and does something about it. Now, the question I want to ask you, these carbon dioxide samples are beyond dispute because of direct physical measurement of old air trapped in glaciers and that they are not subject to any scientific doubt whatsoever. Neither as far as I know is there any question but that the carbon dioxide levels will significantly increase in the order of doubling of pre-industrial times in the next century if we do not act. So the question I ask you, is anything in your criticism of the Mann report in any way suggests that those conclusions I just stated to you that are reflected on this graph regarding carbon dioxide levels are faulty?

DR. WEGMAN. No, I don't believe they are.

MR. INSLEE. So if you accept the first three laws of thermodynamics and basic chemistry and our ability to judge CO_2 levels and if you accept the premise that carbon dioxide in the atmosphere has the capacity of essentially trapping heat in the energy system of the Earth--by the way, do you accept that proposition?

DR. WEGMAN. I don't know about the second proposition. I do not know the mechanisms for trapping heat.

MR. INSLEE. Well, I will just tell you, the mechanisms of carbon dioxide essentially traps heat in infrared range of a frequency. Light comes in an ultraviolet range, it bounces back in an--not really bounces back but emitted in an infrared range and carbon dioxide traps it. It traps it like a blanket, as a crude metaphor. Now, what we know beyond a shadow of a doubt is that carbon dioxide in the next century is going to be at levels double any time in the last 160,000 years and double what it was in pre-industrial times. Now, does your criticism of Dr. Mann's research in any way suggest that it would not be a good idea to reduce our carbon dioxide loading into the atmosphere?

DR. WEGMAN. My expertise does not extend to global warming and I have no position on this.

MR. INSLEE. Well, I think that is important for you to say that because what we are finding here is that there is this enormous worldwide consensus. I look at the joint academy statement--this is a joint academy statement of every science academy in the industrialized world and every single one of them state that it is a consensus that human activity is causing changes to the climate. I will just read directly. "It is likely that most of the warming in recent decades can be attributed to human activities. This warming has already led to changes in the Earth's climate." It is signed by Canada, Germany, France, Italy, Japan, United Kingdom, Russia, China, Brazil, and the National Academy of Sciences under the administration of George Bush. Now, I guess the question to you is, do you have any reason to believe all those academies should change their conclusion because of your criticism of one report?

DR. WEGMAN. Of course not.

MR. INSLEE. Why not?

DR. WEGMAN. Because my report was very specific on a very specific issue that was asked of me and we answered that very specific question.

MR. INSLEE. Well, let me suggest another reason. The reason you don't suggest these academies are wrong is because they have a mountain of evidence from ice core data, through glacier data, to ocean acidification, to radar data, to surface and deep ocean temperature data that indicate that this world is changing because we are putting too much carbon dioxide in it. Isn't that right? That is why you are not suggesting they change their report.

DR. WEGMAN. Well, there is the old statistical process that says association does not mean causation.

MR. INSLEE. Well, there is another statistical by Mark Twain is that there are three kinds of lies: lies, damn lies, and statistics, but I won't bring that one up. I want to ask--

DR. WEGMAN. Of course, he is not a statistician either.

MR. INSLEE. Dr. North, I want to quote--in your testimony you said, "However, our reservations with some aspects of the original papers by Mann et al. should not be construed as evidence that our committee does not believe that the climate is warming and will continue to warm as a result of human activities." You go on to say, "The scientific consensus regarding human-induced global warming would not be substantively altered if for example the global mean surface temperature 1,000 years ago was found to be was warm as it is today." Now, in listening to your testimony, what I take from this is that even if we were to conclude that Dr. Mann had never been born, the study had never been done, conclude even if there was a medieval warming period that approximated temperatures today, even if we were to accept that as a verity, even if we knew that today, what I am hearing your testimony tell us is that there is enough evidence of other methods and other dynamics at work in the climate today that we can with a reasonable degree of assurance conclude that humans are responsible for at least a portion of the changes in temperatures. Is that a fair statement?

DR. NORTH. Well, let me separate myself from the report now. I believe that is true but we didn't address that issue in the report.

MR. INSLEE. And could you at least in summary fashion tell us about the other evidence that leads to your conclusion other than Dr. Mann's?

DR. NORTH. Well, let me mention a few things that my colleague on the committee, Kurt Cuffey from the University of California-Berkeley sent. So this is a little about the medieval warm period. It takes a couple minutes so I apologize for that. So Greenland shows a clear signal of both medieval warmth and 20th Century warming. These are recorded unambiguously in isotopes and boreholes, nothing to do with this extrapolation method. The medieval was warmer than the 20th Century up to about 1990, but you know it has warmed quite a bit in the last 15 years, so another piece of evidence is Ellesmere Island. This is in the Canadian Arctic and there is an icecap there. It also shows evidence of a medieval warm period and 20th Century warming and the isotopes and melt records. The melt in particular shows summertime warmth in the 20th Century was greater than the medieval warm period, so there is that one. The composite of all available low latitude--this is Tibet and the Andes and there is things in Africa, Kilimanjaro. Ice core, isotope records show the 20th Century climate is truly anomalous on the time scale of 2,000 years. This is an objective quantitative measure of climate arising from physical processes. We cannot, however, separate a pure temperature signal from it because these glaciers are influenced by both moisture availability and temperature because hydrology is important too. All we can say is that the sum of the climate processes determining the isotope records have reached an anomalous state. One more--two more. Melt at the summit of Quelccaya--this is a big icecap in the Andes, the largest Andean icecap--was strong enough in the late 20th Century to destroy annual layering of isotopes which did not happen during the medieval period. Now, the tropics are a very interesting place to look at climate. They are probably a little more representative of the global average, not as much natural variability in the tropics. So we had melting recently in the Quelccaya glacier but it didn't happen in the medieval warm period.

MR. INSLEE. Doctor, I want to ask one quick question. My time is almost up.

DR. NORTH. I am sorry.

MR. INSLEE. Put the slide up on the acidification, Tracy, that one right there if I can. Doctor, I made reference to acidification that is taking place in our oceans as a result of carbon dioxide going into the atmosphere, then going into solution in the oceans. Could you briefly summarize that dynamic and what the state of our knowledge is about that?

DR. NORTH. I am not an expert on this. I have seen the report and the essence is that as we increase carbon dioxide in the atmosphere, the carbon dioxide of course dissolves in seawater just as it does in Pepsi-Cola, so the greater the partial pressure of carbon dioxide in the atmosphere, the more that will be dissolved in the ocean and then you wind up with--by combining with other things, you wind up with a more acidic ocean so the pH of the ocean goes down, becomes more acidic. This attacks the corals and other things. So there could be something going on with aquatic life. Again, we are really pretty far away from--

MR. INSLEE. And is that independent of temperature issues?

DR. NORTH. That is independent of temperature.

MR. INSLEE. So even if temperature doesn't go up, this dynamic can acidify the ocean?

DR. NORTH. That has been happening and I presume will continue to happen.

MR. INSLEE. Thank you. Well, we would like to change that actually. Some of us have ideas about that.

DR. NORTH. That is not my job.

MR. WHITFIELD. Mrs. Blackburn, you are recognized for 10 minutes.

MRS. BLACKBURN. Thank you, Mr. Chairman, and thank you all for your patience as we work through our votes today. Dr. Wegman, I have got three quick questions for you and Dr. North, I have got, I think one probably for you and I am going to try to finish so everyone gets their questions in before the next vote. But Dr. Wegman, you said in your testimony that Dr. Mann's data is very obscure, incomplete, and disorganized, and I wanted you to expand on that and give us an example of how that data should have been presented, if you have something tangible.

DR. WEGMAN. Well, I had two things in mind. First of all, when I read the paper originally, it took me probably 10 times to read it to really understand what he was trying to say. He uses phrases that are not standard in the literature I am familiar with. He uses, for example, the phrase "statistical skill" and I floated that phrase by a lot of my statistical colleagues and nobody had ever heard of that phrase, statistical skill. He uses measures of quality of fit that are not focused on the kind of things typically we do. We went to his website to try and figure out where his data was. He has a website at the University of Virginia. We basically downloaded everything that was in his FTP website to try and gather together--try and understand what was going on. The materials tended to be very cryptic. When we looked at the Fortran code that he wrote, it was very difficult to understand how you could, in the Fortran code you read in the data, but it was unclear where the data was and how you could actually read it in and the coding of the data, so all those things tended to make it very difficult to try and replicate anything that he did. Ultimately, I believe it was in 2004, he published a corrigendum and it showed that some of the data that he used in the 1998 paper was not referenced in the 1998 paper and other material that he did reference in the 1998 paper was not actually used. So there was a lack of clarity in both the archived data as well as the writing of the appear itself that I found difficult to decipher.

CHAIRMAN BARTON. Will the gentlelady yield just for--

MRS. BLACKBURN. I will yield.

CHAIRMAN BARTON. When you said his data was in Fortran code, what is Fortran code?

DR. WEGMAN. Fortran is a computer programming language that was invented in 1957.

CHAIRMAN BARTON. And when was the last time anybody else than Dr. Mann used that code?

DR. WEGMAN. Well, I suspect--

CHAIRMAN BARTON. I knew it at Texas A&M in the 1960s and I had not heard the term and I wanted to make sure we were talking the same--

DR. WEGMAN. Well, certainly programming languages have evolved dramatically over the years. Most of my colleagues use a software package called RS Plus. Many people use Mat Lab these days.

CHAIRMAN BARTON. The Fortran code is not something that would be normally used today by too many people?

DR. WEGMAN. I would think in certain circles it might be but it is reflective of the notion that there aren't--

DR. NORTH. Most climate models do use Fortran code.

CHAIRMAN BARTON. Oh, they do?

DR. NORTH. Yes.

CHAIRMAN BARTON. So that is standard?

DR. NORTH. It is standard in mathematical solution of these kinds of problems, not statistics. He is right about that. So Mat Lab is coming on but Fortran is very commonly used in large climate model work.

CHAIRMAN BARTON. Well, then I should be able to do some of this because I can code in Fortran. I yield back.

MRS. BLACKBURN. Dr. Wegman, I still want to come to you. So what you are saying is that he--I want to go back to one thing on the data that he chose to input on the website, he was selective in the nature of what he chose to put in there and I guess that is much like what we saw with the calibration issue over the years that he used in that--

DR. WEGMAN. There were a large number of proxies that were used in the 1998 and 1999 papers. As a matter of fact, it probably wasn't very selective. He essentially threw everything including the kitchen sink into this data set.

MRS. BLACKBURN. I want to ask you a question that Dr. Crowley makes a statement in his testimony that was submitted to us, that the data is reused, Dr. Mann's data is reused because it is the best data. But you say that other papers cannot claim to be independent verification if they reuse the same data. So I would like for you to speak to that and kind of reconcile the differing views.

DR. WEGMAN. Well, in one of our plots we had a plot that showed the data that was being used as the proxies versus the 11 or 12 papers that had been published since 1998 and the striking thing is, I think, that essentially there are two methodologies that we talked about, the CPS methodology and the CFR methodology, and my contention is that if you use the same data and the same basic methodology, you can--

MRS. BLACKBURN. Then following on with that, if you were to structure an external statistical review for climate papers that would guarantee to be an independent verification of methods used, how would you structure this?

DR. WEGMAN. Well, I think there are a couple of approaches. One of the analogies I kind of liked was that the folks that do the hockey stick kind of thing call themselves--I think they call themselves the hockey team and when games are being played, you also need referees, so I think it would be a good idea to have referees for the hockey games. My own feeling is that it would be useful as we said in one of our recommendations that there be an external review and that it be funded as part of this kind of activity. If you have significant statistical methodology being used in a scientific study, then you really ought to have statistical review as well as the peer paleoclimate review. I think this extends beyond just paleoclimate stuff. It is true, for example, in biostatistics, biological science, medical science, that there is typically a heavy involvement with statistical review. I think in terms of things like sociology, psychology, there is heavy involvement with statisticians in this kind of framework. It appears to me that in the physical sciences, the same mental set is not typically done.

MRS. BLACKBURN. Thank you. Dr. North, I have got a couple of quick questions on surface records and satellite measurements that I want to give to you but I have only got a minute and a half left and I think I will submit these to you and then ask for your response, and Mr. Chairman, I will yield back so somebody else can get their questions on the record before we go for another vote.

MR. WHITFIELD. Thank you, Mrs. Blackburn. At this time I recognize Mr. Stupak.

MR. STUPAK. Thank you, Mr. Chairman. Dr. Wegman, in your report you criticized Dr. Mann for not obtaining any feedback or review from mainstream statisticians. In compiling your report, did you obtain any feedback or review from paleoclimatologists?

DR. WEGMAN. No, of course not, but we weren't addressing paleoclimate issues. We were addressing--

MR. STUPAK. But you said you had difficulty understanding some of the terms of art that Dr. Mann used and you had to call your social network to figure it out so wouldn't it have been helpful to have paleoclimatologists?

DR. WEGMAN. To say that I didn't contact any climate people is not entirely accurate. We have--

MR. STUPAK. But they weren't used in compiling your report--that was the question--correct?

DR. WEGMAN. Well, I am not sure how to answer that. I certainly--

MR. STUPAK. Well, yes or no is probably the best way. Did you have any paleoclimatologists when you compiled your report?

DR. WEGMAN. Not on our team, but that doesn't mean I didn't talk to any.

MR. STUPAK. Did anyone outside your social network peer review your report?

DR. WEGMAN. Yes.

MR. STUPAK. Who was that?

DR. WEGMAN. Well, Enders Robinson.

MR. STUPAK. Is that the e-mail we were talking about earlier?

DR. WEGMAN. Pardon?

MR. STUPAK. Is that the e-mail that was--

DR. WEGMAN. Yes. So--

MR. STUPAK. When you do peer review--

DR. WEGMAN. Let me answer the question. Enders Robinson, Grace Waba, who is a member of the National Academy, Noel Cressy, who is at the Ohio State University, Bill Wasorik, who is at Buffalo State SUNY, David Banks, who is at Duke University, Rich Schareen is the immediate past president of the American Statistical--

MR. STUPAK. Let me ask you this question. If you had a peer review, when are peer reviews usually done? Before a report is finalized or after?

DR. WEGMAN. We had submitted this and had feedback from--

MR. STUPAK. No, no, I am talking about general peer review. If you are going to have a peer review, don't you usually do it before you finalize your report?

DR. WEGMAN. Yes.

MR. STUPAK. Well, your peer review was after you finalized it?

DR. WEGMAN. No, it was before. We submitted this long before.

MR. STUPAK. Well, when was your report finalized?

DR. WEGMAN. I think we dated the final copy about 4 days ago.

MR. STUPAK. Four days ago, so that would be about July 15. This e-mail sort of indicates it is July 17 that you asked for this peer review.

DR. WEGMAN. I had feedback from Enders much earlier than that. We had asked him to send material to us for purposes of coming here.

MR. STUPAK. Well, the e-mail read into the record is Tuesday, July 18, so that would be 3 days after you finalized your report.

DR. WEGMAN. I am sorry. We--

MR. STUPAK. Have you seen this e-mail, the one that--

DR. WEGMAN. Yes, of course I have. Dr. Robinson saw our material before the 18th, before the 17th, before the 16th. He gave us feedback. We incorporated that. He gave us feedback verbally. We incorporated that because there was some interest in getting this report to the committee.

CHAIRMAN BARTON. Would my friend from Michigan yield for one simple question on this same point?

MR. STUPAK. Sure.

CHAIRMAN BARTON. Dr. Wegman, do you object to Mr. Stupak or anybody in the Minority submitting your report for a peer review as long as the peers are qualified in statistical analysis?

DR. WEGMAN. Not at all.

CHAIRMAN BARTON. Thank you.

MR. STUPAK. In doing peer reviews, do scientists who do the report, do they usually submit to people they want to do the peer review? Isn’t that sort of an independent review?

DR. WEGMAN. This is basically the same mechanism that was used at the National Academy. The national--you know, this is not a--

MR. STUPAK. Did you ask these people to do your peer review?

DR. WEGMAN. Yes.

MR. STUPAK. So would they be part of your social network?

DR. WEGMAN. No. When I talk about social network, I am talking about people with whom I have actively collaborated in writing research papers.

MR. STUPAK. It sounds--

DR. WEGMAN. None of these people have actively collaborated with me in writing research papers.

MR. STUPAK. Isn’t the same kind of social network you criticized Dr. Mann on because the people that reviewed his were paleoclimatologists?

DR. WEGMAN. Were the people that had actually worked with and published papers with.

MR. STUPAK. And you have published papers with some of these people that peer reviewed your report?

DR. WEGMAN. No. I just told you no, I haven't.

MR. STUPAK. Let me ask you this. Page 34 of your report, I think you have it in front of you, your 52-page summary there, you have a figure that you say is a digitized version of the temperature profile in the IPCC assessment report of 1990. I take it you read the 1990 IPCC report?

DR. WEGMAN. I am sorry. What page was it?

MR. STUPAK. Page 34 of your report. It is figure 4-5. It is this one right here. We have had some--it has been referred to as figure 2 on the screen a couple times today.

DR. WEGMAN. No, I have not been able to obtain a copy of the 1990 report.

MR. STUPAK. Well, then you must have at least discussed this temperature profile.

DR. WEGMAN. The temperature profile that was published in 1990 I believe was related to the European temperatures and was a cartoon--essentially a cartoon. The point of our discussion here was not that we were trying to say that this was what happened in 1990. The point of our discussion was that you could reproduce this shape from the CPF, CFP and the climate plus--whatever--CPS methodology so we are not endorsing that this was the temperature that was thought of in 1990. We are simply using this as an example.

MR. STUPAK. Were you endorsing 1300 as being a real high temperature time? Were you endorsing it in your report?

DR. WEGMAN. No, we have not said that.

MR. STUPAK. What was the 1990 IPCC temperature profile based on? Basically what was this based on? You are a statistician.

DR. WEGMAN. This--

MR. STUPAK. Was this based on data?

DR. WEGMAN. As I just said moments ago, this was a cartoon I believe that was supposed to be representing a consensus opinion of what global temperature was like in 1990 as published by the IPCC.

MR. STUPAK. Well, is this cartoon then--again, I am on page 34, I am reading now from your report, discussion you have underneath this cartoon. Last line: "The 1990 report was not predicated on global warming scenario. It is clear at least in 1990 the medieval warm period was thought to have temperatures considerably warmer than the present era." Is that your discussion?

DR. WEGMAN. Yes.

MR. STUPAK. So we should not believe that statement then?

DR. WEGMAN. No, I said--I didn't say I believed it was. I said they believed it was. The IPCC gave that report in 1990.

MR. STUPAK. All right. This chart--

DR. WEGMAN. I didn't--

MR. STUPAK. This is in your executive summary, right, page 34, and what I read was correct?

DR. WEGMAN. Yes.

MR. STUPAK. Okay. Let me ask you this question. Have you reviewed any of Mr. Mann's later refinements of his 1999 report?

DR. WEGMAN. I have reviewed some level of detail, not in intense level of detail, the continuing papers, most of which are referenced--in fact, the ones that are referenced--

MR. STUPAK. Did he refine his data and his methodology?

DR. WEGMAN. My take on the situation is that rather than accept the criticism that was leveled, he rallied the wagons around and tried to defend this incorrect methodology.

MR. STUPAK. But did he refine his methods in later studies that he conducted, not whether he rallied the troops? Did he refine his methods? Was his job more accurate as he went on with later reports?

DR. WEGMAN. I believe that he does not acknowledge his fundamental mistake and that he has developed additional papers with himself and his colleagues that try and defend the original hockey stick shape.

MR. STUPAK. Do you know that or are you just guessing?

DR. WEGMAN. I am guessing that.

MR. STUPAK. Okay. Statisticians, should they guess or should they have facts to--

DR. WEGMAN. That is called statistical estimation, yes.

MR. STUPAK. I see. Or a cartoon.

DR. WEGMAN. The cartoon is IPCC's cartoon, not mine.

MR. STUPAK. You relied upon it though in your executive summary. So I am looking at the cartoon. There is no data, is there, to say that around 1300 it warmer than it is in the latter half of--

DR. WEGMAN. I think that is an inaccurate statement. I think there is data. I think the data--

MR. STUPAK. Do you have any of it? Can you show us where any of that is?

DR. WEGMAN. No, I don't have it. I take no responsibility for what IPCC did in 1990. There is no way I could do that. Their data is not available to me. In fact, the reason it was digitized was that I had to go back and construct it from their picture. That doesn't mean no data exist.

And in fact, as far as I know, it was based on European and Asian temperature profiles that were available in the 1990s.

MR. STUPAK. Sure, and in that, it was thought--it was still not clear that all the fluctuations indicated were truly global. In fact, I think some of the testimony earlier said that parts of western Europe, China, Japan, and eastern U.S.A. were a few degrees warmer in July than other parts of the world. Parts of Australia, Chile, and I think Greenland were actually cooler, they said, and China was actually colder than at any other time.

DR. WEGMAN. Yes, I don't dispute that.

MR. WHITFIELD. The gentleman's time has expired. I recognize Mr. Bass.

MR. BASS. And I thank the gentleman for recognizing me. Before I start my questions, I just want to mention that there is a considerable amount of climate change work going underway in New Hampshire, my home state of New Hampshire, the Cold Research Laboratory which is run by the Army Corps of Engineers. They are studying ice core samples from both the Arctic and the Antarctic icecaps and also at the University of New Hampshire. NOAA, National Oceanographic and Atmospheric Administration is conducting ongoing longitudinal studies on the North Atlantic, air, water temperatures. And thirdly, at Hubbard Brook which is another research lab, they are studying climate change effect on trees and plants and other organic matter.

CHAIRMAN BARTON. Could the gentleman yield while--

MR. BASS. Yes. Sure.

CHAIRMAN BARTON. Dr. North, Mr. Stupak just went to some lengths discussing this chart on page 34 of Dr. Wegman's report that is from the IPCC assessment report of 1990. Can you tell us what the IPCC assessment report of 1990 was?

DR. NORTH. The IPCC is the Intergovernmental Panel on Climate Change. It is under the auspices of the United Nations and I don't know the network all the way down to this group but this is a group that meets and is tasked to come up with a report every 5 years approximately.

CHAIRMAN BARTON. But in layman's terms, could we say that the IPPC--

DR. NORTH. No, IPCC.

CHAIRMAN BARTON. IPCC is the technical working group for the United Nations council of parties that ultimately drafted the Kyoto Accords?

DR. NORTH. I don't know if there is a connection. I just don't know that. I am sorry.

CHAIRMAN BARTON. It is my understanding that the IPCC is the group that prepared all the analytical materials and forwarded them on--

DR. NORTH. They may have used their information. The IPCC, their job is to provide assessments, so Congress, political bodies go to them and ask for an assessment of the state of the art or the state of the science at the particular time as it is seen at that time. Of course, it changes so they came out again in 1995 and again in 2000 and there will soon be another one issued.

CHAIRMAN BARTON. But in 1990 when these scientists produced that report, this was their assessment of temperatures between the year 1000 and the mid-1950s?

DR. NORTH. That is what they thought at that time.

CHAIRMAN BARTON. It doesn't mean they were right, it doesn't mean that they haven't changed their mind.

DR. NORTH. That is why--

CHAIRMAN BARTON. But in 1990 the state of the art was, that is what--

DR. NORTH. That is what they thought.

CHAIRMAN BARTON. That is what it was. I yield back.

MR. BASS. Thank you, Mr. Chairman. Dr. Karl is going to follow you in the second panel and I will read a sentence out of his testimony and ask you a question about it, the last page. "At the present time there is no formal process whereby federally funded scientists must submit their data to a long-term data archive facility for use by others. The submission of data to institutions like NOAA's, national climatic data center, the world's paleoclimatic data center, requires significant investment of time by the principal investigators who collected the data to provide the useful information about the proxy data to the receiving data center. In addition, if such data are submitted, a significant investment by the data center would need to be made to ensure that the data is usable by others in perpetuity and safeguards for future generations," and then he goes on about discussions. Dr. North, do you think this is an appropriate priority, and if so, do you think it would require any legislative action? What are your observations about Dr. Karl? And I think Dr. Wegman made the same contention. How do you feel about it, Dr. North?

DR. NORTH. Before I say anything, I should say that I know Dr. Karl and I have actually collaborated with him on some things, so that is a fact. I visited his laboratory, his center in Asheville, which is a very nice operation there. So I do think it is a good idea. I think it is something that the Government through a national laboratory like his should take on. I think this is too much for the little principal investigator out at your university or mine to deal with. So this is a way that data like this can be archived in a nice, clean environment. At Texas A&M, for example, we have the ocean drilling program and so we store these cores there that have been dug and they are carefully archived and protected and so I think that different laboratories should be charged with that kind of duty instead of having every little PI's home base, so I do think it is a good idea.

CHAIRMAN BARTON. Would the gentleman yield on that?

MR. BASS. Certainly.

CHAIRMAN BARTON. I just think the record should show that when I was Congressman for Texas A&M, I helped get the money to establish that program and I am responsible for some of those core samples.

DR. NORTH. And I work with some of those people--

CHAIRMAN BARTON. I want the record to show that.

MR. BASS. Reclaiming my time. I might suggest that this concept might be a starting point for some bipartisan cooperation legislatively if necessary to achieve this objective which would move the issue forward. Dr. Wegman, there has been some discussion about the network issues associated with paleoclimatologists. Is it substantially different than--you know, the incestuous nature of the relationships between the paleoclimatologists. Do you think that it is the same or is different from other academic subjects?

DR. WEGMAN. I don't know all of the academic subjects. What is true, I believe, is that in less focused activities, there are probably more competing social networks which even the playing field a little bit more than it appears to be in the paleoclimatology area. As mentioned earlier, I think for one person to have 43 coauthors is an unusually large number of coauthors. I personally believe that I probably have maybe 15 people that I have worked with over the years.

MR. BASS. Fair enough. Would you take--is it appropriate to take into account in that analysis the size of the entire climatic science community or is paleoclimatology so specialized that you couldn't?

DR. WEGMAN. Yes. I think one of the interesting things that we will probably hear later on is the notion that this paleoclimatology is really an interdisciplinary area so it involves dendrology, it involves people that work with trees, with ice cores and so on and so forth. So it is not totally insular in the sense that it doesn't involve people from other parts of this arena. What is insular though I think is that it doesn't really involve people from the areas that I call the enabling sciences such as mathematics, computer science, and so on. But I think if you sort of followed the second order, third order, fourth order links, you would probably get a more interesting social network as well.

MR. BASS. One last question, Dr. Wegman. The National Academy of Science report that was released last month states the following: "It can be said with a high level of confidence that the global mean surface temperature was higher during the last few decades of the 20th Century than during any comparable"--during, I don't know, there must be a typo here—"during the preceding four centuries." Now, I understand from your testimony on the first page that you want to distance yourself from the issue of global warming, its causes, and its solutions, but would you agree with that statement?

DR. WEGMAN. Yes. I think that is a reasonably cautious verifiable statement that in terms of--and I speak now not as a professional statistician but as a citizen of this country. It seems to me that it is entirely reasonable to say that Dr. North and his panel made an accurate assessment, but it must be understood in the context which is that we have relatively speaking a Little Ice Age, which everybody seems to acknowledge, and so it is not so surprising that it is warming if we are coming out of a Little Ice Age.

MR. BASS. I want to thank both of you gentlemen for your testimony today and I yield back.

MR. WALDEN. [Presiding] The gentleman yields back his time. The gentlelady from Illinois, Ms. Schakowsky, is recognized for 10 minutes.

MS. SCHAKOWSKY. Thank you, Mr. Chairman. I have so many things I want to ask here. Let me start again. Dr. North, I want to confirm what I think you already said. Is Dr. Mann's hockey stick study considered to be the foundation on which all climate change science is based?

DR. NORTH. No.

MS. SCHAKOWSKY. It isn't. And again I want to say, if it never were, if the study simply--the hockey stick, the original and there was a revised in 2003-2004, right, my understanding is, which I guess you disagree, Dr. Wegman, acknowledged some of the mistakes and made some changes but if it never did, would most scientists essentially arrive at the same conclusion as we are seeing, that we are engaged--that this is a time of global warming attributable in large part to human activity?

DR. NORTH. Yes, I think that is true.

DR. WEGMAN. By the way, for what it is worth, I think it is true although I would caution you to not say most scientists. Most climate scientists would probably--

DR. NORTH. That is better. Thank you. I appreciate that.

MS. SCHAKOWSKY. Okay, most climate scientists. Should we not rely on climate scientists for our information about the climate?

DR. WEGMAN. The point I was making was that you are saying most scientists, so the testimony--

MS. SCHAKOWSKY. Well, let me ask--

DR. WEGMAN. --of a chemist is irrelevant to--

MS. SCHAKOWSKY. Exactly. So would you agree then that climate scientists are those that we should primarily refer to when we are asking questions about climate?

DR. WEGMAN. Certainly.

MS. SCHAKOWSKY. So you would agree that human activities are not only increasing atmosphere greenhouse gases but that it is attribute would you say in large part mostly in terms of your understanding as not a climate scientist to human activity?

DR. WEGMAN. I am in no position to say--

MS. SCHAKOWSKY. Well, what did you say you did agree with earlier?

DR. WEGMAN. I said I agree that it is warming. That is what I agreed to. I mean, I said it several times now that the temperature record from 1850 onwards indicate that it is warming.

MS. SCHAKOWSKY. I also had said earlier that in my question to Dr. North and that most scientists agree that in large part or for your purposes I will say in some part attributable to human activity. Would you agree with that?

DR. WEGMAN. I don’t know that for a fact.

MS. SCHAKOWSKY. Okay. You don’t know that.

DR. WEGMAN. Again, it is the connection between carbon dioxide and temperature increase. Now, Mr. Inslee pointed out that he thinks there is a physical explanation based on a blanket of carbon dioxide in the reflection. Carbon dioxide is heavier than air. Where it sits in the atmospheric profile, I don’t know. I am not an atmospheric scientist to know that but presumably if the atmospheric--if the carbon dioxide is close to the surface of the Earth, it is not reflecting a lot of infrared back.

MS. SCHAKOWSKY. Okay. But are you not really qualified to--

DR. WEGMAN. No, of course not.

MS. SCHAKOWSKY. --comment on that. I think since we are talking about scientific data, statistics, let us be clear, and you are challenging a report which form what I understand as Dr. North in some part at least you agree with the critique of the Mann data, so--and I am certainly--I am neither, but we are policymakers here so what I--do you believe that your report disproves that climate change is manmade in any way?

DR. WEGMAN. No.

MS. SCHAKOWSKY. And since you think that you are not in a position to make a decision on global warming, are you uncomfortable at all, Dr. Wegman, that the consequences of what you are saying today to policymakers, I think most of whom, if not all of them, are neither statisticians or climate scientists, could have the impact of saying we don’t need to do anything. Does that make you uncomfortable at all?

DR. WEGMAN. I would hope that our legislators are smarter than that to know that when somebody says that they are using wrong methodology, that does not imply that some fact is not true. I would hope that you would take my testimony with the idea that if something is wrong with this piece of work, it ought to be discarded as a policy tool, and that is precisely what I am saying.

MS. SCHAKOWSKY. Well, let me ask you this. Dr. Mann has published dozens of study since the original hockey stick study and as I said earlier, beginning in 2003 he reformulated the statistical methods. Do you take into account these later studies in your report?

DR. WEGMAN. I have read his later studies. I was not asked about his later studies. I think as science iterates, things do get better, but as I indicated before, one of the unfortunate aspects of this overall situation with Dr. Mann and his colleagues, my attack is not an attack at all. It is simply trying to lay out what I perceive to be a true statement. I think it is unfortunate that rather than moving on and saying gosh, I made a mistake and here is the better situation, here is a better approach, there continues to be a defense which is captured in his web log called realclimate.org.

MS. SCHAKOWSKY. And I understand that there are these battles and sort of the academic politics and scientific politics, et cetera, but do you disagree with Dr. North that even without Dr. Mann altogether or are you using these social--what do you call it--to say that everything now has to be discredited?

DR. WEGMAN. No, I don't think everything at all has to be discredited, and I think the things that do not use the techniques, the flawed methodology with respect to principal components, anything that doesn't use those, I have no position on.

MS. SCHAKOWSKY. And you talked about the cartoon that was in the *Wall Street Journal* article and then my understanding that the graph or whatever you call this, this drawing that it in your report, is it not true that it ends in 1975?

DR. WEGMAN. I think that is approximately accurate. But again, I--this also appears in the National Academy report as well as the *Wall Street Journal*. I did not have the original data for that cartoon, for that graph, and so I had no way of knowing what the full range of the time frame was for that.

MS. SCHAKOWSKY. And would you confirm that, Dr. North, that it goes approximately or maybe exactly to 1975?

DR. NORTH. It is 1975. That is correct.

MS. SCHAKOWSKY. I am trying very hard to understand the point of this hearing and this conflict because if we are through many studies come to the conclusion that there is such a thing is global warming, which is hard to deny on a day like today and yesterday, et cetera, although I am not the scientist, and that it at least in some part is caused by human activity, then why we are doing this really does escape me. I can understand why in academia you may have an interest in discrediting Mann and back and forth, but I am very concerned that this is being used in a way to discredit the whole notion that our country and the rest of the industrialized and developing ought to do anything about global warming, and that is why I asked you that question, Dr. Wegman, if this does not make you somewhat uncomfortable. Can you see in any way how this is being used and does it bother you?

DR. WEGMAN. Well, I can understand that it is your job to sort out the political ramifications of what I have said. In some sense it is not fair for you to say well, gee, you have reported on some fact and that is going to be used in a bad way. The other side of the coin is that, you have tried to get me to say that manmade carbon dioxide emissions are associated with the global warming.

MS. SCHAKOWSKY. Which you can't, right, because you are not a climate scientist.

DR. WEGMAN. I cannot say that, but what I can say is that from 1850 to the present time, the global temperature rise is about 1.2 degrees Centigrade according to the Mann chart. One point two degrees Centigrade translates to about two degrees Fahrenheit. I challenge anybody to go out and tell the difference between 72 and 74 degrees Fahrenheit. What I do say and what I have said repeatedly is that you need to focus on the basic science. You need to understand what the transfer of heat from the ocean to the atmosphere, how that dynamic works, how the climate is going to change based on the physical mechanisms, a fundamental understanding of the physical mechanisms, not on some statistical estimation of those signals.

MR. WALDEN. The gentlelady's time has expired. The gentleman from Florida, Mr. Stearns, for 10.

MR. STEARNS. I thank you, Mr. Chairman. Let me thank both of you for your patience here and how long you have been sitting. We have been changing chairmen here. They get to go but you don't so we are very appreciative of what you are doing here. I think you aptly replied to Ms. Schakowsky's comment that basically we are trying to look at the science of this. Mr. Chairman, I think it would be appropriate to put by unanimous consent this *Wall Street Journal* article, if you don't mind to put this in. It is--

MR. WALDEN. Without objection.

[The information follows:]

TAB 15

Hockey Stick Hokum

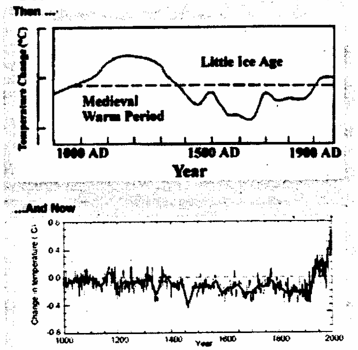
It is routine these days to read in newspapers or hear—almost anywhere the subject of climate change comes up—that the 1990s were the "warmest decade in a millennium" and that 1998 was the warmest year in the last 1,000.

This assertion has become so accepted that it is often recited without qualification, and even without giving a source for the "fact." But a report soon to be released by the House Energy and Commerce Committee by three independent statisticians underlines yet again just how shaky this "consensus" view is, and how recent its vintage.

The claim originates from a 1999 paper by paleoclimatologist Michael Mann. Prior to Mr. Mann's work, the accepted view, as embodied in the U.N.'s 1990 report from the Intergovernmental Panel on Climate Change (IPCC), was that the world had undergone a warming period in the Middle Ages, followed by a mid-millennium cold spell and a subsequent warming period—the current one. That consensus, as shown in the first of the two IPCC-provided graphs nearby, held that the Medieval warm period was considerably warmer than the present day.

Mr. Mann's 1999 paper eliminated the Medieval warm period from the history books, with the result being the bottom graph you see here. It's a man-made global-warming evangelist's dream, with a nice, steady temperature oscillation that persists for centuries followed by a dramatic climb over the past century. In 2001, the IPCC replaced the first graph with the second in its third report on climate change, and since then it has cropped up all over the place. Al Gore uses it in his movie.

Climate 'Consensus'



Source: Intergovernmental Panel on Climate Change

The trouble is that there's no reason to believe that Mr. Mann, or his "hockey stick" graph of global temperature changes, is right. Questions were raised about Mr. Mann's paper almost as soon as it was published. In 2003, two Canadians, Ross McKitrick and Steven McIntyre, published an article in a peer-reviewed journal showing that Mr. Mann's methodology could produce hockey sticks from even random, trendless data.

The report commissioned by the House Energy Committee, due to be released today, backs up and reinforces that conclusion. The three researchers—Edward J. Wegman of George Mason University, David W. Scott of Rice University and Yasmin H. Said of Johns Hopkins University—are not climatologists; they're statisticians. Their task was to look at Mr. Mann's methods from a statistical perspective and assess their validity. Their conclusion is that Mr. Mann's papers are plagued by basic statistical errors that call his conclusions into doubt. Further, Professor Wegman's report upholds the finding of Messrs. McIntyre and McKitrick that Mr. Mann's methodology is biased toward producing "hockey stick" shaped graphs.

Mr. Wegman and his co-authors are careful to point out that doubts about temperatures in the early part of the millennium do not call into question more-recent temperature increases. But as you can see looking at these two charts, it's all about context. In the first, the present falls easily within a range of natural historical variation. The bottom chart looks alarming and discontinuous with the past, which is why global-warming alarmists have adopted it so eagerly.

In addition to debunking the hockey stick, Mr. Wegman goes a step further in his report, attempting to answer why Mr. Mann's mistakes were not exposed by his fellow climatologists. Instead, it fell to two outsiders, Messrs. McIntyre and McKitrick, to uncover the errors.

Mr. Wegman brings to bear a technique called social-network analysis to examine the community of climate researchers. His conclusion is that the coterie of most frequently published climatologists is so insular and close-knit that no effective independent review of the work of Mr. Mann is likely. "As analyzed in our social network," Mr. Wegman writes, "there is a tightly knit group of individuals who passionately believe in their thesis." He continues: "However, our perception is that this group has a self-reinforcing feedback mechanism and, moreover, the work has been sufficiently politicized that they can hardly reassess their public positions without losing credibility."

In other words, climate research often more closely resembles a mutual-admiration society than a competitive and open-minded search for scientific knowledge. And Mr. Wegman's social-network graphs suggest that Mr. Mann himself—and his hockey stick—is at the center of that network.

Mr. Wegman's report was initially requested by the House Energy Committee because some lawmakers were concerned that major decisions about our economy could be made on the basis of the dubious research embodied in the hockey stick. Some of the more partisan scientists and journalists howled that this was an attempt at intimidation. But as Mr. Wegman's paper shows, Congress was right to worry; his conclusions make "consensus" look more like group-think. And the dismissive reaction of the climate-research establishment to the McIntyre-McKitrick critique of the hockey stick confirms that impression.

The Wall Street Journal, July 14, 2006

MR. STEARNS. Thank you. It talked about the hockey stick hokum and it goes on to talk a little bit about Mr. Mann and we all talked about it all morning but it says in 2001 the IPCC replaced the first graph with a second in its third report on climate change and since then this graph has cropped up all over the place. In fact, I think it is in Vice President Gore's movie and I believe it is in his book, "Inconvenient Truth." On page 65 he has got the source as the IPCC and then a little bit above it he talks about the hockey stick, a graphic image representing the research of climate scientist Michael Mann and his colleagues. So I would just say to my colleagues and Ms. Schakowsky to that it is important that if a graph suddenly becomes a significant graph in all these publications and shows up everywhere and is used in debate to make argument, I think it is important for all of us to look at this graph and I think that is all Dr. Wegman is doing is to say we are looking at this graph and as it turns out in this book, "An Inconvenient Truth" by Vice President Gore that he is using a graph as I understand it that has been established this morning that the methodology and the statistical analysis of it is incorrect and--

MS. SCHAKOWSKY. No, that is not--will the gentleman yield for a second?

MR. STEARNS. Well, let me ask--

MS. SCHAKOWSKY. Just for one second.

MR. WALDEN. Just regular order.

MR. STEARNS. I will be glad to do that. Let me just ask Dr. Wegman, if I have in his book the reference to the hockey stick and I have reference to the IPCC, then we have here a graph that you in fact are disputing because of its methodology and the statistics. Would that be a fair statement?

DR. WEGMAN. Well, I would like to be careful in that regard.

MR. STEARNS. Sure. I know. Do you want me to bring the book down and have the staff bring the book to you?

DR. WEGMAN. I have one.

MR. STEARNS. Oh, you have it.

MS. SCHAKOWSKY. Would the gentleman yield--

MR. STUPAK. Would the gentleman yield on that point then?

MR. STEARNS. Well, let me just finish with my question here because what I am trying to understand is, you have a graph that suddenly goes everywhere and we have established today that the methodology for Dr. Mann's graph is questionable, so the question is, if it shows up everywhere, shouldn't the American people understand that some of the reference here in the book, the methodology is in question? That is all I am asking.

MR. STUPAK. Would the gentleman yield on that point?

MR. STEARNS. Well, let me ask--

MR. STUPAK. Because if you are going to ask the question--

MR. WALDEN. Regular order, please. It is the gentleman's time--

MR. STEARNS. I am not asking the question to you. I am asking it to Dr. Wegman, so I think, Mr. Chairman, I would like to have the question asked to him and not to my fellow colleagues.

DR. WEGMAN. Let me be precise on the statement. There is some ambiguity in this book because it talks about ice cores and as I understand it, this particular--

MR. STEARNS. This is on page 65.

DR. WEGMAN. This particular picture--

MR. STEARNS. Yeah, that is right, the same one.

DR. WEGMAN. --was based on ice core studies--

MR. STEARNS. But it says below, it says source, IPCC, at the very little, small little note there.

DR. WEGMAN. Right.

MR. STEARNS. Okay.

DR. WEGMAN. Higher on the same page in the text it talks about Mann but I believe if one is going to be precise, this is a piece of study based on ice cores, not on the temperature reconstruction.

MR. STEARNS. So we just don't know, and I think that is accurate. I am glad you pointed that out so that the reader or anybody looking at this would not necessarily say that the source of the IPCC is indeed Dr. Mann's hockey stick--

MS. SCHAKOWSKY. Would the gentleman yield for just a minute?

MR. STEARNS. No, I am just asking Dr. Wegman--

MS. SCHAKOWSKY. Please, I can read from--I am looking at the same--

MR. STEARNS. You folks had your time. I am just--

MR. WALDEN. Regular order.

MR. STEARNS. When I complete my thing. So the question is, he says IPCC here and he has got this graph that looks like a hockey stick, you are saying that you cannot correlate that to mean that it is Dr. Mann's graph? That is what you are saying?

DR. WEGMAN. I believe that is true.

MR. STEARNS. Okay. All right. Yes, I will be glad to yield to Ms. Schakowsky.

MS. SCHAKOWSKY. Thank you. I just want to read to you from that same--it says "But as Dr. Thompson's thermometer show," and so it is not based on Dr. Mann. This is a different source which our staff had confirmed with Al Gore. I just want to make--

MR. STEARNS. I respect that.

MS. SCHAKOWSKY. --that point. I know, but your question wanted to reinforce the notion that this was based on this false or inaccurate Dr. Mann study--

MR. STEARNS. Well, I think--

MS. SCHAKOWSKY. --and it is not.

MR. STEARNS. Okay.

DR. WEGMAN. And I responded that it was not.

MS. SCHAKOWSKY. No, I--

MR. STEARNS. Go ahead. You respond to that.

DR. WEGMAN. I responded exactly the same way you just did.

MR. STEARNS. And I think that is important to realize because it is showing up not just here but it is showing everywhere and so it is not precise that that is Dr. Mann's graph here, and that is what you have confirmed. Now, I think the other real big question that we sometimes forget is, what effect does this have? I mean, what is--you mentioned here that it could be two degrees Fahrenheit from 1850 to 2006 and you say how many people could know the difference between 72 degrees and 74. That was your words. The Competitive Enterprise Institute put out a report and let me just read from that. Dr. James Hanson of NASA, the father of the greenhouse theory, and Richard Linzen of MIT, both of them are renowned climatologists in the world, agree that if nothing is done to restrict greenhouse gases, the world will see a global temperature increase of about one degree Centigrade in the next 50 to 100 years. Hanson and his colleagues predict additional warming in the next 50 years of .5 degrees Centigrade. A warming rate of .1, tenth of a percent Centigrade per decade, does that seem like an accurate statistic to you? Would you generally agree with that or disagree? I know it is difficult but--

DR. WEGMAN. I have no way of truly knowing.

MR. STEARNS. But I mean, if you say in the last 156 years we have only had two degrees Fahrenheit, I mean, this would confirm that this is not something that is out of control. Wouldn't you say that basically--my point I am trying to establish is, that the estimates of this future warming should not get us into a hysterical mode. I know--

DR. WEGMAN. I would tend to concur but what I would also say is that the global average temperature is probably not a very good measure of global warming in the sense that, as I said before, ocean circulation, salinity, how the Gulf current subducts when it gives up its heat in the Northern Hemisphere, understanding the coupling of that to the atmosphere seems to me to be the scientific issue at hand that really ought to be investigated more thoroughly.

MR. STEARNS. Also in this Competitive Enterprise Institute, the question came up, and Mr. Waxman mentioned a whole group of scientists, renowned scientists, that said that we are into a global warming and in this report it says, “What do scientists agree on and they agree that global average temperature is about .6 degrees Celsius or just over one degree Fahrenheit higher than it was a century ago. Atmospheric levels of carbon dioxide have risen by about 30 percent over the 200 years and carbon dioxide like water vapor is a greenhouse gas whose increase is likely to warm the Earth’s atmosphere.” Is that generally you think accurate?

DR. WEGMAN. As far I know, yes.

MR. STEARNS. But is there in your opinion a scientific consensus that global warming is real and bad for us? Could you say categorically, both you and Dr. North today, that there is a scientific consensus and evidence that global warming is bad and we should be very concerned about it? That is a tough question, I know.

DR. WEGMAN. I believe there is a consensus that global warming is real. My friends in Finland think it is a great thing.

MR. STEARNS. And your friends here in the United States don’t. Would that be fair to say?

DR. WEGMAN. Well--

MR. STEARNS. I mean, that it is occurring but it is not as significant the people that are out there saying we have got to do something tomorrow, we have got to do something, do something.

DR. WEGMAN. I think it is probably less urgent than some would have it be.

MR. STEARNS. Dr. North, I am going to give you a few moments, unless you want to--you don’t have to say anything.

DR. NORTH. Well, my feeling is that it is happening but I don’t do good or bad.

MR. STEARNS. Let me just conclude, Mr. Chairman, just by saying that Dr. Wegman said that in the last 156 years it has gone up just about two degrees Fahrenheit and so I don’t really think we are into a very, very serious concern that we all should be worried about getting overly hot tomorrow.

MR. WALDEN. The gentleman’s time has expired. The gentlelady from Wisconsin, Ms. Baldwin, for 10 minutes.

MS. BALDWIN. Thank you, Mr. Chairman. Dr. Wegman, your report includes a social networking analysis of the authorship in temperature reconstruction, and to your knowledge, has this type of social network analysis ever been done before to look at an academic field?

DR. WEGMAN. No, and in fact, based on reactions to this, I think it is probably a good idea that we do this more broadly.

MS. BALDWIN. And am I correct in understanding that your analysis did not include talking to the paleoclimatologists to get their perspective on how they interact nor did it include substantively analyzing their interactions?

DR. WEGMAN. No. We simply looked at their connection in terms of, based on engineering compendics, based on their coauthorship.

MS. BALDWIN. In your report, you state that, and I quote, "Our findings from this analysis suggest authors in the area of paleoclimate studies are closely connected and thus independent studies may not be as independent as they might appear on the surface." Are you saying that based on your social network analysis, that you are concluding that independent studies may not be independent or are you saying that your network analysis suggests a lack of independence as a hypothesis that one would need to investigate further before one could draw a conclusion?

DR. WEGMAN. I think one should take our social network analysis with a grain of salt to understand that this is an unusual configuration of people with a highly central person involved in this. It is no surprise to any working scientist that there are groups of statisticians, groups of mathematicians, groups of paleoclimate scientists, groups of physicists that work together closely and that there are competing social networks. I would hasten to add that social networks doesn't mean I go out and drink a beer with somebody. It doesn't mean I am a buddy of theirs. It means that I work with them, that I think like they do, that we have similar approaches. Now, if the group of people operating in this area is relatively small, as I believe it is in the paleoclimate area, then I think there is some evidence that probably should be investigated more clearly, that these people are refereeing their own papers. After all, Michael Mann was an editor of the *Journal of Climate* and he publishes a lot of his papers in the *Journal of Climate*. It is pretty hard to say well, I am going to take this guy who is well known and I am going to start rejecting his papers. That is a pretty hard thing to do.

MS. BALDWIN. Well, Dr. Wegman, my question was, is this a hypothesis or is it a conclusion that you have drawn? If it is a hypothesis that would need to be investigated further and of course earlier we heard Dr. North's response to a question about what this--how fiercely competitive people early in their scientific careers, late in their scientific careers are. I am a granddaughter and a niece of two researchers and I feel like I have had a lifelong sense of how competitive these things, even if you have a very narrow perspective. But are you reaching a conclusion or a hypothesis?

DR. WEGMAN. No, this is a hypothesis.

MS. BALDWIN. Okay. Then if I understand you correctly, there are at least two problems with the *Wall Street Journal's* statement in an editorial last week that your "conclusion is that the coterie of the most frequently published climatologists is so insular and so close-knit that no effective independent review of the work of Mr. Mann is likely," because first your social network analysis wasn't of climatologists but a much narrower group of temperature reconstructionists, and second, your social network analysis did not allow you to reach a conclusion about the independence of review of Dr. Mann's work.

DR. WEGMAN. I think that there is--you know, in some sense you are putting words in my mouth but I think there is evidence--

MS. BALDWIN. Well, the *Wall Street Journal*--

DR. WEGMAN. Let me finish. I think there is evidence based on this social network analysis, based on the real climate.org web log, based on the general reaction of Dr. Mann and, for that matter, Dr. Bradley and Dr. Hughes to the initial inquiries to the committee that there is a tight-knit group of people who are interacting with each other and who frankly don't seem to like to be criticized.

MS. BALDWIN. Dr. Wegman, I have an additional question. I think it has been touched on before but I just want to get some real clarity on this. I understand that the data that you used is based on Mann's 1998 and 1999 studies. In the recent years Dr. Mann has altered his reconstructions using different methods and proxies. Each time he has been able to reach virtually the same conclusions. Did you analyze any data from Mann's later studies or those from other reputable climate scientists who have reached similar conclusions?

DR. WEGMAN. We did not attempt to reproduce any of the later material. However, what we did do was look at the proxies that were used and we looked at the series of papers beginning actually with Jones and Bradley, I think it was, in 1993 and compared the proxies that they were using and the methodologies that they were using. Basically Mann articulates I believe in his 2005 paper the set of papers that used the climate field reconstruction, the CFR methodology, and also uses the CPS methodology. Those are articulated by Mann, not by me.

MS. BALDWIN. But you used the 1998 and 1999 studies?

DR. WEGMAN. We were asked to address the issues in 1998 and 1999, yes.

MS. BALDWIN. I would now yield my remaining time to Mr. Inslee, who requested that.

MR. INSLEE. Thank you. Doctor, I have been trying to figure out how to characterize the situation, and the best I can do is to say that we don't debate gravity anymore and we should not debate whether there is a human contribution to global warming anymore, and the way I look at this is sort of like if you had reviewed Newton's Principia where he laid out the basic laws of physics that we have now based, until quantum mechanics came around, most of our science, if you found a statistical flaw, which I will bet you could if you looked at the whole Principia that didn't meet sort of regular statistical proofs right now, you might come into Congress, if the Republicans controlled Congress in 1695, anyway, and say, you know, I found this statistical flaw in this one little piece of Newton's theory, even after we have a mountain of evidence that gravity is a fact, not a theory, upon which we base our science, and that is the reason that you are not urging, as I understand it, us to reject Dr. Mann and his group's conclusion, that humans are a causative factor for global warming. The reason you are not asking us to reject that conclusion is that you recognize that you have found what you believe is a statistical flaw in one study but it does not contravene the mountain of evidence that says global warming is caused a not insignificant part by human activity. Is that a pretty fair metaphor for this?

DR. WEGMAN. Well, I--you know, the issue is, I was asked a very specific question. I came here to testify on a very specific question. And you are asking me to testify off of my level expertise and I--

MR. INSLEE. Well, let me just ask you--

DR. WEGMAN. --am not going to do that.

MR. INSLEE. Let me ask you a quick question. If you found a statistical flaw in the Principia published by Sir Isaac Newton in 16 whenever it was, would you suggest that we reject the theory of gravity?

DR. WEGMAN. I would not suggest anything because that was not the question I was asked and that is not the reason I am here.

MR. INSLEE. Well, unfortunately, this is the reason--

DR. WEGMAN. I mean, if you are asking me as an ordinary citizen--

MR. INSLEE. No, I want you to make sure you understand the reality of the situation. I am giving you all the sincerity that I can give to you. But the reason you are here is not why you think you are here, okay. The reason you are here is to try to win a debate with some industries in this country who are afraid to look forward to a new energy future for this Nation, and the reason you are here is to try to create doubt about whether this country should move forward with a new technological clean energy future or whether we should remain addicted to fossil fuels. That is the reason you are here. Now, that is not the reason individually why you came but that is the reason you are here. Thank you very much.

MR. WALDEN. The gentleman's time has expired, which is the reason I am here to keep control of this.

DR. WEGMAN. But I didn't get to answer.

MR. WALDEN. Well, I will just give Dr. North a question. Does anybody still study gravitational theory in the scientific community?

DR. NORTH. Yes, they do.

MR. WALDEN. If you find--

DR. NORTH. It is a very active field in physics.

MR. WALDEN. Do you ever learn anything new?

Dr. North. Absolutely. Things are being learned all the time.

MR. WALDEN. And are you allowed then to publish new findings that might contradict old findings?

DR. NORTH. Absolutely.

MR. WALDEN. Okay. Good. Science moves forward. Now, I have to apologize. I was in another markup earlier and so I missed some of the questions and some of the opening statements although I am familiar with both of your gentlemen's testimony. But I just want to make sure I understand one sort of underlying piece, and that is, did you both indicate that Dr. Mann's underlying statistical analysis was incorrect? Dr. Wegman?

DR. WEGMAN. Yes.

MR. WALDEN. Dr. North?

DR. NORTH. Well, we found that it is not--there were many choices to make. They probably didn't make the best choice when they did the analysis the way they did.

MR. WALDEN. What do you when--

DR. NORTH. When their claims are wrong, it just means they are not very convincing because of the way they did it.

MR. WALDEN. Okay. Now, I am not a scientist so tell me--

DR. NORTH. That was nuanced. I apologize.

MR. WALDEN. No, no. Tell me what that means as a layperson, as a lawmaker, when you say they made choices in their--

DR. NORTH. Well, when you approach a problem like this, there are many choices when you try to do a statistical analysis and so there are many choices as to should you deter in the data in the 20th Century or should you not. Should you use this kind of validation procedure or a different one.

MR. WALDEN. Right.

DR. NORTH. And in fact, one series of papers by Burger and Cubasch actually looked at the situation and decided there were 64 different ways you could have done it, and had you chosen--and so they actually showed us a family of extrapolations you would have gotten using all of those different--

MR. WALDEN. And did they all look like a hockey stick?

DR. NORTH. They all--well, I mean, to me they do. But, it is a bit curved. It is not exactly like the hockey stick but within the error bars, and by the way, in the *Wall Street Journal* article, there is really a mistake made in that graphic, and that has to do with the error bars. It does show--these two graphics are in our reports, the same ones that are in the *Wall Street Journal* report, and if you look at the *Wall Street Journal* article, they don't put the margin of error in there, which is really important.

MR. WALDEN. What is the margin--

DR. NORTH. I mean, it is totally irresponsible to do this without the margin of error.

MR. WALDEN. Okay. Can I ask you, what should that be so we clarify the record, the margin of error?

DR. NORTH. The margin of error is the plus-minus 95 percent confidence interval.

MR. WALDEN. And that is what it should have been here?

DR. NORTH. That is right.

MR. WALDEN. The plus or minus--

DR. NORTH. And so when you look at the family of curves, they all fall pretty close to that gray area in this graphic but in the *Wall Street Journal* article, the gray is removed.

MR. WALDEN. Now, in the *Wall Street Journal* article too, they make a reference to a McIntyre and McKitrick critique, and I guess, have you reviewed that one, Dr. North

DR. NORTH. Oh, I am familiar with their work and, in fact, Mr. McIntyre is here. He will be testifying later.

MR. WALDEN. Did he present to your panel?

DR. NORTH. Yes, he did. And in fact--

MR. WALDEN. Can their data be replicated or the results be replicated?

DR. NORTH. Well, what they did was a critical study, somewhat like the Wegman report, and I think they did an honest job. It was a nice piece of work.

MR. WALDEN. Dr. Wegman--

DR. NORTH. I have no complaint about what they did.

MR. WALDEN. In terms of replicating data or replicating studies, my understanding is, it is difficult to replicate the Mann study but it was possible to replicate the McIntyre and McKitrick study.

DR. WEGMAN. Yes, that is correct, and we did so.

MR. WALDEN. I want to move on to a little different topic and that is related to data sharing because I have run into this in another committee where I am a subcommittee chair on science and that was, there was a dispute--imagine that--over a report that was run out and published and somebody else tried to get the data to see if they could replicate it and there was a long delay and it was a real problem, and I know Dr. North, in your report, you say--page 112 of the surface temperature reconstructions the past 2,000 years, you make a comment that says, "Our view is that all research benefits from full and open access to published data sets and the clear explanation of analytical methods is mandatory. Peers should have access to the information needed to reproduce published results so that increased confidence in the outcome of the study can be generated inside and outside the scientific community," and you make that comment. Then I note--

DR. NORTH. I was about to read it to you.

MR. WALDEN. What is that?

Dr. North. I was about to read it to you.

MR. WALDEN. Well, we can do it in the key of C next time together. Then Dr. Wegman, on page 4 of your testimony, you say, "Additionally, we judge that sharing research materials, data, and results was haphazardly and grudgingly done," and further I believe it on page 66, there is a reference--there is a question, "Has the information needed to replicate their work been available, and the answer is, in our opinion, no. As mentioned earlier, there were gaps in MBH98." Do we have a situation here where it was very difficult to get the data to do replication, and if so, why, do you think?

DR. WEGMAN. As I mentioned earlier, we did download the data. We have seen the letter that Dr. Mann replied to the committee which basically took the position that this is my intellectual property and I don't have to share it and the National Science Foundation tells me so.

MR. WALDEN. Is that the case, Dr. North? Do you speak for the National Academy of Science?

Dr. North. No, no.

DR. WEGMAN. But the issue is that if there is free and open access to the data and the materials that are associated with the data, it makes the policing of this kind of activity, the referees for the hockey game as I said earlier, it makes it so much easier to be able to do that, and we think that that is an important aspect of the scientific enterprise.

MR. WALDEN. How do statisticians do these sorts of evaluations? Do you share data among yourselves?

DR. WEGMAN. Typically in terms of computer code, there are two places that people typically go to. There is an electronic journal called the *Journal of Statistical Software* which is a refereed journal. People submit their code to that journal. There is also a website that people submit both data and code to.

MR. WALDEN. I don't know if you have had a chance to see Mr. Crowley's testimony whom we will hear from later today but he has some rather unflattering statements about your report. I know it is shocking that different scientists have different views of different scientists and their reports. He says that there are a number of flaws in your report and goes on to list some. Do you have any comment on the testimony we are going to hear later since you won't be back at--

DR. WEGMAN. Well, I probably will be here but not sworn in or at least--

MR. WALDEN. Right. You will still be under oath, they inform me.

DR. WEGMAN. I understand where Dr. Crowley is coming from. He is in a relatively awkward position of having to defend the position that Dr. Mann had taken.

MR. WALDEN. Why? Why is that an awkward position?

DR. WEGMAN. Well, because you have heard from both of us this morning that there are fundamental flaws in the Mann work and to come and have to defend that is an awkward situation, I think. Frankly, I would not have wanted to get the letter that Dr. Mann got and the other coauthors because that is kind of not on the radar screen of typical scientists. You know, you write a paper and you have a file somewhere and right now my dean is telling me that we should throw everything that is more than 3 years old, we shouldn't keep it in the file drawers because we have space considerations, we have to keep space, but I--you know, I think I jotted down the phrases he used about me which is that I am naive and--I think it was naive and uninformed. I don't think those are accurate statements because he has never talked to me either. He has only read what we wrote and he has read it without the interaction with us as statisticians so we will see what happens this afternoon.

MR. WALDEN. Is he a statistician, do you know?

DR. WEGMAN. Not that I know of.

MR. WALDEN. You made a comment about the potential conflict with Dr. Mann being an editor of a journal and also submitting work to that journal.

DR. WEGMAN. Yes.

MR. WALDEN. Do you know if he proofs his own work or does he hold himself--

DR. WEGMAN. Generally the process is that an editor of a journal will submit it, pass on the material to an associate editor who will in turn select some referees. That process is typically what happens in a journal. When I was editor of a journal, I refrained from submitting anything to the same journal that I was editor of simply because it puts pressure on the associate editors and referees to approve.

MR. WALDEN. Gentlemen, we appreciate your testimony and I will go to the full committee Chairman, Mr. Barton.

CHAIRMAN BARTON. Well, I don't want to do a second round because we have subjected these two gentlemen to close to--what is it--four hours of dialog. I would want to--I want to ask unanimous consent to ask Dr. North to comment on the recommendations that Dr. Wegman gave and I also want to renew my request that the Enders Robinson e-mail be put into the official record.

MR. STUPAK. Mr. Chairman, as to the e-mail of Robinson, I have no problem with that being entered in the record, but if you are going to ask further follow-up questions, I know there is one two further follow-up questions on this side we would like to ask.

[The information follows:]

TAB 14

Spencer, Peter

REDACTED

From: Yasmin Said [ysaid[redacted]
Sent: Tuesday, July 18, 2006 6:54 PM
To: Spencer, Peter
Subject: FW: Re: Upcoming Congressional Testimony

REDACTED

>From: endersrobinson[redacted]
>To: "Edward Wegman" <ewegman[redacted]

REDACTED

Email Addresses
Redacted

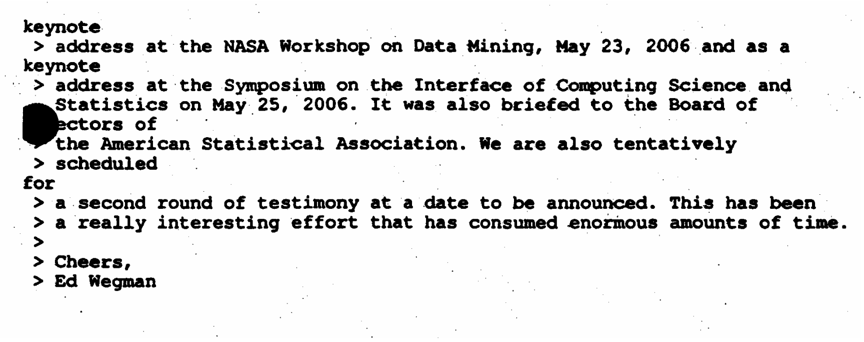
>Subject: Re: Upcoming Congressional Testimony
>Date: Tue, 18 Jul 2006 22:46:15 +0000
>
>July 17, 2006
>
>To whom it my concern:
>
>I have read the report to Chairman Barton and to Chairman Whitfield entitled AD HOC COMMITTEE REPORT ON THE "HOCKEY STICK" GLOBAL CLIMATE RECONSTUCTION by Edward J. Wegman, David W. Scott, and Yasmin H. Said. In what follows this work of Wegman, Scott and Said is simply referred to as the Report.
>
>The assessment of previous results given in the Report is correct. The Report is entirely correct in stating that the most rudimentary additive model (the model of a simple temperature signal with superimposed noise) is not adequate to describe the complex relationships involved in climate dynamics. There is no physical process found in nature that does not involve feedback in one form or another to regulate the action of the system. The statistical methods and models described in the Report use more variables and make possible the construction of more elaborate reconstructions that allow feedback and interactions. The Report represents the correct way to proceed. It is especially important to bring the professional statistical community into the picture in order to assure that a sound analytical foundation secured in the continuing development of this program.

>What has gone on in the past studies is not unusual in science. Many approaches are taken, nearly all of which contribute to understanding. The present Report represents a turning point, which was sure to come

>at one point or another. The turning point results in an assessment of
>past results, and the inclusion of workers from other disciplines to
>fill in gaps and bring the entire endeavor up to the highest scientific standards.
>We are indebted to the present Report for taking this step.

[illegible]would like to mention that the authors of the Report have long
>experience in the construction of statistical models for environmental
>studies. No research workers are better qualified in every sense than
>they are to deal with the problem at hand.
>
>Sincerely,
>
>
>Enders A. Robinson
>
>Professor Emeritus in the Maurice Ewing and J. Lamar Worzel Chair
>Department of Earth and Environmental Engineering Columbia University,
>New York, NY 10027
>
>Member of the National Academy of Engineering of the USA
>
>Fellow of the European Academy of Sciences
>
>
> -------------- Original message ----------------------
>From: "Edward Wegman" <ewegman[redacted]
> > Dear All, Email Redacted
> >
> > As many of you know, last September I was approached by a
> > Congressional staffer concerning doing an independent review of some
> > statistical
>issues
> > related to the so-called "Hockey Stick" temperature profile that was
[illegible] published in *Nature* in 1998 and featured prominently in the
[illegible] Intergovernmental Panel on Climate Change's *Climate Change 2001:
> > The Scientific Basis.* This U.N. report was used widely to support
> > the Kyoto Accord. The "hockey stick" graphic was also featured in Al
> > Gore's film,
>*An
> > Inconvenient Truth. *Along with Drs. David Scott and Yasmin Said, I
> > undertook to investigate the mathematics/statistics associated with
> > the "hockey stick" papers. John Rigsby, a recent graduate with a
> > M.S. in Statistical Science and a Ph.D. student in the College of
> > Science, and Denise Reeves, a Ph.D. student in the Volgenau School
> > also contributed
>to
> > our report. The incovenient truth is that the hockey stick is an
>artifact of
> > a misuse of statistical methodology. Our report was released by
> > Congress this morning and is available online at
> > http://energycommerce.house.gov/108/home/07142006_Wegman_Report.pdf.
> > The House Committee on Energy and Commerce press release is
> > available at
> > http://energycommerce.house.gov/108/News/07142006_1989.htm. A fact
>sheet is
> > available at
> > http://energycommerce.house.gov/108/home/07142006_Wegman_fact_sheet.pdf.
> > Finally, our report was the subject of a Wall Street Journal
> > editorial
>this
> > morning, which is available at
> > http://energycommerce.house.gov/108/News/07142006_1990.htm.

[illegible] Our testimony is scheduled for 10:00 am on July 19, 2006 and wil be
> > on
>the
> > web live and other media. The essence of our report was given as a



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keynote
 > address at the NASA Workshop on Data Mining, May 23, 2006 and as a
keynote
 > address at the Symposium on the Interface of Computing Science and
   Statistics on May 25, 2006. It was also briefed to the Board of
[illegible]ectors of
   the American Statistical Association. We are also tentatively
 > scheduled
for
 > a second round of testimony at a date to be announced. This has been
 > a really interesting effort that has consumed enormous amounts of time.
 >
 > Cheers,
 > Ed Wegman
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CHAIRMAN BARTON. I am sorry. I got the first part. I didn't get your second part.

MR. STUPAK. I said there are one or two follow-up questions--

CHAIRMAN BARTON. Oh, you all have some follow-up? Okay. Could I be recognized then for 5 minutes? Could we do the second--

MR. STUPAK. No objection.

CHAIRMAN BARTON. Five minutes so that we can let this panel go.

MR. WALDEN. The Chairman is recognized for 5 minutes.

CHAIRMAN BARTON. Thank you. Dr. North, Dr. Wegman makes four recommendations on page 6 of his testimony. Do you have that in front of you?

DR. NORTH. I think I have copied them out of there so I think I have them here, yes.

CHAIRMAN BARTON. Could you comment on each recommendation, whether you think his recommendations have merit?

DR. NORTH. Let--I will try to do that. So recommendation one was when massive amounts of public monies and so forth are at stake, academic work should have more intense level of scrutiny and review. Well, nobody would argue with that, of course. It is especially the--we always want to do things better. It is especially the case that authors of policy-related documents like IPCC and so forth should be--

CHAIRMAN BARTON. He says the review should not be the same people that constructed the academic paper.

DR. NORTH. So that is a really very interesting question and subject. You know, when you ask for an expert scientific review of the state of art or the science and you go to the world experts, and that is what the IPCC tries to, you will find authors of the chapters who have also coauthored some of the papers involved and indeed I think sometimes they do promote their own work. That is human nature. We all know how that works. So that process isn't exactly perfect, but I cannot imagine a better, more efficient way to pull several thousand scientists together and they have to meet repeatedly several times over the course of a year, over the course of a couple of years. One time we actually had one of the meetings in College Station some years ago and so people get tired of this. It is really hard to work. I mean, it sounds like it is fun but--

CHAIRMAN BARTON. To the largest extent possible, if you can--

DR. NORTH. So it is very, very hard to--

CHAIRMAN BARTON. Sometimes there is not but two experts in the world and so, you know--

Dr. North. That is right.

CHAIRMAN BARTON. But if it possible--

Dr. North. So, you know, you could go another way and ask a situation like the academy did. We had a small committee of 12 people who were picked on the basis that they were not connected with any of the--I mean, as little as we could possibly do, connected with any of the principals and the problem, so--

CHAIRMAN BARTON. But what about his recommendation number two that there should be a more comprehensive and concise policy on disclosure and that data collected under Federal support should be made publicly available?

DR. NORTH. This is not a bad idea, and in fact, I think Tom Karl is going to address that.

CHAIRMAN BARTON. And then his recommendation number three is that if you are doing review and doing studies that include some sort of a statistical approach on which your conclusions are based, that there should be statistical evaluation of the statistical practices. He says it should be a mandatory part of all grant applications.

DR. NORTH. I think that is a little over the top. I think--

CHAIRMAN BARTON. A little over--

DR. NORTH. I think carrying this to the Federal drug approval process is--

CHAIRMAN BARTON. So you would--

DR. NORTH. It is not a good analogy.

CHAIRMAN BARTON. Well, what about his last one, that emphasis should be placed on the Federal funding of research related to fundamental understanding of the mechanisms of climate change. I think you would accept that. And that the funding should focus on interdisciplinary teams and avoid narrowly focused discipline research, and he is trying to broaden the field so that it is not the same group of people talking to the same group of people.

DR. NORTH. Well, it seems to me the two statements are contradictory. The first one says you should narrow the field and the second one says you should broaden the field, so, I mean--

CHAIRMAN BARTON. You are not real fired up about--

DR. NORTH. I want to see more money come into the field. I think we all would like to see that. That is great. But I am not sure that one was very well formed out.

CHAIRMAN BARTON. Well, let me before I yield back my 52 seconds say why we are doing this hearing, because I have been here almost the entire time for every question and every statement. I missed a little bit but not much. I don't disagree fundamentally with some of what my friends on the minority side have stated. There is no question that the temperature is warmer today than it was in 1850. I think there still is a question about the cause of that, and some of these reports and studies that purport not only to state the fact of the warming but the consequences of it, I think should be open to honest public debate without challenging the merits. Where I disagree with some of my friends on the minority side is that before we make massive public policy changes that affect every American citizen in this country, we need to have with the highest degree of certainty that the facts really are the facts. Now, I have right here a magazine article from *Newsweek* April 28, 1975, that is talking about the cataclysmic consequences of global cooling. Now, that is 30 years ago and the science has changed. Now we are talking about the cataclysmic consequences of global warming. If the United States has ratified Kyoto and if the United States Congress working with the Administration had begun to implement Kyoto, it requires a reduction in CO_2, I believe about 30 to 40 percent, and that means you are not going to have coal-fired power plant combustion in many parts of this country. It means that you are going to have to reduce the automobile emissions of the vehicles that are made in Michigan. And before we go down that trail, I think it is imperative that we do the oversight and do the science and talk--I am not opposed to talking to the climatologists but I agree with Dr. Wegman that we need to make sure that it is an interdisciplinary approach so that we really get everything on the table. If that shows that the human correlation is beyond dispute, then I believe we do have an obligation to take what steps we can to remedy that but I don't believe that science yet shows that. With that I yield back, Mr. Chairman.

MR. WHITFIELD. Thank you, Mr. Chairman. At this time I recognize Mr. Stupak for--

MR. STUPAK. Thank you, Mr. Chairman. Chairman Barton, and I take it, that means we are going to have a lot more hearings on global warming because there are a lot more reports than just Dr. Mann's 1998-1999 report, so if we are going to come to those policy decisions, I would hope we would have more than just one hearing about one report and look at the whole spectrum of reports on global warming. Dr. North, if I may, the IPCC process, is that based upon sound science, sound methodology?

DR. NORTH. In my opinion, when you go out and ask the active scientists in the field to give you an assessment, they select themselves and it has been my experience in the three that have been produced that they do just that. I had very little to do with the last one. I served a referee on--

MR. STUPAK. Sure.

DR. NORTH. But the one before, I had a little bit more to do with it, but I think the process is pretty good. You know, it is human. It has some flaws in it but I think I--it is probably the most massive assessment of this kind that has ever been made. It is remarkable that you get people to do that. And I will tell you this, people are tired of participating. It is a lot of work. Traveling to these countries and having these workshops and meetings, it is a lot of work and so to actually ask people who are not experts to come in and read all of those papers that they weren't involved in, that is asking a lot of people and you won't get anybody to do it because there is no money for this. There is no pay for this.

MR. STUPAK. Okay.

DR. NORTH. Incidentally, the academy report people didn't get paid anything either.

MR. STUPAK. Dr. North, you also mentioned the hockey stick hokum that was in the *Wall Street Journal* last Friday in which they claimed that the graph from 1990 that we have talked a lot about today showing the warming period in the Middle Ages, the *Wall Street Journal* goes on to say that in 1990 the consensus "held that the medieval warm period was considerably warmer than the present day." It has been a long hearing here today but is there any scientific evidence from anyone that supports the claim that temperatures in the Middle Ages were higher than they are today?

DR. NORTH. There may be some locations on the Earth but so why do we care about the global average? You know, that has come up a couple of times. Because if CO_2 is the reason, it is a global forcing so you expect the response to be at the global scale. This is really important. That is why--I mean, nobody takes a picnic at the global scale but the scientists are very interested in what happens to the global average because that is what is being forced by the CO_2. So that is why we are so fixated on the global average and getting large-scale averages. It is easier to measure it because when make measurements at a lot of locations, a lot of the random errors cancel out. That is good. The same thing happens with our models. They do that better than anything else.

MR. STUPAK. Dr. Wegman, I thought I heard you say, and correct me if I am wrong, when you are making comparisons you are saying that you used--I think it was figure 4 on your chart--that you used North American factors in your analysis with Dr. Mann's?

DR. WEGMAN. Dr. Mann himself used North American--what he called the North American PC1 proxy which was a composite based on the principal of component methodology of North American tree rings.

MR. STUPAK. Sure.

DR. WEGMAN. And that is what--we replicated that, yes.

MR. STUPAK. So in your analysis, you used just North American, right?

DR. WEGMAN. We used the North America proxy.

MR. STUPAK. The P1, the P2--

DR. WEGMAN. The PC1--

MR. STUPAK. --and the P3?

DR. WEGMAN. Yes.

MR. STUPAK. PC. I am sorry. PC1, PC2. But didn't really Dr. Mann use 12 proxy indicators from all over the world?

DR. WEGMAN. We were not trying to do paleoclimate reconstruction. We were trying to illustrate what happened if you did--

MR. STUPAK. Sure.

DR. WEGMAN. --the principal component--

MR. STUPAK. Dr. Mann used 12 proxies to come up with his analysis. You took three from North America. Is it fair to say then that using from throughout the world would have a different result than if you just looked at the three in North America?

DR. WEGMAN. Let us be clear. He was doing Northern Hemisphere, NH, reconstruction. He wasn't doing global reconstruction in--

MR. STUPAK. But if you take a look at his report, and I know you did, they talk about Tasmania, taking tree rings from there, Morocco, tree rings from there, France, the Greenland stack core which we talked about, the ice core, polar Urals, again, the tree ring density. It seemed to me he took them from all over the world where your focus is only on North America. So how could you make the comparison then when you use global statistics as opposed to just one part of the world in doing your measurements?

DR. WEGMAN. Well, I am not sure I understand what you are getting at. The--

MR. STUPAK. From a layperson who is not a statistician, I would think if you are going to compare Dr. Mann's statistics, if you will, you would use all of them as opposed to--

DR. WEGMAN. Our discussion--

MR. STUPAK. --just three of them.

DR. WEGMAN. Our discussion is on Dr. Mann's methodology, not his conclusions in terms of paleoclimate--

MR. STUPAK. But you charted, did you not? Didn't you use X axis, Y axis and chart it all out and that is why you got different than the hockey stick? You only used three where he used 12.

DR. WEGMAN. No, no, no. We used the same data to get the hockey stick in that one figure--

MR. STUPAK. From North America?

DR. WEGMAN. From North America.

MR. STUPAK. And he took his from the worldwide.

DR. WEGMAN. No, no, no.

MR. STUPAK. That is not what table one says.

DR. WEGMAN. What we said was that we used that comparison chart that we had that showed the hockey stick. The comparison was meant to show that if--

MR. STUPAK. Right here, yes?

DR. WEGMAN. That is it. If you go to the top chart by using his methodology on the same set of data and the bottom chart is what you would get if you did the centered data, if you did it properly mathematically. So the point--

MR. STUPAK. But yours is only on PC1, PC2--

DR. WEGMAN. So is his--

MR. STUPAK. --and PC3.

DR. WEGMAN. --in that picture.

MR. STUPAK. So you are saying that picture was only PC1, PC2--

DR. WEGMAN. That is--

MR. STUPAK. --PC3 from Mann.

DR. WEGMAN. We are using exactly the same data in the top picture and the bottom picture.

MR. STUPAK. Okay. Thank you.

MR. WHITFIELD. The gentleman's time has expired. Dr. North, in the testimony today, there seems to be universal agreement that the temperature is going up and in the last century it went up about one degree Fahrenheit, I believe is what most people have agreed to, and there has also been a lot of testimony that for a period of time between 1500 to 1800, whatever, that there was a period in which there was a cooling off. So I just want to zero in on this. You have said and others have said and I think there is universal agreement that we are going through a warming trend, and it has been said by some people that that might not be surprising coming from a cooling off period that you would normally get warmer going through a warming trend. So the question that I would ask, as you look into the future, how much warmer can it become before it is something that we should really be alarmed about from your viewpoint, from experiences?

Dr. North. Well, I will say this--well, two things. One is about the Little Ice Age and is it simply a recovery. In other words, is the Earth's temperature a kind of oscillating thing and that the slope upward now is just recovery from a Little Ice Age which was apparently maybe some natural phenomenon. Well, I am not sure that that is actually the right picture. We don't know exactly the true origins of the Little Ice Age but some studies, in fact, a very good one by Tom Crowley, who will be speaking later, suggests that this is due to a series of volcanoes during that period which caused a cooling. It was not a great cooling but some cooling. So now it is--you know, now that we are going through a period when they are not as frequent as they were at that time, the Earth is simply warming back toward equilibrium from that. But now we are also forcing the warming with the CO_2 and other greenhouse gases that are being emitted into the atmosphere. So while if we look at the future, what we might think is that by the end of this century the warming, if it continues and we do nothing about it, will probably be somewhere between about three degrees Fahrenheit and about eight degrees Fahrenheit. Well, three may not be so bad. Eight would be pretty bad, pretty bad. And so in fact, even three is not as benign as you might think. You know, you can look at--for us in our everyday life, three degrees Fahrenheit doesn't seem to mean anything. People after all live in Minneapolis and they live in Houston. But it really does affect conditions. Tree lines move. There is a tree line that runs right up the center of the United States along I-35 between Austin and Minneapolis.

MR. WHITFIELD. Right.

DR. NORTH. That tree line can move hundreds of miles depending on just a couple of degrees or changes in moisture. So what looks like to us in our everyday life not very much, if these things persist for a long time, there are broader ecological responses at these kind of low frequencies that are important. So, I don't know all of the bad or good things that might happen. I mean, there would probably be some winners and losers in a situation like this. And I have to confess to you, I don't know enough about it.

MR. WHITFIELD. But you know as we grapple with this, we have like a 250-year reserve of coal in America. We all want to be less dependent on foreign oil. There are some people that don't want to use fossil fuel at all, it would be better to come up with new innovation, new technology and move on to something cleaner and that can be a goal of ours. In representing a coal area of the country, I have a lot of constituents who come up to me and they will say well, sure, there is some carbon dioxide caused by human beings but there is more carbon dioxide emissions caused by natural processes. Now, I would just like to get your views on that comment. Is there any basis for that or is that just somebody--

DR. NORTH. There is a lot of carbon dioxide emitted into the atmosphere every year and a lot of absorbed back into the system every year, in fact, many times what humans put in. The problem is this. There was an equilibrium established between what is going out and what is drawn back down every year by the system. The oceans and the biosphere, there is this exchange that goes on all the time. The problem with this is that the time scale, the time constant, as we say, is quite long. It takes a couple of hundred years for these adjustments to re-establish themselves, so if you dump in the carbon dioxide much more rapidly than the system can accommodate, it builds up in the atmosphere. If we were to wait several hundred years, then things may come back down, but we don't have that luxury. So the fact is, we are pouring it in there faster than the system can dispose of it. That is the way--

MR. WHITFIELD. Thank you very much. Who is next over here? Mr. Inslee.

MR. INSLEE. Thank you. Just on that note, Dr. North, I have heard the CO_2 that we put in today in the atmosphere could be there as long as 100 years?

DR. NORTH. A couple of hundred years.

MR. INSLEE. I want to use Dr. Wegman's expertise to try to understand an interesting phenomenon. You talked about social networking. I thought you could give us some insights about that. Dr. Naomi Oresky of the University of California at San Diego published a study in *Science* magazine some time ago. She and her team selected a large random sampling of 928 articles about global warming that have been published in peer-reviewed scientific journals and she wanted to look at what they said, these 928 randomly selected peer-reviewed articles about whether they accept or reject or question the idea that humans are contributing to global warming. Of 928 studies, what do you think percentage questioned the proposition or rejected or even cast doubt on the proposition that humans were causing global warming? What do you think, Dr. Wegman? What percentage? Zero. Zero percentage of the scientifically peer-reviewed articles drew the same conclusion that my good friend Joe Barton drew, that there is doubt about this. Zero. Now, my question is, another study looked at 636 randomly selected articles about global warming chosen from the *New York Times*, the *Washington Post*, the *L.A. Times,* and the *Wall Street Journal*. Of those randomly selected publications and those well-respected publications, what percent cast doubt as to the cause of global warming? What do you think?

DR. WEGMAN. Probably about 50 percent.

MR. INSLEE. Fifty-three percent. You win the prize for the day of closest guess, or as you say, estimation. Over half of the popular articles suggested there is a significant question as to whether or not humans are contributing to global warming but zero percentage of the peer-reviewed science. Now, I believe that is one of the reasons that Congress has not acted on this because frankly, the press is creating doubt where there isn't any. So the question of a social scientist, the social networks, do we have a problem with the press that are hanging out in the bars all together too much too like the climatologists or what is your explanation for this huge anomaly?

DR. WEGMAN. Well, there is no doubt in my mind that there are two camps in the publication literature as well in the popular press and, they are competing just like I suggested that academics compete in social science that there is two networks that are trying to promote different agendas.

MR. INSLEE. Well, my point is, I hope the press starts to get off the story of doubt and get on the story of a scientific consensus which exists in those 900 articles, and no one should report this hearing unless they say that because both you and Dr. North and every single person who is going to testify today is going to say that there is a scientific consensus that humans are responsible for at least a portion of the global warming that is taking place. Now, I want to ask Dr. North if we can put this slide up here about the CO_2 and go back to the one he had there just a moment ago. Dr. North, I gave some of a very inarticulate description of how carbon dioxide works to trap energy in the planetary system. Could you give a little better explanation? We will see that all the scientists, everybody has projected levels of approximating double of pre-industrial times if we don't change our course. Could you explain in a little better way how carbon dioxide affects the energy balance of the Earth?

DR. NORTH. I will try. First of all, carbon dioxide is well mixed in the atmosphere so it isn't just lying down on the surface. It is very well mixed. This process takes a few months but--and in fact, if you emit it in one hemisphere of the Earth, it takes about a year or two before it homogenizes throughout the world. So whether you emit your gas, your CO_2 in Texas or anywhere else, it doesn't make any difference. It winds up homogeneous throughout the world. So what happens now? So the sunlight comes in, passes right through the CO_2 and warms the ground. The ground in contact with the atmosphere through latent heat release, that is, evaporation from the surface and just sensible heat convection to the surface warms the atmosphere. So and then we establish an equilibrium because the radiation going out to space matches exactly what comes in over a long-term average. So that is the energy balance of the Earth. Now, suppose you turn up the carbon dioxide a little bit in the atmosphere. Well, one thing that happens is, since the gas homogenizes all through the planet, all around the planet. The level up in the atmosphere where the CO_2 emits to space goes up a little bit and higher in the atmosphere, 50 meters or something like that if you double it. That means it emits from a cooler place in the atmosphere once you have doubled it. That means the amount going out isn't as much as it was before. So what happens is, you have to warm the surface in order to regain the equilibrium. That is a complicated explanation. But in the process right in the middle of this, you warm the planet a little bit, more water comes into the atmosphere from the oceans and other wet surfaces. Water vapor is also a greenhouse gas so this process gets amplified maybe a factor of two. So basically, I mean, what you said about the blanket is more or less right. A slightly more technical discussion is well, when you put in more of this stuff, it now emits from a higher place from a cooler surface rather than a warm surface so the radiation out to space is less, you have got to warm up the planet to match again. Sorry for such a long-winded answer.

MR. WHITFIELD. Thank you. The gentleman's time has expired. Mr. Walden.

MR. WALDEN. Thank you very much, Mr. Chairman. Dr. North, what are some of the biggest natural emitters of CO_2?

DR. NORTH. Well, there are many. Decaying biological matter is one, so rotting, decaying at the floor of the great forests and all over the planet, respiring animals and so forth. So there are many--

MR. WALDEN. What about forest fires?

DR. NORTH. Forest fires contribute but not nearly as significant as these other natural products, and also volcanoes of course emit CO_2 but on our scale, I mean, that is sporadic. It does happen from time to time and of course it is the historical origin of CO_2 in our atmosphere but--

MR. WALDEN. And what consumes--

DR. NORTH. --it is not important.

MR. WALDEN. What consumes CO_2?

DR. NORTH. So what consumes CO_2 is the biological matter, the photosynthesis process, so sunlight is combined with--

MR. WALDEN. Plant matter--

DR. NORTH. --chlorophyll in the plant leaves and that is converted to--so it removes CO_2.

MR. WALDEN. So younger, healthier plants and trees consume more CO_2 than older, dying--

DR. NORTH. As they grow, they consume. Right. You are making wood with the carbon.

MR. WALDEN. Because I also in my other part in the Congress chair the Subcommittee on Forests and Forest Health and we see--

DR. NORTH. That is very important.

MR. WALDEN. --these overgrown, decaying and dying forests. We see fires occur that emit far more than CO_2. They emit a lot of other noxious gases. They have--

DR. NORTH. Sure.

MR. WALDEN. You know, the smoke will settle on the valleys. I mean, it causes all kinds of problems and then the decaying matter sits there for 3 or 4 years rather than being processed and a new forest planted sooner. Are you aware of any research that would indicate that by planting sooner, getting a healthy forest a start faster, you might begin consuming carbon quicker than just leaving it to regenerate naturally?

DR. NORTH. Well, you are getting way off from my field but, intuitively, yes.

MR. WALDEN. All right. Dr. Wegman, in your report, it is page 27, you say a common phrase among statisticians is correlation does not imply causation, and you go on to say the variables affecting Earth's climate and atmosphere are most likely to be numerous and confounding, making conclusive statements without specific findings with regard to atmospheric forcings suggests a lack of scientific rigor and possibly an agenda. What do you mean by that?

DR. WEGMAN. Well, as we--when we were talking about tree ring growth, for example--

MR. WALDEN. Right.

DR. WEGMAN. --there are many, many factors. Moisture as well as--

MR. WALDEN. Carbohydrates. Right.

DR. WEGMAN. And nitrates, for example, that are emitted into the atmosphere. All of those affect tree ring growth.

MR. WALDEN. Can you pinpoint temperature in a tree ring?

DR. WEGMAN. Well, presumably there is some element of that. I am not an expert on tree ring dendrology but presumably all other factors being equal, if things are warmer, there is more sunlight, there is a longer growing season, presumably the trees are going to have wider tree rings. So the issue though is the confounding factors. If you simply say that this tree ring growth, what is called the late wood density, is higher, that means the temperature is higher and ignore all the confounding factors, you are certainly not teasing out what really is the temperature.

MR. WALDEN. Now, we have seen the slide a couple of times from my colleague from Washington, Mr. Inslee, that shows CO_2 levels back 160,000 years. Can either of you tell me, how do we know with precision what happened 160,000 years ago?

DR. NORTH. Would you like me to--

MR. WALDEN. Sure. Maybe from you, Dr. Wegman, statistically, what does that mean and how do you evaluate it, and Dr. North, from you maybe, the science behind--

DR. WEGMAN. Well, we have read actually Bradley's work on this material so essentially when snow gets deposited, it gets compressed, ultimately it becomes a second layer called a firn, f-i-r-n, and then ultimately ice and when the snow gets compressed it has ice, so it has bubbles of air in there and presumably what is happening is that as they drill ice cores down and go further into the past, presumably 160,000 years of ice, they can look at these microscopic bubbles of air and get the greenhouse gas composition associated with that. So that is again a statistical estimation process--

MR. WALDEN. Are you comfortable with that process as a statistician, not as a--

DR. WEGMAN. Well, presumably that curve that we have seen a couple of times from Congressman Inslee should have error bars as well associated with it.

MR. WALDEN. Should have what?

DR. WEGMAN. Error bars associated with it, imprecision, how much variability there is.

MR. WALDEN. And do we know what that would be? I guess he has left. So we are--it is much like the criticism Dr. North had of the *Wall Street Journal* report where it lacked the 95 percent--

DR. NORTH. Yes, we would like to see those error bars. That is very--

MR. WALDEN. Yes, we would like to see it as politicians in our polls too to know, what plus or minus are we dealing with here. Thank you, Mr. Chairman. Thank you, gentlemen.

MR. WHITFIELD. Ms. Schakowsky.

DR. NORTH. I would like to point out that we now actually can go back 650,000 years. Six hundred and fifty thousand years in Antarctica in the past year.

MR. WALDEN. With precision?

DR. NORTH. Just not 150 but 650,000 years, still no CO_2 at this level.

MR. WALDEN. There is still no what?

DR. NORTH. No CO_2 at this same concentration.

MR. WALDEN. I see. Thank you.

MS. SCHAKOWSKY. I wanted to explore a little bit the statement that Chairman Barton made. He was referring to, I think it was 1975 or sometime in the 1970s when apparently there was a prediction of cooling, that actually the planet was getting cooler, and here is my question, and maybe I am not asking the right one and you could fill me in on that. But could not--and he used it, I thought, as making the point that science is not conclusive. But I am wondering if one could not also see it as a confirmation that human activity is in fact causing fairly dramatic change in the climate, something that may not have been factored in in 1975 but the science based on sort of older predictors. So I just wanted to ask how to interpret--and first of all, is that the case that it was predicted to be a cooling period? Let me ask Dr. North, the climate scientist, first.

DR. NORTH. Yes, there was a prediction made in the 1970s by Reed Bryson, a professor at Madison. He probably gave us all a hard time about this because I have heard this a thousand times in the last year or so, few years. So but, there are two competing factors. There is the dust in the atmosphere, the tiny aerosols, tiny droplets of water and they come from air pollution and volcanoes and other things but mainly air pollution in our urban areas, manufacturing processes and so on. So out come these tiny droplets. Well, they scatter the sunlight back to space and therefore tend to cool the planet a little bit. The other competing factor is the greenhouse gases. They have been rising, and especially during the war when there was a lot of energy produced and not very much regulation on what was allowed to go into the atmosphere. At that time there was actually--the aerosols were kind of winning the war, winning the war of balancing the heat in the atmosphere, so there was a cooling that did occur and probably Reed Bryson was right and that that was probably the dominant effect. But it didn't take very long the way we are putting the greenhouse gases in exponentially. The greenhouse gas is increasing roughly a percent per year all together, so this is an enormous rise in the other competing factor which causes the warming. So the thinking is that the warming has now become much greater than the cooling due to the aerosols.

MS. SCHAKOWSKY. And haven't we--because of the hole in the ozone layer, haven't we reduced aerosols or--

DR. NORTH. Well, the ozone layer, I would give--that is a completely different story, so I would rather we not get off to that.

MS. SCHAKOWSKY. Okay. There was another scientific question I wanted to ask you and again I am not sure how to phrase it. There was something about variability, and isn't there a conclusion that could be made that if there is a great variability, that that might be something that we really need to worry about in fact that the effects of carbon dioxide in the atmosphere in fact may be worse than we thought if we are--

DR. NORTH. Yes. There are natural fluctuations in the system just as weather tickles the whole system and the whole thing rumbles. I mean, we have a climate system that sort of rattles around, so this is the part that we call natural variability. It is a kind of noise in the system.

But then when we apply these nudges that are continuous, then we get a secular trend and the noise on top of it. And by the way, that does tend to be a linear process. There have been many, many studies with climate models, and while of course they are not perfect, they do imitate the atmospheric climate system quite well, and for small nudges like the ones we are talking about, I mean, they seem to us to be quite big but in fact, in that system, they are tiny. We are changing the temperature a degree or two Kelvin compared to 300, so they are tiny. So this is actually a fairly linear process. The signals that we see in the system from warming and cooling and other things, pretty linear, not that nonlinear. So natural variability is there and we worry that we don't understand every bit of it. For example, it could be that there are slow processes in the climate system such as the deep oceans, the overturning and so on of the deep oceans, and it could be that that is the underlying reason for whatever this medieval warm period was. We are not sure about that. It could be that some warm water surfaced. What we know now though is that that is not the cause of the warming in the last 50 years. The warming in the last 50 years could not have been because of--we now have data. We know that is not the reason. In fact, if we look at the map of warming, we see that it is warming more over the continents than it is over the oceans. They are being pulled along because they are not as heavy, they are not as inertial. So the fingerprints of the warming are exactly what we would expect if carbon dioxide were the reason.

Now, as we go back 1,000 years, we don't have all that information to put in there to check it out so we don't know exactly why that might have happened then but we have a very good idea of what has been going on the last 100 years.

MS. SCHAKOWSKY. Thank you.

MR. WHITFIELD. The gentlelady's time has expired, and Ms. Baldwin, you are recognized.

MS. BALDWIN. Thank you, Mr. Chairman. As has been referenced earlier in several opening statements and some questions, we know how the tobacco industry wanting to keep doubt in the public mind and in fact in 1993 the *Wall Street Journal* published a front-page exposé on how the tobacco industry had kept the public doubt alive about whether smoking caused cancer. For four decades the big tobacco companies funded a sham research organization to feed the public doubt about the health effects of smoking, and despite smoking being responsible for over 400,000 deaths a year, that strategy worked tremendously well for decades. The *Wall Street Journal* quoted one big tobacco employee who said, and I quote, "The scientists can come from Mars but no matter how obscure or misbegotten, as long as they are willing to tell the scientific lie that it is not proven, the tobacco industry is off the hook." In May of this year, we learned that some of the same people who worked on tobacco also worked to confuse the consensus on global warming. Mark Hurtsgard reported in *Vanity Fair* that for 20 years Dr. Frederick Siete directed $45 million in medical research for R.J. Reynolds to maintain a hint of doubt about the hazards of smoking. In the 1990s Sietes turned his attention to global warming. Dr. Sietes assaulted the integrity of the Intergovernmental Panel on Climate Change on the op-ed page of the *Wall Street Journal*. He accused the Clinton Administration of misrepresenting the science and authored a paper which said that global warming was an exaggerated threat.

These people have a plan. They want this hearing to stand for the proposition that there is not a consensus on global warming and they have stalled action for a decade or two and they think they can drag it out even longer. So Dr. North, I am wondering if you can help put this in context. Dr. Mann had concluded that the late 20th Century warmth in the Northern Hemisphere was unprecedented during the last 1,000 years. You said very clearly in your testimony that Dr. Mann's conclusion has been subsequently supported by an array of evidence. We have a high level of confidence that late 20th Century is the warmest period the planet has seen in the last 400 years and you found it was plausible that the planet is warmer than it has been in 1,000 years. Is that a fair summary?

DR. NORTH. Yes.

MS. BALDWIN. You said it was plausible that the planet is warmer now than it has been any time in the last 1,000 years. Has anyone provided affirmative evidence that there has been a warmer period in the last 1,000 years?

DR. NORTH. No, we have not. That is what we mean by plausible, that there just doesn't seem to be any counter information, so it is a reasonable thing to--

MS. BALDWIN. Is it plausible that human beings have caused greenhouse gas emissions that are contributing--I am sorry. Let me put it in the negative. Is it plausible that human-caused greenhouse gas emissions are not contributing to global warming?

DR. NORTH. It is not plausible.

MS. BALDWIN. How confident is the scientific community that human emissions are contributing to global warming? Seventy-five percent, 80 percent?

DR. NORTH. In the scientific--in the climate science community, I think that Mr. Inslee's quote about the number of papers and who says yes and who says no tells the story.

MS. BALDWIN. Okay.

DR. NORTH. It is hard to find anyone who works in this field who is opposed. I mean, if somebody can come up with a really good physical explanation for why this is false, they will win the Nobel Prize. So there are a lot of people who might be attracted to the idea but we can't find any.

MS. BALDWIN. Well, finally, I just want to ask you about the IPCC report since we have been hearing a lot about it. Does the NRC report in any way discredit the IPCC's 2001 third assessment report?

DR. NORTH. Well, we have some differences with the details of the hockey stick curve and we said that. We are a little less confident. I mean, our error bars as we have been saying, our margin of error is a little larger than what was stated in that report and that is natural. As we go on and learn more, we adjust and adapt. So, no, we don't believe individual years--Dr. Wegman said this, and we agree. We don't trust individual years, the 1998 or 2006 or something as being the warmest of any time period because we can't state things to that degree.

MS. BALDWIN. Just to clarify, my question was, did your report in any way discredit the IPCC's 2001 third assessment report? Would you view--

DR. NORTH. No, we wouldn't--

MS. BALDWIN. --what you are describing as discrediting that report?

DR. NORTH. No, it doesn't discredit it.

MS. BALDWIN. Okay. Thank you.

MR. WHITFIELD. Thank you, and after almost 5 hours, that concludes the first panel, so we should be through with the second panel in about 10 hours. Dr. Wegman, I want to thank you very much and Dr. North for your testimony and obviously this is a subject matter of great interest and importance and we thank you for your testimony, and now I look forward to the second panel and so I will release you all. And on the second panel we have another distinguished group of individuals. Mr. Thomas Karl is director of the National Climatic Data Center from Asheville, North Carolina. Dr. Thomas Crowley is the Nicholas Professor of Earth System Science at Duke University. Mr. Stephen McIntyre of Playter Boulevard in Toronto Canada, and then Dr. Hans von Storch, who is the director of the Institute for Coastal Research who flew to this meeting from Germany exclusively for this meeting, and Dr. Storch, how do I pronounce the name of your town in Germany where you are from? On here it says G-e-e-s-t-h-a-c-h-t.

DR. VON STORCH. Geesthacht.

MR. WHITFIELD. Geesthacht. Okay. Anyway, we welcome all of you, and as you know, this is an Oversight and Investigations hearing and it is our customary manner to take this testimony under oath and I would ask you, do any of you have objection to testifying under oath? And I am assuming you do not need legal counsel. So if you would please raise your right hand.

[Witnesses sworn.]

MR. WHITFIELD. Thank you very much. You all are now under oath, and Mr. Karl, we will start with you and we will recognize you for your 5-minute opening statement.

STATEMENTS OF DR. THOMAS R. KARL, DIRECTOR, NATIONAL CLIMATIC DATA CENTER; DR. THOMAS J. CROWLEY, NICHOLAS PROFESSOR OF EARTH SYSTEM SCIENCE; DR. HANS VON STORCH, DIRECTOR OF INSTITUTE FOR COASTAL RESEARCH; AND MR. STEPHEN MCINTYRE, TORONTO, ONTARIO

DR. KARL. Mr. Chairman, members of the committee, I am pleased to have the opportunity to testify before you today. I am the Director of NOAA's National Climatic Data Center. The National Climatic Data Center houses the World Data Center for Paleoclimatology, which includes the data sets that have been used to reconstruct temperatures for the past 1,000 years or more.

I was one of the two coordinating lead authors for chapter 2 of the Intergovernmental Panel on Climate Change, the IPCC 2001 assessment. The primary intent of the IPCC periodic assessments is to provide government policymakers with the latest and most comprehensive scientific information possible about the human influences on our global climate in a language that has meaning and relevance to government policymakers. Our responsibility as coordinating lead author was to act as co-chair during the lead author chapter meetings. Each chapter has multiple lead authors and chapter 2 had 10 lead authors. Chapter 2 was to assess the data for changes and variations in climate. Coordinating lead authors are ultimately responsible for ensuring that the final version of the chapter is delivered to the IPCC bureau on schedule. Each chapter is agreed to by all lead authors and discussed and reviewed with other chapter lead authors. There is a very lengthy review process which includes review editors to oversee the review process. In 2001 the IPCC report concluded, and I quote, "New analyses indicate that the magnitude of the warming over the 20th Century is likely to have been the largest of any century in the last 1,000 years," and I emphasize warming here, the magnitude of the warming. Those are my words. "The 1990s are likely to have been the warmest decade of the millennium in the Northern Hemisphere and 1998 is likely to have been the warmest year." These findings were developed after careful consideration of the published literature on this topic in 2001.

The IPCC lead authors considered uncertainties related to two types of temperature reconstruction errors. Such errors can be thought of as having two fundamentally different sources. I will use some technical terms. Parametric and structural, but I will define these. Parametric uncertainty, which results from finite sample sizes to estimate coefficients of a statistical model, is much less important than structural uncertainty. Human decisions that underlie the development of the reconstructed time series may be thought of as forming a structure depicting both real and artificial behavior in paleoclimatic data. Assumptions that guide the decisions made by the experts may not be correct. More important factors may have been ignored. These possibilities lead to structural uncertainty. Structural uncertainty can only be estimated by comparing the differences of equally plausible reconstruction methods. The IPCC 2001 lead authors were able to estimate structural uncertainty associated with the IPCC findings because of the availability of several reconstructed time series.

It is important to note the language used by IPCC in the 2001 assessment included an expert assessment of the certainty of various findings. The IPCC reported findings when the probability of being true reflected certainty between 66 and 90 percent, or in odds terms, better than two to one. Lead authors were asked to develop findings based on at least three levels of certainty, likely, better than two to one odds of being correct, very likely, better than nine to one odds, and virtually certain, better than 99 to one. These odds of probability were based on the lead author's assessment of the published literature and in consideration of thousands of expert review comments. I note that such expert assessments in related fields such as the probability of precipitation forecasting have proven to be quite reliable.

Several research teams have challenged the reconstructed temperatures featured in IPCC. These challenges are not without validity. But now each of the challenges have been assessed in a variety of new analyses. For the past several years there have been at least half a dozen new analyses using many of the same paleoclimatic data featured in IPCC 2001 as well as new data covering longer time periods or slightly expanded geographic coverage. Of all these analyses, none show temperatures during the past 1,000 years higher than the last few decades of the 20th Century and into the 21st Century. These analyses used different statistical methods, various types of paleoclimatic data and different temperature calibration approaches.

In June, the National Research Council reassessed the 1,000-year reconstructed time series. The NRC not only assessed the paleoclimatic data but considered how well the data stands up to our ability to simulate the temperature record of the past 1,000 years. The NRC found that for the most part, climate model simulations are consistent with reconstructed paleoclimatic data of the Northern Hemisphere. The NRC report indicates it is plausible, as we heard, that the last few decades of the 20th Century were warmer than any other time during the past 1,000 years. I note the NRC does not define the odds of probability associated with the term "plausible." In contrast to IPCC 2001, the recent NRC report did not highlight the rate of temperature increase during the 20th Century compared to the previous 10 centuries. I note the rate of temperature increase is also relevant to our ability to adapt to changes in both our society as well as the planet's ecosystems.

In order to improve our estimates of reconstructed temperature, more proxy records await our extraction. Setting out to extract and calibrate proxy paleoclimatic data is necessary but not sufficient to reduce further uncertainty. The data from proxies must also be accessible by the broader science community for analysis. At the present time there is no formal process whereby federally funded scientists must submit their data to a long-term data archive facility for use by the general community.

In conclusion, considering the additional evidence since the IPCC 2001 assessment, I would extend the IPCC 2001 statement about the Northern Hemisphere temperatures in the 1990s being higher than any other decade during the past 1,000 years with probability of better than two to one to include the most recent two decades.

Thank you, Mr. Chairman and committee members, for allowing me the opportunity to discuss and inform the committee.

[The prepared statement of Dr. Thomas R. Karl follows:]

PREPARED STATEMENT OF DR. THOMAS R. KARL, DIRECTOR, NATIONAL CLIMATIC DATA CENTER, NATIONAL OCEANIC AND ATMOSPHERIC ADMINISTRATION, U.S. DEPARTMENT OF COMMERCE

WRITTEN STATEMENT BY
DR. THOMAS R. KARL
DIRECTOR, NATIONAL CLIMATIC DATA CENTER
NATIONAL OCEANIC AND ATMOSPHERIC ADMINISTRATION (NOAA)
U.S. DEPARTMENT OF COMMERCE

FOR AN OVERSIGHT HEARING ON
THE CLIMATE CHANGE HOCKEY STICK

BEFORE THE
COMMITTEE ON ENERGY AND COMMERCE
SUBCOMMITTEE ON OVERSIGHT AND INVESTIGATIONS
U.S. HOUSE OF REPRESENTATIVES

JULY 19, 2006

Mr. Chairman and Members of the Committee: As Director of the National Climatic Data Center, which is part of the National Environmental Satellite, Data, and Information Service (NESDIS) within the National Oceanic and Atmospheric Administration (NOAA), and as Program Manager for one of five different NOAA Climate Goal Programs (Climate Observations and Analysis), I am pleased to have the opportunity to testify before you today. The National Climatic Data Center houses the World Data Center for Paleoclimatology which includes the data sets that have been used to reconstruct temperatures of the past millennium.

I was one of two Coordinating Lead Authors for Chapter 3 of the Intergovernmental Panel on Climate Change (IPCC) 2001 Assessment which contained a number of statements related to temperature change over the past 1000 years. Chapter 3's remit was to assess the data for changes and variations in climate. Coordinating Lead Authors act as Chairs during the Lead Author chapter meetings. Each chapter has multiple Lead Authors, and Chapter 3 of the 2001 IPCC Assessment had ten Lead Authors. The Coordinating Lead Authors are ultimately responsible for ensuring that the final chapter is delivered to the IPCC Bureau on schedule. Each Chapter is agreed to by all Lead Authors and discussed and reviewed with other Chapter Lead Authors. The IPCC has a very comprehensive review process. Review Editors are assigned to each chapter to oversee the review process.

My testimony reviews how the Lead Authors of the 2001 IPCC Assessment developed the various findings contained in that Assessment. Since 2001, there has been considerable additional work related to this topic. I will relate this new work to the findings in 2001 and the June 2006 National Research Council Report on this same topic. I also note that IPCC has an updated policy-makers summary for the Fourth Assessment scheduled for release in 2007.

The primary intent of the IPCC periodic assessments is to provide government policy-makers with the latest and most comprehensive scientific information possible about human influences on our global climate in a language that has meaning and relevance to governmental policy-makers. The IPCC assessments have, however, provided much more. From purely a scientific perspective, participation in the IPCC process is extremely beneficial, as it provides the means for the world's scientists to discuss leading-edge issues with rigorous worldwide scientific review. The IPCC process ensures that the scientists who participate gain from the process a fuller appreciation of where important pay-offs in new research and observing systems are most likely to emerge. This has important impacts on our nation's climate change programs including the Climate Change Science Program.

The IPCC 2001 Assessment

In 2001 the IPCC had several key findings related to changes of temperature during the past 1000 years. This included the time prior to the advent of measuring temperatures with modern instruments such as thermometers, or from more sophisticated methods such as remotely sensed spectral radiances from satellites. Temperatures derived prior to using modern instruments are referred to as the pre-instrumental temperature record. Data from the pre-instrumental period back to 1000 A.D., and up through instrumental period make up what has come to be called the "hockey stick" temperature time series. The 2001 IPCC Assessment included the following findings regarding climate change over the past millennium:

- *New analyses indicate that the magnitude of the warming over the 20th century is likely to have been the largest of any century in the last 1,000 years.*

- *The 1990s are likely to have been the warmest decade of the millennium in the Northern Hemisphere and 1998 is likely to have been the warmest year. Because less data are available, less is known about annual averages prior to 1,000 years before the present and for conditions prevailing in most of the Southern Hemisphere prior to 1861.*

- *Evidence does not support the existence of globally synchronous periods of cooling or warming associated with the 'Little Ice Age' and 'Medieval Warm Period'. However a reconstructed Northern Hemisphere temperature does show a cooling during the 15th to 19th centuries and a relative warm period during the 11th to the 14th centuries, although the latter period is still cooler than the late 20th century.*

- *Analyses of borehole temperatures*[1] *indicate a non-linear increase in global average ground surface temperatures over land of 1.0 ± 0.3°C over the last 500*

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[1] The heat received from the atmosphere at the earth's surface is conducted into the underlying soil and rock. It takes considerable time for this heat to work its way down to deeper layers earth. This transfer of heat occurs constantly over time, and with current methods the signal is strong enough to estimate temperatures about 400 years before the present. Time series of such temperatures are referred to as borehole temperatures and these measurements are used to relate profiles of temperatures with depth to the history of temperature changes at the ground surface.

> *years, with most of the increase occurring since the late 19th century. There may be additional uncertainties due to the assumption used in this technique, and decreasing resolution back in time limits confidence in the exact timing of the warming."*

These four findings were developed after careful consideration of the published literature on this topic. At that time several new analyses of Northern Hemisphere temperatures had become available. The work of Dr. Michael Mann and his colleagues (Mann et al., 1998) enabled the IPCC to consider, for the first time, the rate and strength of the 20th Century warming in comparison to temperatures over the past 1000 years. It is unlikely however, that this work alone could have led the IPCC to the findings listed above. Two other different reconstructions (Jones et al., 1998; Briffa, 2000) of Northern Hemisphere temperatures were also considered that were based on a smaller set of proxies[2]. Unlike Mann's time series data that reflected annual temperatures, these other reconstructed temperatures best reflected summer temperatures and were limited to Northern Hemisphere land areas. One reconstruction used various types of proxies and the other used tree ring density (as opposed to tree ring width). Additionally, other proxies without annual resolution such as borehole ground temperatures and glacial length were considered in the IPCC (2001) findings. A limiting factor in all these analyses is the sparseness of proxy data, especially as one goes further back in time.

Similar to the recent U.S. Climate Change Science Program Synthesis and Assessment Product 1.1, the IPCC lead authors considered uncertainties related to two types of construction errors. Such errors can be thought of as having two fundamentally different sources: parametric and structural. Parametric uncertainty, which results from limited data to estimate the coefficients of various equations in a statistical model, is much less important than structural uncertainty. The human decisions that underlie the development of the reconstructed temperatures may be thought of as forming a structure for depicting real and artificial behavior in the temperature data. Assumptions that guide the decisions made by the experts in selecting specific analysis techniques may not be correct, or important factors may have been ignored. These possibilities lead to structural uncertainty. Structural uncertainty can only be estimated by comparing the differences of equally plausible reconstruction analysis techniques. The IPCC 2001 lead authors were able to estimate the structural uncertainty associated with the IPCC findings because of the availability of several reconstructed temperature time series.

It is important to note that the language used by IPCC in the 2001 Assessment included an expert assessment of the certainty (or uncertainty) of the various findings. The IPCC used the following descriptive terms to represent uncertainty: "virtually certain" indicated a greater than 99% probability of being true (odds better than 99 to 1), "very likely" reflected a certainty of between 90% and 99% (odds better than 9 to 1), and the term

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[2] A proxy climate indicator is a local record that is interpreted, using physical and biophysical principles, to represent some combination of climate-related variations back in time. Climate-related data derived in this way are referred to as proxy data. Examples of proxies are: tree ring records, characteristics of corals, and various data derived from ice cores.

"likely" reflected a certainty of being true between 66% and 90% (odds better than 2 to1) of the time. Similar expert assessments of certainty (or uncertainty) have proven to be quite reliable in related areas of the science, such as weather forecasting.

The total measure of uncertainty for the findings in the IPCC Assessment incorporates both the parametric and structural uncertainty of the reconstructed temperatures. In developing the 2001 Assessment, the IPCC Coordinating Lead Authors and Lead Authors debated whether to use the terminology "likely" versus "very likely" in describing the rate of 20th Century warming, the warmth of the 1990s, and that of 1998 as record highs in comparison with the annual temperature record of the preceding 1000 years. The final decision was to use the term "likely."

This reasoning was based on the newness of the reconstructed temperature results and the limited number of analyses that were available. These factors reduced our confidence in the reconstructed time series reflecting the "hockey stick" curvature late in the record, and led to the selection of the use of the term "likely." The term "likely" reflects a much lower threshold of scientific rigor than is normally associated with more confident findings (as mentioned above, the term "very likely" is linked to odds better than 9 to 1). "Likely" was thus selected as the modifier for the various findings related to the rate of temperature change in the 20th Century and the warmth of the 1990s and 1998 in comparison with the previous 1,000 years for the Northern Hemisphere.

Since IPCC 2001

It is common practice in science to challenge new results and test them under different conditions. Replication of results is a key component of science. The results of these tests are critical to scientific advancement. Indeed, the work of Dr. Michael Mann and his colleagues has brought an opportunity to test and refine the various techniques of developing reconstructed temperatures. Several research teams have challenged the technique used by Mann and colleagues to develop reconstructed temperatures. This includes criticisms of the weight given to specific proxies, the statistical method used and its propensity for underestimating multi-decadal temperature variability, and the short period of overlap with the instrumental record available to calibrate proxy data. These challenges are not without validity, but now each of them has been assessed in a variety of new analyses.

Over the past several years there have been many new analyses using many of the same proxy data Mann and colleagues used, as well as new proxies including longer time periods or slightly expanded geographic coverage (Esper et al., 2002; Briffa and Osborn, 2002; Mann and Jones, 2003; Briffa et al, 2004; Moberg et al., 2005; Rutherford et al., 2005; Esper et al., 2005; D'Arrigo et al., 2006; Hegerl et al., in press). Of all these analyses only one shows temperature during Medieval times higher than those of the early 20th Century, and none of the analyses show temperatures higher than the last few decades of the 20th Century and into the 21st Century. The various analyses used different statistical methods, proxies, and temperature calibration approaches.

In addition, there have been new analyses of glacial length, borehole surface temperatures, corals, and isotopic records of low latitude ice cores (Oelermans, 2005; Cole, 2003; Pollack and Smerdon, 2004; Hoffman et al., 2003; Vuille and Werner, 2005; Vuille et al., 2005). These analyses indicate that the later half of the 20th Century is certainly warmer than any time during the past several hundred years (based on the length of the borehole and glacial length proxies) and the past 1200 years (based on isotopic ice core records).

In June the National Research Council (NRC) reassessed the so-called "hockey stick" reconstructed time series. The NRC Report not only assessed the observational data, but considered how well the data stands up to our ability to simulate the temperature record of the past 1000 years. The NRC found that for the most part, given the various limitations of our knowledge about the history of important causes of climate variations and change (such as changes in solar variability and volcanic eruptions), the climate model simulations are consistent with the inferred large-scale tree-ring and multi-proxy-base reconstructions of the Northern Hemisphere temperature.

The NRC Report indicates it is plausible that the last few decades of the 20th Century were warmer than any other time during the past 1000 years. The NRC has less confidence in quantifying the relative warmth of a specific decade or year because of the difficulty in calculating and estimating uncertainties at this high temporal resolution, e.g., by specific year or decade. It should be noted, however, that the NRC Report did not use language similar to IPCC. In the IPCC Assessment (2001) significant findings are highlighted when the odds are as low as 2 to 1 in favor of being correct, while a more stringent level of scientific certainty is usually imposed on new results (e.g., better than 20 to 1) within the scientific community. The NRC Report went on to state that the Little Ice Age from 1500 to 1850 "is supported by a wide variety of evidence." Further, it notes that evidence for Medieval Warm Period "can be found in a diverse but more limited set of records." Jones et al. (1998) and Osborn and Brifiia (2006) provide local paleoclimatic proxies back to 1000 A.D., or earlier, showing that the Medieval Warm Period was not a hemispheric-wide phenomena, unlike the warmth of the past few decades. In contrast to IPCC 2001, the recent NRC Report did not highlight the rate of temperature increase during the 20th Century compared to the previous ten centuries.

Reducing Uncertainties

To improve our estimates of reconstructed temperature more paleoclimatic proxy records await our extraction. This includes ice cores, corals, sediments at the bottom of the sea, and tree rings among other types of proxy records. As stated in the IPCC 2001 Assessment, more proxies from the Southern Hemisphere would be of great value, as we are unable to estimate Southern Hemisphere temperatures prior to a few hundred years ago. The recent NRC Report also emphasizes the value of increased geographic coverage for longer-term proxies (earlier than ~1600 A.D.). In addition, the power of any single type of proxy is greatly increased when combined with other proxy records to develop multi-proxy data sets, or to estimate the uncertainty of the reconstructed temperature time series.

Setting out to extract and calibrate proxy paleoclimate data is a necessary, but not a sufficient condition to narrow our uncertainties related to quantifying and attributing past climate variations and change. The data from the proxies must be made publicly available for analysis. At the present time, there is no formal process whereby federally-funded scientists must submit their data to a long-term data archive facility for use by others. The submission of data to institutions like NOAA's National Climatic Data Center's World Paleoclimatic Data Center requires significant investment of time by the principal investigators who collected the data to provide the useful information about the proxy data to the receiving Data Center. In addition, if such data are submitted, a significant investment by the Data Center would need to be made to ensure the data are useable to others in perpetuity and safeguarded for future generations. Some initial discussions to help formalize such transactions are beginning to emerge.

This concludes my testimony. Thank you for allowing me the opportunity to help inform the Committee on this topic.

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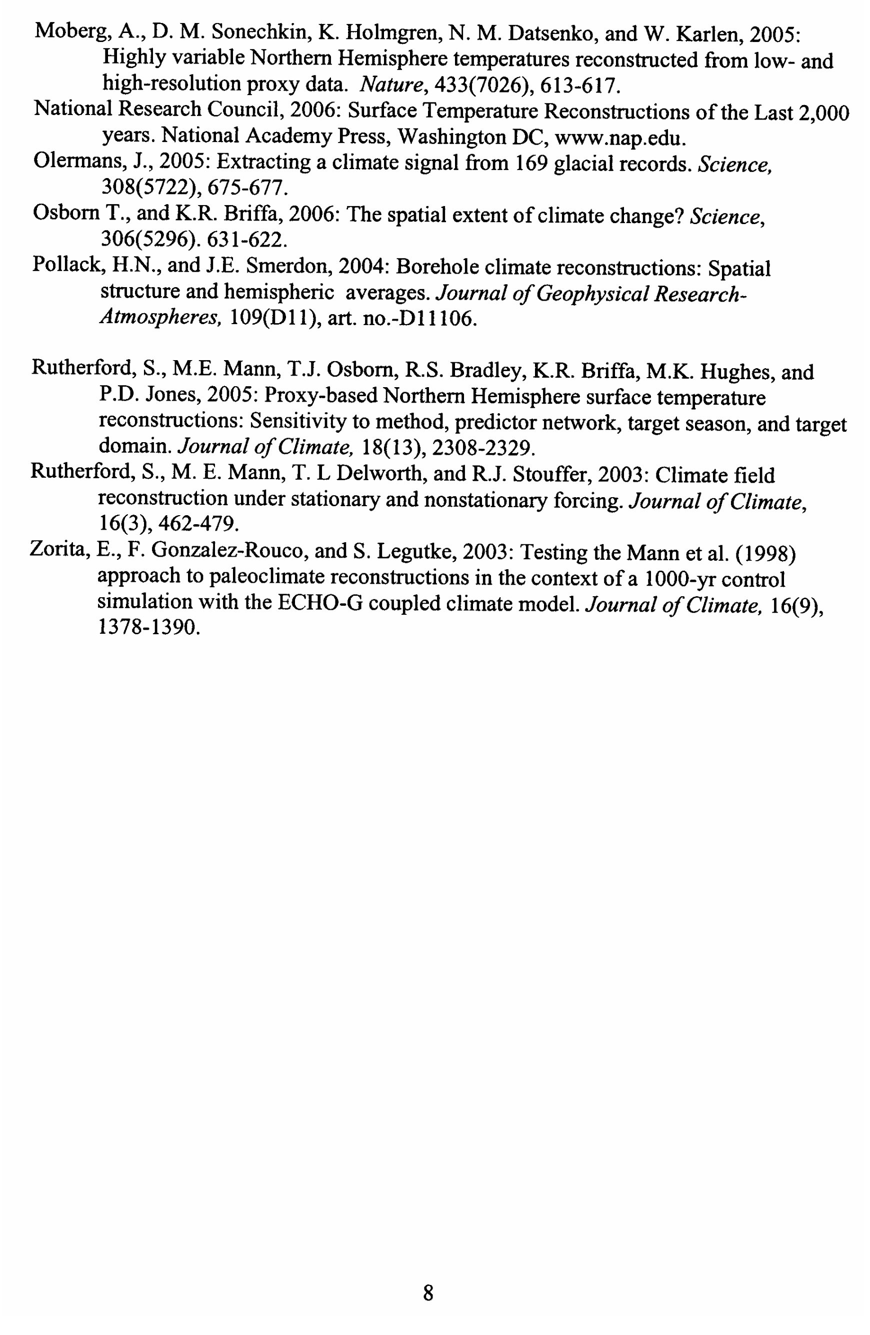

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MR. WHITFIELD. Thank you, Mr. Karl, and Dr. Crowley, you are recognized for your 5-minute opening statement.

DR. CROWLEY. Thank you very much for the opportunity to present my testimony. I will briefly state my credentials and give a short history of the Mann et al. paper with respect to the Intergovernmental Panel for Climate Change report.

As background, I received a Ph.D. in marine geology from Brown University and I specialized in study of the Earth's past climates. Over the last few years I have spent part of my time working on climate change over the last 1,000 to 2,000 years. You have gone over some aspects of the Mann et al. paper ad nauseum so I am going to skip over some elements of what I am going to say and then discuss aspects of how I perceived the Mann et al. report was included in the IPCC, okay. I was not part of IPCC but I am familiar enough with some of the science that was going on that I thought it might be useful. But it is my perception of it, okay.

So with respect to the inclusion of the Mann et al. report, especially into the summary for policymakers, at that time there were three reconstructions that went back 1,000 years, okay, at the time of the IPCC 3, so back to the Middle Ages. Now, one of the reconstructions--Mann et al. was the second to come along. One of the reconstructions uses completely different methodology from Mann et al., and if I could have the second figure there, okay, and I can't read that very well. Really focus on, if you can even read, the right axis, okay. That is temperature variations. Forget the left axis there, and that is time over the last 1,000 years, and the Mann et al. reconstruction is in green. That is decadally smooth Mann et al., reconstruction, okay. And this other reconstruction which I was involved in really stemmed from a discussion I had at a meeting where people say well, I don't believe Mann, and there was nothing written about it. They just say they don't believe Mann. And so out of this grew, I said okay, I got so exasperated. We just--I will go analyze some data myself and just see what it looks like, and we deliberately took a very different approach rather than using what they call the sealed method for reconstructing a temperature. We took this other methodology which has the scientific term "bonehead" associated with it in which we just added up all the individual curves and took the average, okay. And the reason we do that in part is so we can see exactly in the terms of the curve here, you can understand exactly how your composite curve originates from the nature of the raw data, okay, and that is real easy. If there is a bump, you can go back to the raw data and see where it came from, okay. And the other reason for doing that is geological data is by definition dirty data and sometimes it is very helpful sometimes to be somewhat conservative in the statistical methodologies you employ. So what we did is, the bonehead approach using some of the data from Mann et al. but other records also, some of which have been cited as indicative of a medieval warm period, and even though some of these records locally clearly show temperatures locally warmer than 20th Century during the Middle Ages, when we summed up all the different records, we got a pattern that was surprisingly similar to what Mann had gotten as you can see from the red curve there. Yes, there are some differences there but the similarities look a lot more--you know, a lot--there are more similarities than there are differences there. We stopped our analysis in 1960, okay, so that is why we don't get this big tail at the end. But over most of it is pretty similar. So this was in some way was a very--it was a surprise. I had no idea what it would look like, but it suggested that the Mann et al. result might be robust in terms of its pattern about the relative magnitude of warmth in the Middle Ages and what was happening there, when you go back to the raw data which you can easily do with this type of reconstruction, the reason we didn't get a very warm medieval warm period was that whereas some places were warm, others were cold at the same time so when you averaged them, it came out to some value in between. So we understood then why that happened, why we were getting that result.

Now, there is a follow-up to that. Our reconstruction, we weren't trying to say it was better than Mann or anything. We were just trying to do what is called a sensitivity test on the Mann et al. result, okay. Now, the Mann et al. result was the only paper that actually estimated the sensitivities, the uncertainties of your conclusion which Dr. North has emphasized is very important, and for that reason, I believe it was legitimate to include that, to select that as the paper that would go in to highlight the millennial perspective for IPCC because it was the one that had the objectively determined uncertainties in the reconstruction, okay. So that is how it wound up in IPCC and they had some additional information that it might be okay.

Now, science progresses and sometimes past conclusions have to be modified. A notable example with respect to IPCC involves this significance of satellite upper air data that previously had not agreed with model predictions. Now they seem to, okay. That is just the way science goes. Similarly, some papers have been published since the IPCC suggesting greater variability than Mann et al., and contrary to claims of the Wegman report, and again, I should point out here, I apologize for the sometimes poor use of terms that I have used to describe Dr. Wegman and I apologize to Dr. Wegman for that. But contrary to the claims in that report, one of these reconstructions used a completely independent data set for verification.

Can we have the next figure, please? And I was hoping to have a pointer, but the main point here is what you see here is--I just want to spend a little bit of time on this because you are seeing basically the same net conclusion as you see from Mann et al. even though we had greater variability that Mann et al. in this reconstruction, and again, this is sort of a slightly sophisticated update of the bonehead methodology, okay. So it is bonehead squared or something. But what you see here is we have reconstructions here in blue and red and yellow, different length of time series, but it goes back to about 500 A.D., and we have uncertainties assigned to these reconstructions based on the uncertainty in the overlap interval with the instrumental record here. But the difference we get is that we have a completely independent validation based on the methodology data from borehole measurements of heat flow in the Earth's interior, okay. That is completely independent from the data we use. And statistically, we actually have two borehole scientists on our team for that paper plus two sets of statistical climatologists, I might add, that the relationship between this low resolution borehole record and this higher resolution surface reconstruction are indistinguishable and yet the variations you see here are much greater than what you see in Mann et al., okay, the variability. We have a slightly warmer medieval warm period than Mann et al. but even there if you take decadal smoothing there, that peak value here in the medieval warm period really at best approximates what happens in the mid-20th Century, all right? And again, because of the nature of the way we combine the data, we understand exactly why it doesn't get really warm, okay. And so this is a paper that is coming out--well, it has actually been accepted by the *Journal of Climate* and will be out sometime later this year. So this is one of the things we don't agree with Mann in terms of variability. Others, Mann has updated his reconstruction and he still believes that it is the same. So there are still differences in the field but a number of other studies show higher variability at that time.

The interesting point about the higher variability and you have to be really aware of this is that it is not--some people may--it almost seems sometimes in reading papers that people enjoy disproving Mann, okay, but one of the things you have to be aware of, you have a reconstruction that has higher variability and greater warming in the Middle Ages. What it means is, your climate system has higher sensitivity, okay, than the Mann et al. reconstruction which has only small wiggles, okay, and high climate sensitivity carries over to what the implications are for carbon dioxide forcing because the only--sensitivity means that if you have a certain amount of forcing, you either get a small response or a big response, okay. You have a system with low sensitivity, then it doesn't wiggle much. If that was true for carbon dioxide, you wouldn't have to worry about it. You just close the door and throw away the key and keep burning oil until you want. If it has a high sensitivity though, you have to start worrying, and the implication of this result is that climate sensitivity is much higher than before, okay.

So now, I may be almost out of time here, okay, because I have spent a lot of time on this. I have a few comments--

MR. WHITFIELD. Dr. Crowley, you are about 4 minutes over your time, but if you want to summarize, then there will be plenty of questions for you as well.

DR. CROWLEY. Okay. I just wanted to highlight a couple points on the Wegman report. I am not going to talk about their assessment of the Mann et al. thing. That is really--I disagree with them with respect to their recommendations and I will just summarize these disagreements, one being that the interactions with the statistics community have really increased very significantly and I think that Dr. Wegman and his colleagues may have been working with--had a small sample problem just looking at some of the paleoclimate papers because in fact it is a rather substantial improvement in the interactions between real statisticians and the climate--and percolating down into the paleoclimate community, and that is true even for the IPCC. The key chapter in the new IPCC report actually has a statistician and a statistical climatologist as co-lead authors of this chapter, okay. So they are being well integrated into the process.

And finally, with respect to authors should not assess their own work, this sounds fine in theory but in practice it seems almost unworkable because who else but experts can produce an expert report. And with respect to the IPCC, I think it is a marvelous document. It involves hundreds of scientists, reviews of thousands of papers, and received on the order of 10,000 comments for each of the earlier drafts.

MR. WHITFIELD. Are you about ready to conclude, Dr. Crowley?

DR. CROWLEY. So my feeling is that it is a very, very thoroughly reviewed and vetted manuscript and I think it is just about the best thing we have.

[The prepared statement of Dr. Thomas J. Crowley follows:]

PREPARED STATEMENT OF DR. THOMAS J. CROWLEY, NICHOLAS PROFESSOR OF EARTH SCIENCE SYSTEM, DUKE UNIVERSITY

I thank the committee for the opportunity to submit my response to the findings of the NRC and Wegman Reports. As background to my testimony, I will briefly state my credentials. I received a Ph.D. in marine geology from Brown University and have a long interest in the history of the Earth's past climates, both from a modeling and observational viewpoint. I have published about 100 peer-reviewed papers and have co-authored a book on the subject. I have worked in academia, the private sector, and at two government agencies – at NSF as a program director in climate and at NASA/Goddard Space Flight Center as a National Research Council senior fellow. I am presently the Nicholas Professor of Earth Systems Science in the Nicholas School of the Environment at Duke University.

Because this hearing has been called to better understand the influence of the much-discussed 1998 and 1999 papers by Michael Mann, Raymond Bradley, and Malcolm Hughes, I think it would be useful to provide a brief scientific background to the subject. Prior to 1998 there had been only one attempt to summarize the various types of data from past climate to get a broader picture as to how it has changed over the last few centuries. In 1998 Mann et al. introduced a new technique to develop more quantitative estimates of the nature of climate change since AD 1400 for the northern hemisphere, and in 1999 the group extended that record back to AD 1000 and concluded that the late 20th century warming was the largest in the last 1000 years. This report was among a number of scientific studies highlighted in the IPCC Third Assessment Report (TAR) to conclude that "there is new and stronger evidence that most of the observed warming over the last fifty years is attributable to human activities".

With respect to the committee's interests in whether the objectivity of the IPCC with respect to the Mann et al. studies I elaborate on several points below. At the time of IPCC TAR it represented the best estimate of past millennial temperatures and their uncertainties, and that the most important conclusion from IPCC (stated above) does not depend on the Mann et al. papers for its credibility, and are even more robust today than they were in 2001.

The final part of my presentation involves a number of objections, both major and minor, to the Wegman Report.

I have five main points to make concerning the following subjects:

(1) The relation between the Mann et al paper and the IPCC Third Report in 2001. The Mann et al paper was certainly influential in the IPCC Third Assessment Report (TAR), but so were many other papers. But the papers that made the biggest difference were the ones focusing on the instrumental record in which it was shown that models and data could not be reconciled unless an anthropogenic greenhouse influence was invoked. The most compelling driver of all was the fact that global temperatures kept going up and up since the 1996 report, and meltback of glaciers increased in many parts of the world. I might add that this trend has only accelerated since 2001, with melting in the Arctic and on Greenland reaching alarming levels.

(2) The Mann et al paper in and of itself. At the time of IPCC TAR there were two other reconstructions going back to the Middle Ages, with decadally smoothed data showing, at best, past millennial temperatures comparable to the mid-20th century warm interval. One reconstruction (Crowley and Lowery, attached) using a completely different methodology agreed with Mann et al. quite well (Fig. 2). However, Mann et al. was the only paper of the three that estimated uncertainties, and it is no surprise that this paper was the one chosen to highlight the millennial perspective for IPCC. *The significant criticisms of the Mann et al. paper that have been published since 2001 are by definition after the fact with respect to IPCC TAR.*

(3) The present state of our knowledge on millennial changes Science always progresses and sometimes past conclusons have to be modified. A notable example with respect to IPCC involves the significant reassessment of satellite upper air data that previously had not agreed with model predictions of increasing air temperatures in that region; new assessments indicated that the models and data were now in approximate agreement. Similarly, some papers have been published in the last five years suggesting greater variability than Mann et al. Contrary to the claims of the Wegman Report, one of these reconstructions (Hegerl et al., attachment 2) uses a completely independent data set from borehole measurements (fig. 3) of the effects of air temperature change on heat flow in the upper part of the Earth's crust.

Because Mann et al. have more recently obtained results similar to their earlier work, but now using a different methodology, it continues to be necessary to understand the causes of differences among the different reconstructions before the estimates of higher temperature variability can be accepted. Even if the latter estimates ultimately prove to be more accurate, there is no room for gloating (as sometimes seems evident in discussion of the newer results), *for the higher variability inevitably implies a higher climate sensitivity, which is a cause of much more serious concern for either the committee, or society at large*. By this I mean that for any given level of climate forcing from carbon dioxide, the expected temperature response would be larger than it would if the Mann et al. reconstruction was ultimately deemed to be the "final word" on the magnitude of past climate change (see Hegerl et al., third attachment).

(4) The claim of unusual level of warmth for the late 20th century is still valid for all but one of the new reconstructions. Contrary to the conclusions of the the Wegman report, there *is* reason to believe in the unique nature of late 20th century warmth (this is the only major point in which I differ from the NRC report). Although the early millennium records are small in number, the composite reconstruction agrees in the overlap interval (A.D. 1500-1960) with reconstructions using more extensive data sets. Furthermore, examination of the raw data indicates that even in the high latitude northern hemisphere they show regional variations in the timing of warmth that is much greater than in the late 20th century. In other words, some regions are warm and some cold – a very different pattern from the late 20th century, where almost every region has warmed over the last 100 years. It is therefore no surprise that, when these records are composited, the sum value is smaller than for the late 20th century.

(5) The conclusions and recommendations of the Wegman Report have some serious flaws. In addition to a number of technical errors, large and small, the following comments can be made in the bullets on page two of the committee's summary of findings (fact sheet):

    (a) bullet one (concerning specifics of Mann et al.) – responses discussed above

    (b) bullet two – "many of the proxies are reused in most of the papers….it is not surprising that would obtain similar results…" This almost sounds as if it is wrong for everyone to use the best existing data! The more important point, and one not stated, is that different methodologies are employed by each of the investigators. Furthermore, there is nothing wrong with talking to or even collaborating with someone else in a field that you respect, and has expertise that you don't have. The Wegman Report almost seems to imply that collaboration is equivalent to collusion, a result that would apply to the Wegman Report itself if that were always true.

    The inference in the same bullet concerning the failure of the peer review statement is an oversimplification. The anonymity of peer review still allows papers to be rejected, as almost any scientist can testify. As a former NSF program director, I have had significant opportunity to evaluate the peer-review system. It is not perfect but in general the best work gets funded. For publications, editors usually select a variety of reviewers who cover the different expertises in the study. But it is just not practical to expand the number of peer reviews for many publications – the work load is just too onerous for the reviewing pool, and most people will simply decline the request to review the papers. Finally, I would like to comment that the Wegman Report now before the committee has not undergone any extensive peer review from anyone in the climate community prior to its submission to the committee for inclusion into the record and, most problematically, possible use as a guide to further recommendations by the committee.

(c) Bullet three – the researchers do not seem to be interacting with the statistical community. This statement is based on a small subsample of paleoclimate papers. Overall, there is increasingly strong incorporation of statistical methodologies in the climate sciences, including increased interactions with statisticians. For example, the National Center for Atmospheric Research has had a postdoctoral program for statisticians for thirteen years. A key project jointly funded by DOE and NOAA for detection and attribution of climate change involves not only several statistical climatologists but also explicitly seeks out input from statisticians. The present (and key) IPCC Fourth assessment chapter on detection and attribution of climate change has a statistician and statistical climatologist (with a training in applied mathematics) as co-lead authors. Statisticians are welcome to respond to any of the chapters in the review process. From these statements it is clear that the Wegman Report is somewhat uninformed with respect to the effort to include statisticians in the IPCC review process.

I might add that interactions between geoscientists and statisticians have long been hampered by what can only be described by some as a condescending attitude from some statisticians that geoscientists were not employing the most recent, state of the art statistical methods. Such attitudes almost guarantee subsequent poor communication and fail to recognize the unusual nature of “field laboratory” geoscience data, which are very different than “closed laboratories” where the conditions of an experiment are well controlled. The latter types of data require an intimate understanding of the raw data and simpler, more robust statistical methodologies that recognize the limitations of such data.

(d) Bullet four – authors of policy assessment should not assess their own work. This statement may sound fine but in practice but seems almost totally workable. Who else but experts should produce an expert report? The third and fourth IPCC reports involved hundreds of scientists around the world, a review of thousands of papers, and received on the order of 10,000 comments in the early stages of drafts. The final summary for policymakers requires a vote – by government representatives of the signatory nations -- on every single sentence before it is accepted! I can attest from personal experience that the resultant high quality of the IPCC documents make them ideal choices for teaching graduate and professional courses because they are by definition our best statement on the present state of knowledge of the climate system. It is inconceivable to me that a report of this quality could be produced by a group of nonspecialists.

(e) Bullet five – paleoclimate data does not provide insight into physical processes The statement on physical processes is completely wrong. In fact, paleoclimate modeling results indicate that about half of the decadally scaled variance between 1270 and 1850 can be explained by natural variations in solar and (primarily) volcanic forcing. When these forcings are carried over into the 20th century, they cannot explain the 20th temperature rise. Only greenhouse gases can explain the rise, not only for the late 20th century, but also in part for the mid-20th century.

In this same bullet the Wegman Report recommends that federal research should emphasize fundamental understanding of the mechanisms of climate change and should focus on interdisciplinary teams to avoid narrowly focused discipline research. I find this to be an extremely naïve statement. Climate studies are among the most interdisciplinary field that one can imagine – as just one example I submit a copy of a paper (attachment four) on causes of climate change over the last millennium that discusses changes in solar output, volcanism, trace gas variations in climate, tree rings, ice cores, climate models, impact of vegetation, etc etc. There are many other examples of interdisciplinary activities.

As a former program director at the National Science Foundation, I think I can also speak for many present program managers in federal agencies concerning the lack of interdisciplinary activities on different projects. This interdisciplinary is the core concept of terms such as "Global Change" and "Earth Systems Science" and as such the agencies have made a great effort at supporting interdisciplinary research. Furthermore, every major modeling group in IPCC addresses a host of interdisciplinary science.

But it would be a big mistake to forget the lone investigator. Sometimes the most fundamental findings in a field come from these lone investigators (who may nevertheless have much contact with many others). There must be room for individual creative science in climate science.

Summary and Concluding Remarks In my view the debate over the Mann et al paper is a tempest in a teapot. It is legitimate material for scientific discussion but the implications with respect to the operations of the IPCC are unproven and seemingly based, in my opinion, much more on repetition of innuendo than on any real facts. Although there is always a need for enhanced interaction with the statistics community, the lack of communication is seriously misrepresented in the Wegman Reprot. I believe that this report should not be used as either a legitimate assessment of the science or as a guide to policy modification. Finally, I believe it is time to stop using Michael Mann as a whipping post and to start directing attention to the more important matters of whether anything should be done about global warming, and if so, what?

Attachments:

1. Crowley, T.J., and Lowery, T.S., 2000. How warm was the Medieval Warm Period? *Ambio* (publication of the Royal Swedish Academy of Sciences), v. 29, no. 1, pp 51-54.
2. Hegerl, G.C., Crowley, T.J., Allen, M., Hyde, W.T., Pollack, H.N., Smerdon, J., and Zorita, E., 2006. Detection of human influence on a new, validated, 1500 year temperature reconstruction, *Journal of Climate* (accepted).
3. Hegerl, G.C., Crowley, T.J., Hyde, W.T., and Frame, D.J., 2006. Climate sensitivity constrained by temperature reconstructions of the past seven centuries. *Nature*, v. 440, 1029-1032.
4. Crowley, T.J., Causes of climate change over the last 1000 years. *Science*, v. 289, 270-277.

Report

Thomas J. Crowley and Thomas S. Lowery

How Warm Was the Medieval Warm Period?

A frequent conclusion based on study of individual records from the so-called Medieval Warm Period (~1000-1300 A.D.) is that the present warmth of the 20th century is not unusual and therefore cannot be taken as an indication of forced climate change from greenhouse gas emissions. This conclusion is not supported by published composites of Northern Hemisphere climate change, but the conclusions of such syntheses are often either ignored or challenged. In this paper, we revisit the controversy by incorporating additional time series not used in earlier hemispheric compilations. Another difference is that the present reconstruction uses records that are only 900–1000 years long, thereby, avoiding the potential problem of uncertainties introduced by using different numbers of records at different times. Despite clear evidence for Medieval warmth greater than present in some individual records, the new hemispheric composite supports the principal conclusion of earlier hemispheric reconstructions and, furthermore, indicates that maximum Medieval warmth was restricted to two-three 20–30 year intervals, with composite values during these times being only comparable to the mid-20th century warm time interval. Failure to substantiate hemispheric warmth greater than the present consistently occurs in composites because there are significant offsets in timing of warmth in different regions; ignoring these offsets can lead to serious errors concerning inferences about the magnitude of Medieval warmth and its relevance to interpretation of late 20th century warming.

INTRODUCTION

For many years it has been widely known that a "Medieval" warm period occurred during an interval generally cited as being approximately 1000–1300 A.D. (e.g. 1–7). For example, grapes suitable for wine-making were reportedly grown in England (2), and the tree line in Scandinavia was 100–200 m higher than present (8). But were all of these changes synchronous, with hemispheric amplitudes comparable to or warmer than present? Very early in the discussion of this period a number of authors (1, 3, 4) pointed out that there were some significant phase offsets between the timing of warmth in different regions. Two recent (9, 10) Northern Hemisphere temperature reconstructions support the idea of Medieval warming being at most comparable to the mid-20th century Northern Hemisphere temperature peak (that is, about 0.3°C cooler than the decadal average of the 1990s).

Despite these compilations there are still widespread differences of opinion as to the relative warmth of the so-called Medieval Warm Period (MWP) *vis-à-vis* the present century (11). Some authors, especially greenhouse gas skeptics (e.g. 12), continue to extrapolate evidence from individual sites and small regions to infer that the present 20th century warmth is not unusual and is therefore evidence against a major effect of greenhouse gas changes on global climate. Because of the continued debate on this topic, it is revisited in this paper, with some different choices in data, which are also analyzed in a different manner than previous studies.

METHODS

There are two principal differences between the present reconstruction and those of Jones et al. (9) and Mann et al. (10): i) whereas the earlier reconstructions used a different number of records for different time intervals (with coverage for earlier time intervals sparser), the present reconstruction has almost the same number of records used for all time periods—there are a few instances of data cutoff problems at the ends of records but the number of records is still more time-invariant than previous studies; ii) a number of records (ice core, pollen, marine, historical climate records) were chosen that were not included in either of the previous reconstructions; the justification for these inclusions is that these records have often been cited as evidence for Medieval warmth and it is important to test robustness of conclusions with respect to relative levels of Medieval warmth.

Fifteen records were included in the summary (Fig. 1), with an attempt to obtain a balanced spread of sites from among the relatively small number of records that extend back approximately 1000 years. Four records are from the western two-thirds of North America—the White Mountain tree ring record from the lee of the Sierra Nevadas (13); tree ring records from central Colorado (14); and Jasper National Park, Alberta, Canada (15), and a pollen record from central Michigan (16). An oxygen isotope record from the western Sargasso Sea (17) was included, as were 6 sites from the northern North Atlantic/western European sector: the central Greenland GISP2 $\delta^{18}O$ ice core record (18); a historical sea ice/temperature record from Iceland (19); the central England temperature record (20) extended to 1000 A.D. by Lamb (1); tree ring records from northern Sweden (21); the Alps of southeastern France (22); and the Black Forest of Germany (23). The final set of 4 sites are from Asia: the Ural Mountains of western Siberia (24); a tree ring record from the Qilian Shan Mountains of western China (25); a $\delta^{18}O$ ice core record from the Dunde Ice Cap on the Tibetan Plateau (26); and a "phenological" temperature record from eastern China (3). This latter record is from the extensive historical Chi-

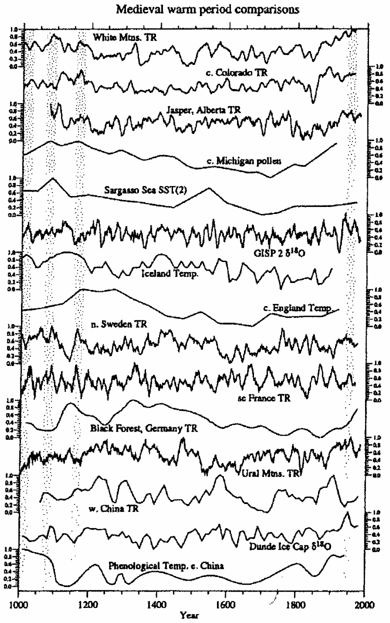

Figure 1. Scaled record of climate change based on 15 sites discussed in text. Vertical grey bars indicate intervals of maximum hemispheric warmth from Figure 2; see text for record citations.

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nese data set (27) and is based on observations of changes in distribution of temperature sensitive biota and other climate indices.

Of the 15 records chosen for this investigation only 6 were included in the Jones et al. (9) and Mann et al. (10) data bases. The records chosen are also less homogeneous than the records employed by Jones et al. (9) and Mann et al. (10) in terms of both type of index and their correlation with temperature. For example, the Iceland record (18) is primarily a winter index, the central Michigan pollen record (15) is an estimate of growing season temperature, the Swedish (21), Urals (24), and west China tree ring records correlate best with summer temperature, the Black Forest (23) record is based on $\delta^{13}C$ measurements, and the central England record is an estimate of mean annual temperature (1). For reference, the Jones et al. (9) compilation is an estimate of summer temperature and the Mann et al. (10) reconstruction is an estimate of mean annual temperature.

Temporal resolution also differs from the earlier studies. While the Jones et al. and Mann et al. records have annual resolution, only 7 of the records from this study have such resolution. Five of the records have decadal-scale resolution and 3 have an average sampling resolution of about 50 years (16, 17, 20). In a sense, these inhomogeneities can be considered in a positive light as a sensitivity test to the robustness of the conclusion of Medieval warmth, with the repeat analysis justified based on the sheer frequency with which such records are used to make broad-scale generalizations about the relative magnitude of warmth in the Middle Ages.

With respect to analysis of the records they (Fig. 2) were scaled from 0 to 1, with annual resolution records first smoothed with an 11-point Stineman filter to bring out the lower frequency trends. The coarser resolution records (Michigan, Sargasso Sea, and central England) were interpolated to 1-year intervals. Due to chronology uncertainties (± 50 years) in the Sargasso Sea record (17), the peak warming was deliberately reset by 20–30 years to line up with maximum warming in the composite (see below); this was done to obtain an optimal configuration for maximum hemispheric warming so that the final conclusions would not be sensitive to chronology uncertainties. Because of the more uncertain temporal resolution of the Michigan pollen (16) and Sargasso Sea (17) records, we constructed 2 composites, the baseline without these 2 indices, and a second "full" composite with these 2 indices included.

RESULTS

A comparison of the individual climate records in Figure 1 and the hemispheric composites (Fig. 2) reveals some interesting patterns. The most prominent times of Medieval warmth in the composites are restricted to 3 relatively narrow intervals (1010–1040, 1070–1105, and 1155–1190). Highest MWP warmth is in the middle interval of the composite section (Fig. 2) and is found in 8 of the 15 records (Fig. 1), a percentage comparable to the 7 of 13 intervals that record the mid-20th century warm period. Subsequent to the third MWP decadal warming, temperatures decrease to a 17th century minimum. This time period (approximately1580–1850) has long been known as the coldest part of the "Little Ice Age" (LIA), with the beginning of this interval coinciding approximately with a pulse of volcanism in the late 16th century (28).

Despite the greater inhomogeneity of the data set in the present composite, the basic features of the previous composites are preserved in the present analysis (Fig. 2). Although the MWP temperature maxima in the different composites (Fig. 2) differ in relative magnitude they agree closely in timing. Correlations on the decadal band, using our baseline reconstruction with the 21-point. smoothed Jones et al. (9) and Mann et al. (10) records–justified to emphasize lower frequency variability–and accounting for the autocorrelation of the time series, yield values of 0.75 and 0.72, respectively, ($p < 0.01$, with correlations being 0.74 ($p < 0.01$) and 0.68 ($p < 0.05$) for our alternate (full) reconstruction). For reference, the correlation between the 21-point smoothed Jones et al. and Mann et al. records is 0.74 ($p < 0.01$). It is, therefore, clear that even a small, inhomogeneous data set can sometimes recover the basic features of hemispheric climate change, such as the Little Ice Age and mid-20th century warm period. This result supports the basic value of length-scale arguments concerning the relatively low number of independent samples needed to obtain reasonably reliable large-scale estimates of temperature (29, 30).

The non-synchroneity of temperature changes referred to in the introduction is evident when comparing the shaded intervals of maximum warmth in the composite (Fig. 2) with the patterns in individual records (Fig. 1). For example, none of the records between Germany and western China—about 100° of longitude –contribute significantly to the peak MWP warming from about 1070–1105. The oft-cited central England temperature record (1) contributes to the third MWP decadal warming (1155–1190) but most of the warming (1150–1290) postdates the final MWP peak in the composite. This response is shared by the Siberian and China records and is almost the inverse to the areas that were cool when a number of sites were warm between 1070–1105.

The spatial pattern for the center parts of two MWP warm intervals, the intervening cooler period (Fig. 2), and the mid-20th century warm period are compared in Figure 3. One difference between the Medieval and mid-20th century warmings involves the general restriction of peak MWP warming to the North American/Atlantic/western Europe sector, whereas the mid-20th century warming appears to be more of a land-sea difference. There are broad similarities between the proxy mid-20th century warming and the instrumental record (31), but due to lack of proxy data in the highest latitudes we cannot substantiate the maximum mid-20th century warming along what appears to be the snow/sea ice edge in the Arctic/North Atlantic sector. More proxy data would be required to test the robustness of the conclusion regarding spatial differences in warming pattern between the MWP and mid-20th century.

Although it might be tempting to attribute the MWP decadal temperature increases to changes in the North Atlantic thermohaline circulation, this temptation should be avoided. Peak Medieval warmth in central Greenland and Iceland, regions associated with a strong North Atlantic Current and more active thermohaline circulation, occurs during a *cool* interlude during

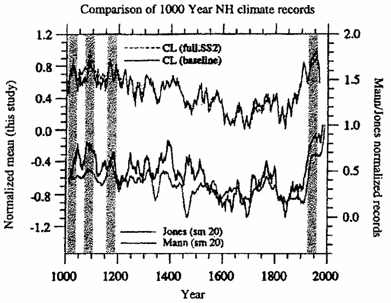

Figure 2. Comparison of hemispheric composites from this study with that of Jones et al. (9) and Mann et al. (10). Shaded intervals refer to times of peak warmth (see text). The dotted line indicates hemispheric composite values if two lower resolution records [Michigan pollen record (16) and Sargasso Sea $\delta^{18}O$ record(17)] are added to the baseline composite (see text). All records have been scaled between 0 and 1.

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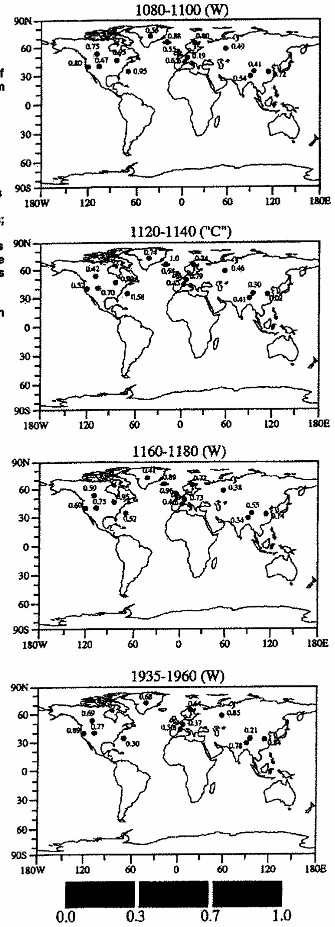
the MWP (Fig. 1). This and other temperature offsets may reflect displacements of the meridional flow pattern of the upper air westerlies (3–5). Again, more geographic coverage would be required to test this hypothesis, although a relatively dense network of tree rings for North America (32) supports this type of response for the cold 17th century climate fluctuation (Fig. 2).

The new composite time series were converted to mean annual temperature in the following manner. The two composites were scaled to agree with the Jones et al. (31) instrumental record for the Northern Hemisphere over the intervals 1856-1880 and 1920-1965 (too few of the proxies record information after this date). The reason for restricting the comparison to these two intervals involves the considerable deviation of the proxy time series from the instrumental record over the interval 1880–1920 (Fig. 4). The deviation occurs in 5 of our records (White Mountains, Colorado, Urals, and west and east China records), has been observed before (10, 33) and been attributed to (10) anomalous tree-ring growth due to the late 19th century rise in CO_2. Mann et al. (10) addressed this problem by removing the postulated CO_2 growth effect before estimating past temperatures. However, because this response also occurs in the Chinese phenological data set, another source of variance for high tree-ring growth rates cannot be excluded. The correlations between the present Crowley-Lowery (CL) composite and the Jones et al. instrumental record were therefore determined in two ways—one using the entire record 1856–1965 and the other using only the tie points 1856–1880 and 1920–1965, excluding the hypothesized interval of CO_2-induced tree ring growth. Correlations using the full time series are 0.55 (CL baseline) and 0.49 (CL full). Correlations using the 2 end member intervals are 0.87 (CL baseline) and 0.88 (CL full). All correlations are significant at the 1% level. Although all detrended correlations are significant at the 5% level, none explains more than 17% of the variance and are therefore of limited use from a paleoclimatic perspective.

Figure 3. Comparison of the spatial pattern for the central parts of two MWP warm peaks, the intervening cooler period (Fig. 2), and the mid-20th century warm period. Ages of the intervals are listed in panel captions; values for individual sites are from Figure 1 data. Results are presented in terciles of relative warmth for the entire ~1000-year interval for each site.

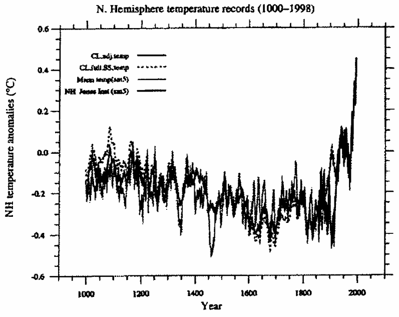


Scaling the CL composites to the Jones et al. instrumental record (31) yields minimum LIA temperatures ~0.45–0.50°C less than the mid-20th century – a result similar to the Mann et al. (34) estimate of ~0.40°C, but less than the ~0.7°C estimate determined from borehole temperature estimates (35). Peak Medieval warming in our composites is with ± 0.05°C of the mid-20th century warm period. Average MWP temperatures (1000-1200) are only about 0.20°C warmer than the LIA interval of maximum cooling from about 1580–1840. If these numbers are substantiated by further investigations they would provide an important constraint on mechanisms for low-frequency climate variability. At this stage they can only be considered as estimates awaiting further clarifications of the reasons for the late 19th century divergence of the proxy records from the instrumental record and the disagreements between borehole and surface proxy records.

DISCUSSION AND CONCLUSIONS

To conclude, a new compilation of evidence for Medieval warmth indicates 3 relatively short-lived warming intervals (1010–1040, 1070–1105, and 1155–1190) that are comparable

Figure 4. Comparison of mean annual temperature records (Fig. 2) from this study with 5-pt. Smoothed Mann et al. (10) reconstruction and the Jones et al. (31) Northern Hemispheric instrumental temperature record. CL.adj.temp refers to the baseline composite adjusted to the Jones et al. record (see text); CL.full.SS.temp refers to all time series in the CL composite, with the Sargasso Sea (SS) record adjusted slightly in chronology to agree better with maximum warming in the hemispheric composite (again see text for details).



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to the mid-20th century warm period. Scaling of the hemispheric composite to the Northern Hemisphere temperature records suggests that Little Ice Age temperatures were about 0.45–0.50°C colder than the mid-20th century warm period and that mean temperatures between 1000–1200 were only about 0.20°C warmer than the Little Ice Age. These results provide useful constraints on mechanisms of climate change on decadal-centennial time scales. For example, forced variations from CO_2, volcanism, and solar forcing have been implicated as contributing to the mid-20th century temperature increase after the Little Ice Age (28, 36–38), and changes in the latter may also have influenced the MWP (e.g. 39).

Because of uncertainties in the proxy-instrumental temperature calibration, it is still difficult to unequivocably assert that the late 20th century warming is significantly greater than the peak warmth of the Medieval Warm Period. But there is even less justification to assert the opposite–it is not possible to make a robust statement that the Medieval Warm Period was warmer than the last two decades. Similar conclusions can be derived from the sparser Southern Hemisphere data set of climate change over the last millennium (7, 9).

In an earlier study, Bradley and Jones (40) questioned the utility of the term "Little Ice Age" in light of their findings concerning significant decadal-centennial scale variability and regional climate trends sometimes of opposite signs. Given the findings of this study a similar concern could be raised about the utility of the term "Medieval Warm Period". Because mean temperatures during this interval were warmer than the subsequent Little Ice Age, we believe that the term Medieval Warm Period still has value, as long as it is restricted to the northern hemisphere (there is insufficient documentation as to its existence in the Southern Hemisphere) and as long as the user is careful to interpret regional trends within the context of hemispheric-scale variations.

The results from this study re-emphasize the hazards of using single or small-area records to make inferences about hemispheric warmth, particularly when the "evidence" is used to conclude that late 20th century warmth in not unusual in the context of the historical record of climate change. The results also indicate that the primary error associated with earlier conclusions is not statistical or climatological but rather stratigraphic – that is, the assumption that a climato- [or litho- (rock)] stratigraphic unit is a time stratigraphic unit. For more than 30 years geologists have recognized that this assumption is not valid in classical stratigraphic applications, but the error still frequently occurs when applied to interpretations of climate change. The error occurs despite the fact that the time resolution of one to a few years for records spanning the last millennium is vastly superior to any other time in the geologic record. Other examples of discordant decadal-centennial-scale trends involves peak warmth over the last 1400 years about 950 A.D. in the Greenland GISP2 $\delta^{18}O$ and borehole records (18, 41) almost exactly coincident with ice advances in Fennoscandia, the Alps, the Colorado Rockies (42, 43). The " Early Medieval Glacial Advances" (44) also occur at the same time as warmth in China (3). The widespread occurrences of such discordances underscores the need for extreme caution in extrapolating local climatic trends to larger-scale inferences.

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45. Acknowledgements. We thank L. Thompson and P. Jones for making data available to us and G. Hegerl, W. Hyde, M. Mann, P. Jones, and H. Rodhe for discussion, assistance, and comments.

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Detection of human influence on a new, validated 1500 year temperature reconstruction

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Submitted to *Journal of Climate*: January 3, 2006, resubmitted June 12, 2006

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Abstract:

Climate records over the last millennium place the 20th-century warming in a longer historical context. Reconstructions of millennial temperatures show a wide range of variability, raising questions about the reliability of currently available reconstruction techniques and the uniqueness of late 20th- century warming. We suggest a calibration method that avoids the loss of low-frequency variance. A new reconstruction using this method shows substantial variability over the last 1500 years. This record is consistent with independent temperature change estimates from borehole geothermal records, compared over the same spatial and temporal domain. The record is also broadly consistent with other recent reconstructions that attempt to fully recover low-frequency climate variability in their central estimate.

High variability in reconstructions does not hamper the detection of greenhouse gas induced climate change, since a substantial fraction of the variance in these reconstructions from the beginning of the analysis in the late 13th century to the end of the records can be attributed to external forcing. Results from a detection and attribution analysis show that greenhouse warming is detectable in all analyzed high-variance reconstructions (with the possible exception of one ending in 1925), and that about a third of the warming in the first half of the 20th century can be attributed to anthropogenic greenhouse gas emissions. The estimated magnitude of the anthropogenic signal is consistent with most of the warming in the second half of the 20th century being anthropogenic.

1. Introduction

Climate records over the last millennium provide observational information on natural climate variability on timescales of multiple decades and centuries. It is from this backdrop of natural climate variability that anthropogenic changes need to be distinguished (e.g., Mitchell et al. 2001; IDAG 2005). Reconstructions of pre-instrumental surface temperature have employed ‘proxy’ information derived from historical records, tree rings, ice cores, marine and terrestrial sediments (see review in Jones and Mann 2004; also Mann et al. 1998, 1999; Crowley and Lowery 2000; Cook et al. 2004; and Moberg et al. 2005) and subsurface temperatures measured in boreholes (Huang et al. 2000; Harris and Chapmann 2001; Beltrami 2002). A number of recent studies have used this information to estimate Northern Hemispheric temperature changes during the last 500-1000 years; these studies indicate a difference between the peak of the cold period in the 17th century and the mid-20th century warming that ranges from about 0.4 to 1.0°C.

Concern about the reliability of reconstructions of past temperature arises from a climate-model based evaluation of one of the reconstruction methods (von Storch et al. 2004), which suggests that reconstruction techniques using methods based on ordinary least square regression for calibration fail to recover some of the low-frequency variance in the model’s true hemispheric temperature. However, the magnitude of the variance loss may be smaller than estimated by von Storch et al. (Mann et al. 2005, see also Wahl et al., 2006) and depends on the properties of the noise (von Storch et al., 2006).

A further concern is that some tree ring data, which dominate the input to most high-resolution surface proxy composites, may not adequately recover low-frequency centennial-millennial scale variability unless standardized to enhance low-frequency information (Cook et al., 1995; Briffa et al. 2001; Esper et al. 2002). These concerns are emphasized by the discrepancy between most surface proxy reconstructions and Northern Hemispheric borehole composites (see Jones and Mann 2004). One exception involves a recent reconstruction that bases its low-frequency variability on lower resolution sediment data from lake and ocean cores, and adjusts the variance to instrumental data during the calibration interval (Moberg et al. 2005).

In this paper we propose a simple calibration method that does not lose variance. It is based on a total least squares fit that allows for proper accounting of noise in both proxy and instrumental data during calibration, yielding a reconstruction with realistic amplitude and uncertainty ranges. When applied to model-generated 'pseudo-proxies' that are sampled over the same locations, and show similar correlations to local temperatures, our approach captures the full range of past climate variability and provides a reliable estimate of uncertainty. We use this technique to reconstruct a decadal record over the past 1450 years. Over the last 500 years, this new record is consistent with borehole results over the same time and space domains, and shows similar levels of Little Ice Age cooling and Medieval Warm Period warming as other reconstructions using data and methods that aim at fully recovering low-frequency variance.

Since the 20th century trend stands out less from trends in previous centuries in reconstructions with higher variance, high variance reconstructions have sometimes been used to question the importance of anthropogenic forcing. However, natural influences on climate, such as changes in volcanism and possibly solar radiation, are responsible for a substantial fraction of past climate variations (e.g., Robock and Free 1995; Crowley

2000; Hegerl et al. 2003; Bertrand et al. 2002; Weber 2005, Stendel et al., 2005). By quantifying the influence of external forcing on these high variance reconstructions, we find that high variability does not prevent confident detection and attribution of anthropogenic climate change.

The paper is structured as follows: Section 2 introduces our reconstruction method, and Section 3 demonstrates the reliability of this method with climate model data. Section 4 compares our reconstruction with other high-variance reconstructions and borehole data. A detection and attribution method is applied in Section 5 to quantify the influence of external forcing on our reconstruction and other high variance reconstructions. We summarize results in Section 6.

2. Reconstruction of hemispheric temperature variability

Our new reconstruction of decadal Northern Hemispheric mean temperature is related to an earlier reconstruction based on a simple average approach (Crowley and Lowery 2000), but uses updated records, a modified reconstruction method, and a new calibration technique. The reconstruction consists of three individual segments: A baseline reconstruction uses 12 decadal records and covers the period to 1505. One longer, less densely sampled reconstruction, which we call CH-blend (long), is based on 7 records back to to AD 946, and CH-blend (Dark Ages) consists of 5 records back to to AD 558. The three reconstructions are each based on equally long proxy records or regional proxy reconstructions based on multiple records that are distributed across the extra-tropical Northern Hemisphere. Using only data covering the entire length of each individual reconstruction largely avoids inhomogeneity in the variability of reconstructions that arises due to changes in spatial sampling density over time (see Crowley and Lowery 2000).

a) Construction of proxy timeseries

We use decadal or decadally smoothed proxy records since information about low-frequency variability is crucial to separate natural climate variability from greenhouse warming, and because fewer records are needed to reliably sample decadal and hemispheric temperature variability (global decadal mean surface temperature is estimated to have effectively 8-16 degrees of spatial freedom, see Jones et al. 1997; Zwiers and Shen 1997). Since almost all long proxies are located in the mid- to high-northern land areas, the reconstruction is calibrated to 30-90°N temperature. A second version of the reconstruction is calibrated to land and ocean regions north of 30°N, which is used for the detection study (see below) and a study estimating climate sensitivity (Hegerl et al., 2006). The tree ring data have mostly been processed in a manner that preserves low-frequency variance (Briffa et al. 2001; Esper et al. 2002; details are given in Appendix A).

The primary reconstruction for the time interval since 1505 is based on 12 well-spaced sites, some of which are area-averages based on multiple records (Figure 1, for individual sites see Appendix A). The first step of the reconstruction technique is to scale the individual proxy records to unit standard deviation, weigh them by their correlation with decadal NH 30-90°N temperature (land or land and ocean, depending on the target of reconstruction) during the period 1880 to 1960, and then average them. This yields a unitless paleo timeseries $\hat{\theta}_{pal}(t)$ that shows high correlations with the instrumental data during the calibration period 1880 to 1960 (0.97 for land only and 0.92 for land and ocean temperatures, correlations become 0.82 for land and 0.75 for land and ocean after detrending).

Such a weighted average is mathematically equivalent to a multiple regression of proxy data onto the target of reconstruction for uncorrelated individual records (our records have only modest correlations with each other), but is much less prone to over-fitting than full multiple regression. This is important because the shortness of the common period of the decadal instrumental and proxy data (1880 to 1960, prior to that, the spatial sampling of instrumental data is very inhomogeneous), does not allow a formal separation of the period of overlap into a calibration and a validation period. Note that the only information from instrumental data used for computing $\hat{\theta}_{pal}(t)$ is the correlation weight, which has only a small influence on the resulting time series; an unweighted, and hence fully independent, average of the sites correlates r=0.96 (0.79 detrended) with instrumental land data compared to the correlations of the weighted average of 0.97 (0.82 detrended).

b) *Calibration to Northern Hemispheric temperature*

The next step is to calibrate the proxy timeseries $\hat{\theta}_{pal}(t)$ against the average NH 30-90 N (land or land and ocean) instrumental temperature record $\hat{T}_{inst}(t)$, using a conversion factor α (hats denote estimated timeseries). This is accomplished by fitting a statistical model of the form

$$T_{inst}(t)+\varepsilon_{inst}(t)=\alpha\cdot(\theta_{pal}(t)+\varepsilon_{pal}(t)). \qquad (1)$$

where ε_{inst} refers to realizations of sampling error and random measurement error in instrumental data, and ε_{pal} to realizations of sampling error and non-temperature variability in proxy records. In most reconstructions, α is estimated by an ordinary least square (OLS) fit, neglecting the considerable uncertainty ε_{pal}, which leads to a low bias of the scaling factor α (see, e.g. von Storch et al. 2004; Allen and Stott, 2003) and thus underestimated past temperature ranges in the scaled proxy timeseries. The loss of variance is largest if the correlation between $\hat{T}_{inst}$ and $\hat{\theta}_{pal}$ is low, as tends to be the case when annual data are reconstructed. To avoid this problem, some reconstructions use less rigorous calibration methods, such as adjusting to the variance of instrumental data or qualitatively scaling the low-frequency component. However, if a total least square (TLS) fit is used, in which noise in both instrumental and proxy data is considered, the shared signal with the instrumental timeseries is scaled to the same amplitude (Adcock 1878; Allen and Stott 2003) This method has previously been used for estimating the amplitude of noisy fingerprints from observations in optimal fingerprinting (Stott et al. 2003). Note that if the uncertainties in the paleo reconstruction are much larger than in instrumental data, an alternative is the use of inverse regression, neglecting error in instrumental data (Coehlo et al., 2004).

The TLS fit requires an estimate of the ratio of the variances of ε_{pal} to ε_{inst}. The variance of ε_{inst} is estimated from sub-sampled climate model data (von Storch et al. 2004; Zorita et al. 2003) yielding an error estimate that is very similar to earlier published estimates of uncertainty associated with sampling error in decadally averaged instrumental data for the entire NH (Jones et al. 1997). Note that we assume that systematic instrumental errors are small relative to sampling errors. This may not be strictly correct for sea surface temperature data, which may be affected by systematic biases such as the correction for using canvas buckets in earlier temperature measurements (Folland et al., 2001, Brohan et al., 2006). These are, however, only used in the land and ocean reconstruction.

Since the variance of ε_{pal} is unknown, we account for this uncertainty by determining the scaling factor α and its confidence interval for a wide range of plausible variance ratios for ε_{pal} to ε_{inst}. For each, the likelihood of the regression residual being drawn from errors with variances of ε_{pal} and ε_{inst} is determined. We then average these regression results, weighted by the likelihood of each residual, to arrive at an overall scaling α and its uncertainty (details are given in Appendix B). The time series $\hat{\theta}_{pal}$ is then scaled by α. The overall uncertainty for the reconstruction is derived from a combination of the uncertainty in the amplitude (α) and the range of error variances for ε_{pal} that is consistent with the calibration residual (see Appendix B). Note that our approach assumes that the errors ε_{pal} and ε_{inst} are uncorrelated and normally distributed. The first assumption appears reasonable since red noise possibly present in the individual records should have been filtered in $\hat{\theta}_{pal}$ (note the small changes in the correlation to instrumental temperature between detrended or nondetrended data), and the second is justified for hemispheric means by the central limit theorem.

The longer reconstructions are constructed in the same manner as CH-blend short and are calibrated to CH-blend (short) rather than to instrumental data, since the longer overlap period allows for a more accurate calibration. Due to their high correlation with CH-blend (short) (r=0.97 and r=0.93), this second calibration step adds only a small additional uncertainty that is accounted for in the uncertainty range (see Appendix B). Note that the high correlations and overall similarity of the longer, sparser sampled reconstructions CH-blend (long) and CH-blend (Dark Ages) with the more densely sampled CH-blend (short) during the period of overlap (see Figure 2) suggests that even a few well-spaced decadal records can provide a surprisingly reliable estimate of the last five centuries.

Figure 2 shows the time series calibrated to instrumental land annual temperatures 30-90 N. CH-blend (short) is similar to the reconstruction used in Hegerl et al. (2003) and Crowley et al. (2003, see also Crowley, 2000), but has enhanced amplitude (by 13% for zonal mean temperature and 7% for land temperature). Larger enhancement due to TLS calibration was found for other records if re-calibrated, yielding on the order of 20-30% enhancement, and if annual reconstructions are attempted (loss of half of variance with OLS, see below). Note that a prominent 11th-century peak in the 2003 reconstruction is not as evident after balancing the composite with more data from Asia.

3. Testing the reconstruction method with climate model data

We have tested our reconstruction technique with model-derived synthetic proxy data, i.e., a "perfect model approach", using the same Atmosphere-Ocean General Circulation Method (AOGCM) simulation of climate of the past few centuries that was used by von Storch et al. (2004). The simulation was driven by estimates of past volcanic, solar and greenhouse gas forcing, primarily from Crowley (2000). The AOGCM provides dynamically consistent data with approximately realistic teleconnections, enabling a reliable estimate of the effects of sparse networks and calibration techniques on reconstructions.

We first create a synthetic instrumental ('pseudo-instrumental') record from the simulation, using the same spatial coverage of annual mean land temperatures as in the actual instrumental record. Next, we derive a synthetic paleo reconstruction ('pseudo-proxy reconstruction'), using gridboxes that represent the areas and sites used in CH-blend (see Figure 1). These model data were perturbed by adding decadal white noise until the correlation between pseudo-proxy data and the pseudo-instrumental decadal data was similar to those in CH-blend over the calibration interval. This reduces the correlation of unperturbed to perturbed decadal local model data to values of 0.1 to 0.7, averaging 0.45, which is similar to correlations between decadal proxy data and local temperature data. These pseudo-proxies are then weighted by their correlation with pseudo-instrumental mean temperature from 30-90°N, averaged and calibrated to the pseudo-instrumental NH 30-90°N mean using the TLS approach.

Figure 3 shows the mean of ten such pseudo-proxy reconstructions with different noise added, and two individual reconstructions with the highest and the lowest variance among these ten. This test shows that the reconstruction method reproduces the actual model 30-90 N land temperature variability very well (correlation over the calibration interval is on average r=0.97, and 0.68 if using detrended timeseries; correlation with the model's true 30-90°N mean ranges from r=0.96 to r=0.98 for different realizations of error over entire reconstruction). On average 97% of the variance of the true model 30-90N mean has been recovered. This result is based on a calibration period of 1898-1977, since the peak early 20th-century warming occurs later in the model and hence the shape of the time series in that period is more similar to that in the observations than the period 1881-1960, which is strongly dominated by a trend only (c.f. figures 2 and 3). Note that each calibration interval is affected by different sampling error due to the extent to which the sampling regions in proxy and instrumental data capture simulated hemispheric mean change. This leads to differences in the extent of variance recovered in the reconstruction, such as the small overestimate of cooling into the little ice age seen in figure 3. Based on an 1880-1960 calibration period, the reconstructed variance is on average underestimated (although less so than if using an OLS technique), and overestimated if using a later calibration period. However, in each of these cases, as well as in the case shown in figure 3, the true hemispheric average remains within the estimated uncertainty range slightly more than 95% of the time, indicating that our uncertainty ranges are conservative, thus validating the reconstruction method.

In contrast, using OLS instead of TLS for calibration reduces the inter-century variance (filtered by applying a consecutive running 60yr- and 40-yr moving average) of the 30-90N mean on average by a factor of 0.81 for calibration of decadal data, and by a factor of 0.40 to 0.30 for annual data. This indicates a very severe reduction of low-frequency variance using OLS calibration. If the proxy data are OLS fitted to local decadal temperature and then averaged, the variance is reduced by about half.

4. Comparison of reconstructed temperature history with other reconstructions based on surface and borehole data

Our reconstruction (Figure 2) shows features seen in many other reconstructions – the broad transition from the relatively warm Middle Ages to the Little Ice Age, and the warming after the Little Ice Age to the present, along with multi-decadal to centennial scale fluctuations (see Jones and Mann 2004). The cool interval before the Middle Ages coincides with the Early Medieval Cool Period, a time of glacial advance in many regions (Grove 1988; Oerlemanns 2005), which corresponds approximately to the historical interval after the collapse of the Roman Empire sometimes called the Dark Ages. Late 20th-century temperatures substantially exceed the maximum temperatures in the Middle Ages, confirming that the late 20th century is probably the warmest interval in at least 1500 years.

Figure 4 shows a comparison of our reconstruction with two other recently published reconstructions with high variance: that by Moberg et al. (2005), and by Esper et al. (2002), calibrated by Cook to NH 30-90N land data (Cook et al. 2004). For the comparison with Moberg et al. (2005), we have re-calibrated our data to NH 0-90N annual means. The CH-blend data show a somewhat smaller level of cooling into the little ice age and a similar warming in the medieval warm period than Moberg et al. (2005), along with overall similar features to that latter reconstruction. Given the uncertainty in the amplitude of CH-blend (only amplitude uncertainty for NH 30-90N land is shown in figure 4 for simplicity, note that the amplitude uncertainty for 0-90N land and ocean is substantially larger); and in Moberg et al. (dotted), the reconstructions are consistent in their estimate of Little Ice Age cooling and Medieval Warm Period temperatures. Similarly, our reconstruction for 30-90N land data is broadly consistent with the Cook et al. (2005) calibrated version of Esper et al. (2002) for both periods. Note, however, that Esper et al. (2002) shows differences from other records in the 12th and 13th centuries.

Figure 2 also shows a comparison between CH-blend and subsurface temperature histories measured in boreholes over the same spatial domain (30-90N land). Boreholes provide estimates of centennial-scale surface temperature changes over the past five centuries that are independent of both the instrumental record and climate proxies. Here we compare the history of temperature changes estimated from 631 extra-tropical boreholes (latitude > 30N, see Figure 1 for locations) to that derived from the multi-proxy reconstruction described above. The ensemble of individual borehole results has been averaged onto a 5° x 5° spatial grid to yield the area-weighted reconstruction shown in Figure 2 (Pollack and Smerdon 2004, see also Pollack and Huang, 2000), which is in good agreement with our surface proxy reconstruction for extratropical land data. An implicit assumption in the borehole verification of the new multi-proxy reconstruction is that borehole reconstructions represent a valid target of verification for surface proxy reconstructions, i.e., that both track changes in the surface air temperature. An analysis of millennial simulations of air and subsurface temperatures from AOGCMs (Gonzalez-Ruoco et al. 2004; 2006) shows that despite seasonal decoupling between air and ground temperatures, deep soil temperature is a good proxy for the annual surface air temperature on land.

We further compare the new multi-proxy reconstruction with borehole results by calculating the subsurface temperature anomaly for each reconstruction (see Figure 5). The temperature anomaly represents the transient warming of the subsurface at a given time as the subsurface evolves from a previous temperature field. The subsurface temperature anomalies for both the borehole and CH-blend (short) reconstructions are calculated using a conductive forward model in which each reconstruction comprises a time-varying surface boundary condition (see Carslaw and Jaeger, 1959). The borehole subsurface temperature anomaly shown in Figure 5 is calculated from the mean of 631 reconstructions, each based on individually inverted borehole temperature profiles, rather than calculating an average borehole profile directly from the data. A comparison of the calculated subsurface temperature anomaly with the average observed anomaly determined by Harris and Chapman (2001) shows the two to be almost identical. Because the geothermal reconstruction extends back to only 1500 A.D., we have compared only a segment of the multi-proxy reconstruction with it. To achieve isolation of the post-1500 A.D. history, we assume a steady-state (zero heat-flux) surface boundary condition prior to 1500 A.D. for both the multi-proxy and geothermal reconstructions. This assumption enables a direct comparison of the two reconstructions in their common time interval, 1505 to 1960. As can be seen in Figure 5, the subsurface expressions of both reconstructions are almost indistinguishable. The uncertainty envelopes of each are also shown; these were calculated by driving the model with the upper and lower uncertainty time series shown in Figure 2. These uncertainties also encompass the results generated from the range of reasonable initialization temperatures in the conductive-forward model.

5. Implications for Attribution of Climate Change to Causes

Changes in external radiative forcing need to be considered in order to understand the origin of past climate variability. We thus apply a detection and attribution analysis in order to quantify the role of external forcing, including greenhouse gas forcing, in our reconstruction and in other reconstructions with substantial century-scale variability. Such an analysis based on paleo reconstructions from several centuries enables a better separation of the climate response to natural forcing, particularly between solar and volcanic forcing, both of which are spuriously correlated over the 20th century (e.g., North and Stevens 1998), and between solar and anthropogenic forcing (e.g., Cubasch et al., 1997). Long reconstructions also provide an opportunity to estimate internal climate variability independently from climate model data. The dependence of detection results on climate model control simulations is an important uncertainty in the attribution of climate change to causes (Mitchell et al. 2001).

We use a multi-fingerprint detection and attribution method (Hegerl et al. 2003; see also Hegerl et al. 1997; Tett et al. 1999) using fingerprints for solar, volcanic, and a combination of greenhouse gas and aerosol forcing. Fingerprints for external forcing are derived from simulations with a two-dimensional Energy Balance Model (EBM) and additionally from the ECHO-G AOGCM used above. The EBM is the same as used in Hegerl et al. (2003), it has a realistic land-sea distribution and seasonal cycle. It is driven with solar forcing variations ("sol"), greenhouse gas plus sulfate aerosol forcing ("anthro"), and forcing by explosive volcanism ("vol", forcing details see Appendix C). All climate model data are sampled over the same spatial and temporal domain that is represented by the proxy reconstruction (see table 2, note that we use zonal mean CH-blend data since these are simulated more realistically in the EBM). The influence of external forcings on the proxy reconstruction, T_{proxy}, is assessed by estimating a scaling factor β to best match the time-dependent fingerprints $f(t)$ to observations (note that this scaling factor is determined by an OLS approach since the EBM provides noise free fingerprints):

$$T_{proxy}(t) = \beta_{sol} \cdot f_{sol}(t) + \beta_{vol} \cdot f_{vol}(t) + \beta_{anthro} \cdot f_{anthro}(t) + \varepsilon_{noise}(t)\,. \qquad (2)$$

We estimate the amplitudes β of the fingerprints and the residual climate variability ε_{noise} simultaneously from the proxy reconstruction from 1270 on, since forcing data are less reliable prior to that time. We show results mainly for CH-blend (long) for this analysis, results for CH-blend (short) are similar. Errors in the overall magnitude of forcing or climate model response will not affect detection results. Random errors in the size of individual volcanic eruptions tend to influence results only slightly (see Hegerl et al., 2006). Note also that we do not rotate fingerprints in order to optimize signal-to-noise ratios as often done analysis of instrumental records (e.g., Hegerl et al., 1997; Stott et al., 2003).

A fingerprint is detected in the reconstruction if its scaling factor β is significantly larger than variations in β that occur due to internal climate variability alone. The residual variability ε_{noise} is used as our main estimate of internal climate variability that determines variations in β due to internal climate variability. The estimated residuals are shifted in time by varying increments to increase the number of sample timeseries for internal climate variability and thus to improve the power of the detection test (see Hegerl et al. 2003). The number of effective degrees of freedom in these samples is limited due to autocorrelation. In order to estimate this effect, the autocorrelation and thus the decorrelation time of 20-yr offset residuals has been estimated (reasonable changes in offset period do not affect results). The effective sample size is then estimated from the number of samples separated by the decorrelation time, and further reduced by the number of signals fitted to the data (three for the multi-signal approach). This often yields less than 10 degrees of freedom. However, all results given here are robust to reasonable changes in estimated degrees of freedom (for the shortest Briffa et al., 2001 timeseries the residual from using the unscaled solar and volcanic timeseries is used to preserve more degrees of freedom)

We have assessed the robustness of our attribution results by using the internal variability in the ECHO-G simulations (Zorita et al. 2003; von Storch et al. 2004) rather than residual variability in proxy reconstructions to estimate the significance of scaling factors for fingerprints. We concatenated the control simulation variability and the difference between both forced AOGCM simulations (adjusted by a factor of sqrt(2) to account for doubled variance in a difference between simulations). These data are then treated similarly as the noise residual. Generally, the model has somewhat more variance than the residual from all proxy reconstructions (table 2, ECHO-G has a standard deviation of 0.18K for 30-90N decadally averaged temperature, note that residual variability from reconstructions has been corrected for bias in variance due to fitting three fingerprints), suggesting that the use of this climate model's internal variability is conservative in detection and attribution approaches.

Results using CH-blend show that its decadal temperature variability is largely driven by external forcing (typically 60-75% of the variance in entire proxy reconstructions, table 2). We clearly detect the influence of volcanism on hemispheric temperatures (see Figure 6), similar to previous studies (Robock and Free 1995, Crowley 2000; Hegerl et al. 2003). The EBM does not simulate changes in atmospheric dynamics associated with volcanism (Shindell et al. 2004), but these affect hemispheric-scale and annual temperature only slightly (Thompson et al. 2000). Consistent with earlier results (Hegerl et al. 2003), a response to solar forcing cannot be robustly distinguished. This can be due to either the climate response to solar forcing being small, or to low-frequency variations in solar forcing being different from estimates used here. The latter is quite possible given large uncertainties in these reconstructions (Lean et al., 2002). Note that the detection and attribution approach has only been applied to the best-guess reconstruction. This is appropriate since the detection of fingerprints is unaffected by the amplitude the reconstruction (see Mitchell et al., 2001), and random sampling errors are indirectly accounted for by the noise residual.

The fingerprint analysis also reveals that anthropogenic greenhouse gas and aerosol forcing is detectable in our reconstruction by the end of the record (< 2.5% significance level). The anthropogenic signal is estimated to be more than 4.5 times larger than the standard deviation of scaling factors derived from the residual of the reconstruction. Sensitivity tests reveal that this result is not sensitive to omitting a reduction in greenhouse gas forcing during the Little Ice Age (Figure 6). The detected signal of greenhouse warming is consistent with results from the late 20th century and thus supports the finding that most of the late 20th-century warming has been anthropogenic (e.g., Tett et al. 1999; Mitchell et al. 2001). The anthropogenic forcing explains about a third of the trend over the first half of the 20th century in the reconstruction (90% confidence interval 22-52%, Table 2, see also Figure 6). Additional contributions to the early 20th-century warming arise from a decrease in volcanism (16% to 39%), internal variability (best estimate 30%, compare with Delworth and Knutson 2000), and an uncertain contribution from an increase in solar forcing that is smaller than typically estimated from data for the 20th century alone (Stott et al. 2003).

The fingerprint of all radiative forcings combined from the ECHO-G simulations (von Storch et al. 2004; Zorita et al. 2003) can also be detected in CH-blend (long) from 1270 on (using a single model simulation as fingerprint) and CH-blend (short) (using an average of two simulations). The model fingerprint has to be substantially reduced in amplitude to match our reconstruction, despite application of a TLS fit to account for noise in the fingerprint (see Stott et al., 2003). As a sensitivity test, we have also performed the same multi-fingerprint detection analysis with data from the ECHO-G simulations instead of the reconstruction, with similar detection results. However, the ECHO-G simulation has substantially enhanced volcanic (scaling factor 2.8 +/- 0.8) and probably enhanced solar fingerprints (factor 2.7+/- 1.7) relative to the EBM fingerprints, despite both models having a similar climate sensitivity of ca 2.5°C. This difference is at least partly explained by the use of an older version of the volcanic forcing from Crowley (2000), and a stronger solar forcing.

A detection and attribution analysis of other high-variance reconstructions yields similar results (Table 2), and estimated signals are quite consistent between reconstructions (Figure 7). Table 2 also shows estimates of the contribution of various forcings to the trend over the first half of the 20th century. The detection of the anthropogenic fingerprint is robust for all results except Moberg, where significance levels of that signal are dependent on details of the analysis. The Moberg et al. (2005) reconstruction is also the only one that shows a detectable solar signal, although this is somewhat sensitive to details of the analysis. The residual decadal variance of all reconstructions is less than that of the ECHO-G model (table 2). Thus our results suggest that model simulated internal climate variability, at least from ECHO-G, is conservative.

6. Conclusions

We have introduced a simple method to reconstruct past climate variability that applies a total least squares approach in the calibration step that overcomes the inherent loss of variance in ordinary least squares calibration methods. This reconstruction method has been successfully validated with climate model data. A reconstruction based on this method is consistent with borehole reconstructions, when compared over the same spatial domain, and shows Little Ice Age cooling and Medieval Warm Period warming that is consistent with that in other recent reconstructions that focus on the recovery of low-frequency variance. Our results demonstrate that long, properly calibrated high resolution proxies indeed carry low-frequency information and produce reconstructions with considerable variability between centuries.

We find that natural forcing, particularly by volcanism, explains a substantial fraction of decadal variance in our and other high-variance reconstructions. Greenhouse gas forcing is detectable with high significance level in all analyzed reconstructions except Moberg et al. (2005), which ends in 1925, and explains a considerable fraction of the early 20th century warming. In contrast, solar forcing is marginally detectable. In conclusion, our results indicate that enhanced variability in the past does not make it more difficult to detect greenhouse warming, since a large fraction of the variability can be attributed to external forcing. Quantifying the influence of external forcing on proxy records is therefore more relevant to understanding climate variability and its causes than determining if past periods were possibly as warm as the 20th century.

Acknowledgements

The authors wish to thank Jesse Kenyon for technical support, Francis Zwiers for discussion and many helpful suggestions, and three reviewers for their comments. NOAA, NOAA's office of global programmes and the DOE Office of Science, Office of

Biological and Environmental Research, and by Duke University. GH was additionally supported by NSF grant ATM-0002206 and ATM-0296007. EZ was partially funded by the European research project SO&P. HP and JS were supported by NASA Grant GEWEC-0000-0132, and JS was additionally supported by a Lamont-Doherty Postdoctoral Fellowship.

Appendix A: Records used for the new 1500 yr reconstruction

The CH-blend reconstruction is composed of records from twelve sites, some of which contain multiple records (Figure 1 shows their locations). It is termed a blended time series because it includes some data from Esper et al. (2002) that is not available elsewhere. All records obtained from Esper et al. (2002) had already been standardized using regional curve standardization (RCS) methods, which better preserve low-frequency variations in treering-based reconstructions. Processing of the other records varied by source (see below). With the exception of one record (East Asia, decadal), all records are annual records that we have decadally smoothed.

- *western U.S.*: this time series uses an RCS processed treering composite used in Mann et al. (1999), and kindly provided by Malcolm Hughes, and two sites generated by Lloyd and Graumlich (1997), analyzed by Esper et al. (Boreal and Upper Wright), and provided by E. Cook. The Esper analyses were first averaged. Although there are a number of broad similarities between the Esper and Hughes reconstructions, the correlation is only 0.66. The two composites were averaged.
- *Alberta, Canada*: this time series is also a composite of two different analyses of the 1997 reconstruction of Luckman (1997) – one is unchanged from Luckman's paper, and the other (Athabasca) has been RCS processed by Esper et al. (2002). The correlation between these analyses is unimpressive (0.11); the records were simply averaged. Note that although the correlation with the decadally smoothed 30-90N instrument (land) temperatures varies greatly between the two records (0.14 for Athabasca, 0.82 for Jasper), the composite correlation is 0.84.
- *Mackenzie Delta*: The original time series (Szeicz and MacDonald 1995) provided by Esper et al. only had a 0.04 correlation with the 1880-1960 decadal average of NH temperature, which yields a very small weight if used for the hemispheric composite. We experimented with various other data from the National Geophysical

Data Center (NGDC, http://www.ncdc.noaa.gov/paleo/ftp-search.html) to determine if other reconstructions for that area would yield more information for a hemispheric reconstruction. We found generally that proxy data for that region show little correlation with hemispheric mean temperature. We nevertheless included this site for the sake of completeness and in order to include as many long sites as possible.

- *Quebec*: The situation with Esper's Quebec reconstruction is somewhat similar to what was experienced for the Mackenzie Delta time series; the correlation between their Quebec record and the 30-90N average is only 0.25, partly because the time series ends in ~1930. Examination of the NGDC data base indicates that the original Esper et al. reconstruction appears to be from the Boniface site. A record from nearby St. Anne also shows many similarities to Boniface (r=0.66), extends closer in time to the present, but is also slightly shorter (the Boniface/St. Anne correlation is 0.70). Although the Boniface/St Anne composite has a very high correlation with the 30-90N (land) record (0.88), inspection of shorter records from Fort Chimo and No Name Lake showed a different 20th century response – earlier warming and late cooling. In order to preclude a Quebec composite from indicating a potentially unrealistic magnitude of late 20th century warmth for the whole region, we created a shorter composite of the four sites that averages records from a Fort Chino and No Name Lake composite after 1806. The new composite still shows significant warming in the 20th century, but not as extreme as the Boniface/St. Anne sites alone. It is not claimed that this solution represents the best possible way to deal with the conflicting evidence from Quebec; the problem can best be resolved if more long records from other regions of Quebec can be uniformly stacked together without any late-century adjustments.

west Greenland: this composite is from Fisher et al. (1996).

- *European historical*: this composite was kindly provided by J. Luterbacher et al. (2004).
- *n. Sweden*: this is from Grudd et al. (2002) by way of Esper.
- *w. Siberia*: in order to avoid any heavy biases of the mean composite by a number of sites from one region, the west Siberia time series is a composite of three/four time series from this region: two "polar Urals" records east of the Urals – Yamal (Briffa et al. 1995) and Mangazeja (Hantemirov and Shiyatov 2002 - both by way of Esper et al.) and two records from west of the Urals (Hantemirov and Shiyatov 2002). The records from each side of the Urals were first averaged and then combined for the w.Siberia.short composite; the w.Siberia.long composite involved Yamal and the west.Urals composite. The sites from Esper have been RCS processed.
- *Taimyr Peninsula*: this is from Naurzbayev et al. (2002) by way of Esper.
- *e.Siberia*: the Esper et al. (2002) composite used the Zhaschiviresk time series from Schweingruber. However, this composite only went to 1708. We combined it with a ring width (by Schweingruber, available NGDC) series from the nearby Ayandina River site after removing the obvious growth overprint in the early part of the younger record.
- *Mongolia*: this is from the D'Arrigo et al. (2001) study. However, the full composite illustrated in this paper is not available. We reconstructed the composite from nine records from tree ring sites sent to the NGDC sites. The early growth part of the treering series from overlapping records was removed without further removal of low-frequency variability.
- e.Asia: this is the high resolution record (10-yr average) from Yang et al. (2002).

Individual proxy sites are shown in Figure A1, correlations between these and NH 30-90N land and land-and-ocean instrumental temperatures are given in Table A1.

Appendix B: Estimating uncertainty ranges of our reconstructions

This appendix details the derivation of the uncertainty ranges of our reconstruction. As explained in the body of the paper, we apply a total least square fit (Allen and Stott 2003), since this method can account for errors in instrumental and proxy data without artificial loss of amplitude of the reconstruction. It is related to fitting the main axis of an ellipse through a cloud of points, after scaling the data so that the ratio of the variance of errors ($\varepsilon_{inst} / \varepsilon_{pal}$, see Section 2) is 1. The uncertainty ε_{inst} of the instrumental data for land 30-90N annual averages due to sampling is estimated from comparing true model averages from a coupled climate model simulation with the ECHO-G model (von Storch et al. 2004) to averages based on instrumental coverage over the area reconstructed and the calibration interval. Estimates of ε_{inst} varied between two simulations and time-periods analyzed and range from a standard deviation of 0.034°C to 0.055°C (0.046°C to 0.056°C land and ocean combined). This is broadly similar to the error estimated in Jones et al. (1997) of 0.060°C for decadal averages over the whole Northern Hemisphere in the first half of the 20th century. We chose standard errors ε_{inst} of 0.047°C for land records and 0.056°C for land and ocean combined records (amplitudes and uncertainty ranges vary only by a few percent if ε_{inst} is varied within the range found in sub-sampled ECHO-G simulations and for instrumental data). Since the true error ε_{pal} is not known, we have varied its variance from 0 to 16 times that of the instrumental error, and calculated the probability of the actual calibration residual between reconstruction and instrumental data based on an F-distribution for each variance ratio. Note that the thus determined most likely ratio of proxy to instrumental data is one that yields a residual which is close to that actual observed. To obtain our best guess and uncertainty range, the results for every variance ratio have been integrated, weighted by the likelihood of each residual given the assumed noise ratio. In all cases, a variance ratio of 16 yielded very small probabilities, and was also assumed to be highly unlikely, so we have not sampled the tail of the F-distribution beyond a ratio of 16. Overall, this yields a best guess amplitude α and errorbars that account for the uncertainty in our knowledge of the variance of ε_{pal}.

Using this approach not only yields slightly larger best guess values of the scaling factor α than using an OLS approach (see Section 2), but more importantly substantially increased 97.5% upper limits for the amplitude (37% higher for land, 65% higher for zonal). This demonstrates that given the true uncertainty in paleoclimatic reconstructions, large past climate variations are much more likely to be retained with this method than with standard regression techniques.

Uncertainty of each decadal mean value of the reconstruction in any given year consists not only of the uncertainty in the scaling of the record, but also of the random sampling error and the non-temperature variability in the proxy records that is not removed by averaging. The latter is unknown, but the F-values of the residuals in calibration for each ratio $\mathrm{var}(\varepsilon_{inst})/\mathrm{var}(\varepsilon_{pal})$ discussed above yield the most likely variance ratio and hence (since we can estimate the variance of ε_{inst}) also the most likely variance of ε_{pal}. This variance is used to estimate the random sampling uncertainty of the reconstruction for the best-guess scaling factor. The sampling uncertainty for the upper 97.5% range is derived from the ratio of both errors associated with a high scaling factor, and for the lower 2.5% bar from that for the low scaling value. Resulting errors are typically about 2-3 times as large for the high-amplitude upper scaling limit than for the low-amplitude lower limit, and on the order of twice or more the instrumental error.

The amplitude error and the interdecadal noise are combined by associating the timeseries with upper and lower amplitude limits with the respective errors, and choosing the entire range covered as the reconstruction's full error range. This results in an overestimate of uncertainties, but not dramatically so (order of 3% rather than 5% exceedances of 95% uncertainty range in the perfect model study discussed below). This uncertainty range is shown in the figures.

We have scaled only the best-sampled reconstruction CH-blend (short) from 1505 on to instrumental data. Both the long CH-blend (long) and the very long reconstruction CH-blend (Dark Ages) are calibrated to this shorter and better sampled proxy series 1505 to 1960. This yields a small increase in uncertainty range due to the calibration between long and short series, which is accomplished by an inverse OLS fit (the extreme case of a TLS fit with no additional noise on the target of reconstruction; see Coelho et al. (2004); this is appropriate since only the subsampled longer series exhibits sampling error when calibrated against the shorter, more densely sampled, series). We calibrate the primary reconstruction (termed CH-blend (short), 1505-1960) to the instrumental period as discussed above. The longer, less densely sampled reconstructions (CH-blend (long), to 946, and CH-blend (Dark Ages), to 558) are then calibrated to CH-blend (short). Due to their high correlation with CH-blend (short) (r=0.97 and r=0.93), this adds only little additional uncertainty that is accounted for in the uncertainty range for this section of the time series.

Appendix C: External radiative forcing during the past millennium

This appendix briefly describes the estimates for external radiative forcing used in our fingerprints for the past millennium, largely referring to Hegerl et al. (2003). Greenhouse gas forcing for the instrumental period is based on IPCC radiative forcing reconstructions for well-mixed greenhouse gases. Ice core evidence suggests small multi-decadal to centennial scale changes in greenhouse gases such as CO_2, N_2O, CH_4 over the last millennium (Etheridge et al. 1996, 1998; Fluckinger et al. 1999), in addition to the anthropogenic increase in these greenhouse gases over the industrial period. We applied standard radiative forcing conversion algorithms to convert these changes to radiative forcing.

The solar forcing time series is based on a C^{14} residual record of which has been converted to C^{14} production rate changes using a box model (Bond et al. 2002). This record was combined with the Lean et al. (1995) solar irradiance reconstruction (note that there is considerably uncertainty in reconstructions of solar forcing, see Lean et al. 2002).

We also applied an updated version of a global volcano reconstruction (Crowley 2000), which is based on long ice core records of volcanism that extend back to A.D. 1200, four ice cores from Greenland and five from Antarctica and is described in more detail in Hegerl et al. (2003) and a new publication (T. Crowley, in prep.). The reconstruction includes a latitude profile of forcing that is either based on knowledge of the origin of the eruption or on an estimate based on the representation of the aerosols in ice cores in both hemispheres. The reconstruction of aerosols in ice cores is then calibrated to aerosol optical depth and converted to radiative forcing perturbations using the Hansen et al. (2002) $\Delta F = 21.0$ (AOD) conversion. Because the radiative forcing from extremely large volcanic eruptions results in larger particles that enhance the infrared warming effect (Pinto et al. 1989), we used a 2/3 power scaling between AOD and RF for eruptions exceeding 4 W/m^2. This adjustment makes only modest effects except for the very large 1258 eruption not included in the analysis, and the 1453 Kuwae eruption loadings over Antarctica.

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Figure captions

Figure 1: Regions used for the paleo reconstruction (pink, yellow and blue areas) and for the borehole reconstruction (blue triangles). The data shown in pink are used only for the reconstruction extending to 1505 A.D. Shaded blue regions are used in the short reconstruction and the long reconstructions to 946, of these, only the Mongolian and Alberta (Canada) records drop out in the longest reconstruction to 558. The Scandinavian record shown in yellow is used in both long and short reconstructions, as there is only a small correlation between this record and the large-scale European record that encompasses it.

Figure 2: Proxy reconstruction of 30-90 N mean annual decadally averaged temperatures over land back to 558 A.D. The long time series is made from three segments covered by different amounts of data, which are kept constant within that segment. Grey shaded ranges give the 95% uncertainty bounds of decadal temperature estimates. Another recent proxy reconstruction and the borehole reconstruction over the same spatial domain are shown for comparison. Each time series is plotted relative to its 1880-1960 mean (shown by grey shaded region). This is a departure from the commonly used reference period of the 1961-1990 instrumental mean (indicated by a dotted line; for example, Pollack and Smerdon, 2004, pinned the zero crossing of the 20th-century trend in the borehole time series to the equivalent point in the linear trend of the 20th-century instrumental data plotted relative to the 1961-1990 mean). We now use the 1880-1960 calibration period since it demonstrates most clearly the differences in the reconstructions prior to calibration.

Figure 3: Validation of our reconstruction method with climate model data perturbed by noise. The mean of 10 synthetic reconstructions and their

uncertainty ranges, derived using a climate model simulation from 1000 to 1990, (red, shown with 95% errorbars) is compared to the true NH 30-90N annual model mean (black) and the sub-sampled model data using instrumental coverage (green). The calibration interval is shown as vertical shaded region (note that results vary to some extent with calibration period, see text). The case with highest and lowest variance from the 10 synthetic reconstructions (blue) is also shown. Uncertainty ranges are smaller for timeperiods with temperatures close to the calibration period, while periods far away from them are more affected by scaling uncertainty.

Figure 4: Comparison of our reconstruction with two recent high-variance reconstructions. The CH-blend (short: solid, long: dashed) and Esper et al. (after Cook et al., 2004) timeseries have been smoothed by applying a 60 and then a 40-yr moving average (this yields nearly identical smoothing as that used by Moberg et al., 2005). For CH-blend land data, we also show the 95% uncertainty range in the amplitude of the reconstruction (dotted, see figure 2); for Moberg et al.'s reconstruction, the 95% range is shown by red dots. All reconstructions have been centered on the period 1860 to 1920 for a quantitative comparison with Moberg (which ends in 1925). Esper and CH-blend are shortened due to the application of the filter.

Figure 5: Comparison of two forward modelled subsurface temperature anomalies driven respectively by the composite extratropical borehole reconstruction and the CH-blend (short) reconstruction.

Figure 6. Contribution of external forcing to reconstructed decadal mean temperature. The top panel shows CH-blend and the instrumental record compared to simulations with an Atmosphere-Ocean General Circulation model and an EBM forced with estimates of volcanic, solar, and anthropogenic forcing (EBM simulation with natural forcing only dashed). The simulations are scaled to best fit the reconstruction (90% confidence interval for EBM fingerprint shaded). The bottom panel shows an estimate of the contribution to CH-blend (long) from individual forcings (volcanism, solar forcing, and greenhouse gas and aerosol forcing combined) and the associated 90% uncertainty range for the detectable signals, which are marked by an asterisk (*). Forcing fingerprints are centered to the period analyzed.

Figure 7. Similar as Figure 6, but comparing detection results to those for other high variance reconstructions. The top panel compares reconstructions by Esper et al. 2002, Moberg et al., 2005, Briffa et al., 2001 and CH-blend (details see table 2) with NH 30-90N average temperature from an EBM simulation forced with volcanic, solar, and anthropogenic forcing combined and instrumental data (green line). All top panel data are smoothed removing variance below 20 yrs, bottom panel data show fingerprints in the decadal time domain used for detection. The bottom panel compares the contribution from individual forcings (volcanism, solar forcing, and greenhouse gas and aerosol forcing combined, scaling factor see table 2) to individual records: Briffa (solid, fat), Esper (dotted), Moberg (dashed) and Ch-blend (solid, thin, shown with associated 90% uncertainty range). Forcing fingerprints are centered to the period analyzed.

Figure A1: Decadally smoothed or decadal proxy sites used for the reconstructions in standard deviation units. The records are explaned in Appendix A.

Table 1: 20-pt smoothed correlations between different proxy reconstructions. Reconstructions are Mann et al. (1999); Esper et al. (2002), Jones et al. (2001), Moberg et al. (2005), and CH-blend (short, Dark Ages, and long, calibrated to 30-90N temperature over land). The right column gives the average correlation between CH-blend, Mann, Jones and Esper.

	CH-blend	CH-DA	CH-long	Mann	Jones	Esper	Moberg	Mean and std
CH-blend	1.0	0.91	0.98	0.69	0.71	0.82	0.50	0.74+/-0.07
CH-dark		1.0	0.93	0.59	0.65	0.74	0.60	0.66+/-0.06
CH-short			1.0	0.61	0.63	0.82	0.55	0.69 +/-0.12
Mann et al.				1.0	0.70	0.52	0.44	0.60+/-0.13
Jones et al.					1.0	0.53	0.50	0.61+/-0.12
Esper et al.						1.0	0.28	0.52+/-0.09
Moberg et al.							1.0	0.41+/-0.12

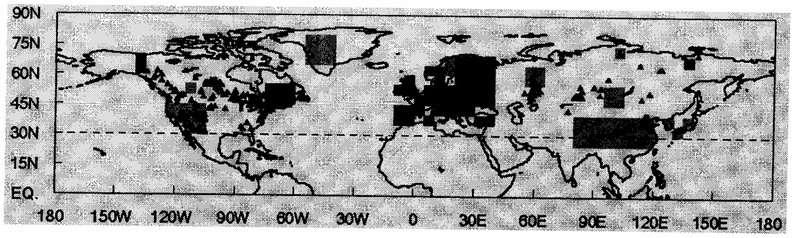
Table 2: Detection and attribution results for a range of high-variance paleoclimatic records of the last millennium. "Y" or "N" indicates that the response to external forcing is, or is not, detectable, with the number in brackets giving the best guess scaling factor for the model's fingerprint. "ghg+aer" denotes the anthropogenic fingerprint, which is a combination of greenhouse gas forcing and aerosol forcing from 1900 on, and an asterisk "*" marks that a result is sensitive to details of the analysis. The bottom row gives the standard deviation of the (decadally smoothed) residual and, in parantheses, the amount of decadal variance explained by external forcing. The 5-95% of the estimated contribution of external forcings to the early 20th century trend (1901-1950) are also given (marked by "20thc", except for Moberg which ends too early. Note that the non-detection of anthropogenic forcing in Moberg from 1000 on is largely due to an increase in residual variability, possibly partly due to forcing error.

Record	Briffa et al., (2001)	Esper et al., (2002)	CH-blend	Moberg	Moberg
Analysis Period	1402-1940	1400-1960	1270-1960	1270-1925	1001-1925
Represents:	20-90N land growing s.	20-90N land growing s.	30-90N all annual	0-90N all annual	0.90N all annual
Volcanic	Y (0.9)	Y (1.0)	Y (1.5)	Y (1.1)	Y(1.4)
Solar	N (-0.1)	N (-0.2)	N (0.5)	N(Yperiods)	Y(2.2)
Ghg+aer	Y (1.1)	Y (1.9)	Y (1.0)	Y (1.3)*	N
20thc Ghg+aer	10-99%	50-100%	22-52%	-	-
20thc volcanic	18-50%	18-51%	16-39%	-	-
20thc solar	<22%	<15%	<16%	-	-
20thc internal	13%	2%	30%	-	-
Residual std	0.11 (57%)	0.17 (60%)	0.10 (70%)	0.13(61%)	0.18 (52%)

Table A1: Correlations of local proxy data with decadally smoothed hemispheric mean instrumental data

Site	30-90N land	30-90N zonal
w. U.S.composite	0.71	0.61
w.U.S.Hughes	0.78	0.65
Alberta	0.82	0.79
Mackenzie	0.04	-0.14
Quebec	0.72	0.87
w. Greenland	0.57	0.57
Europe historical	0.89	0.83
n. Norway	0.83	0.89
w. Siberia short	0.72	0.51
w. Siberia	0.74	0.59
Taimyr	0.87	0.87
e. Siberia	0.42	0.50
Mongolia	0.78	0.66
e. Asia	0.84	0.90

Figures



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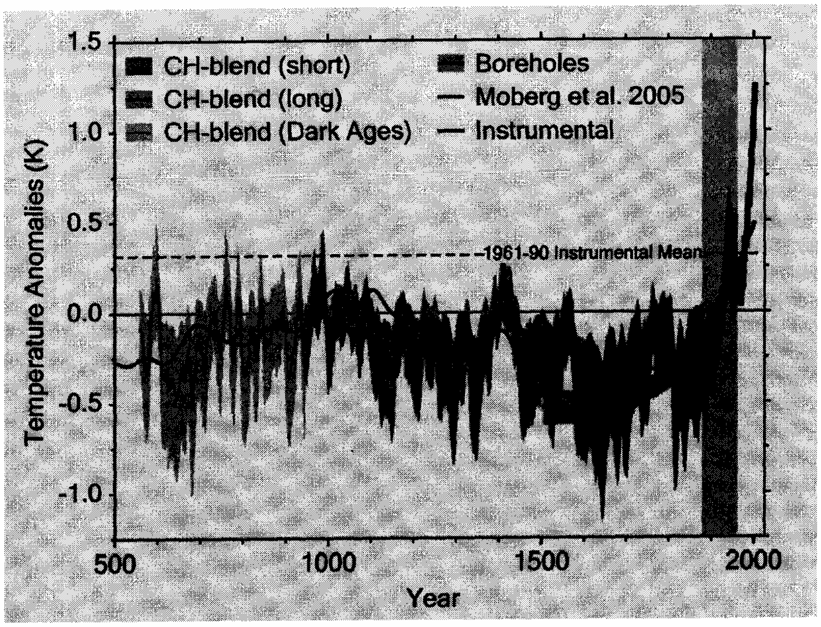

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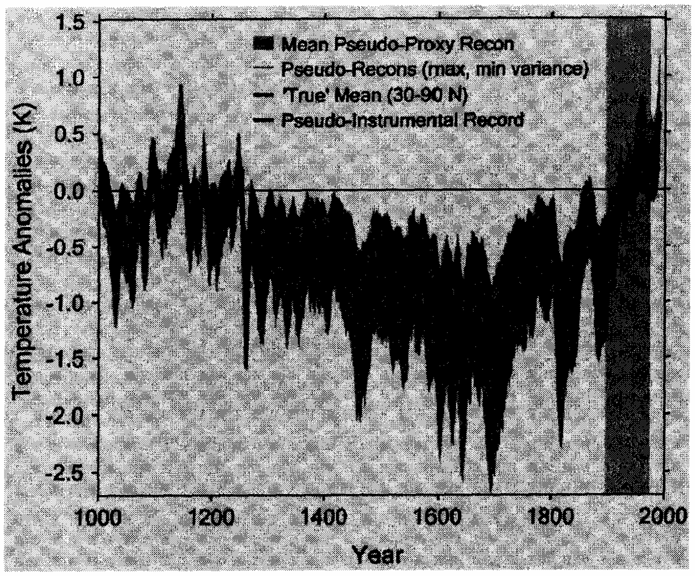

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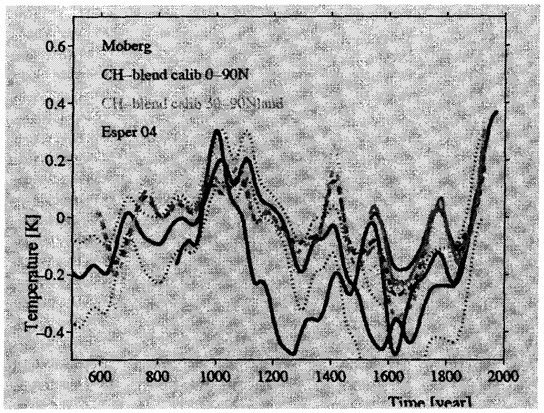

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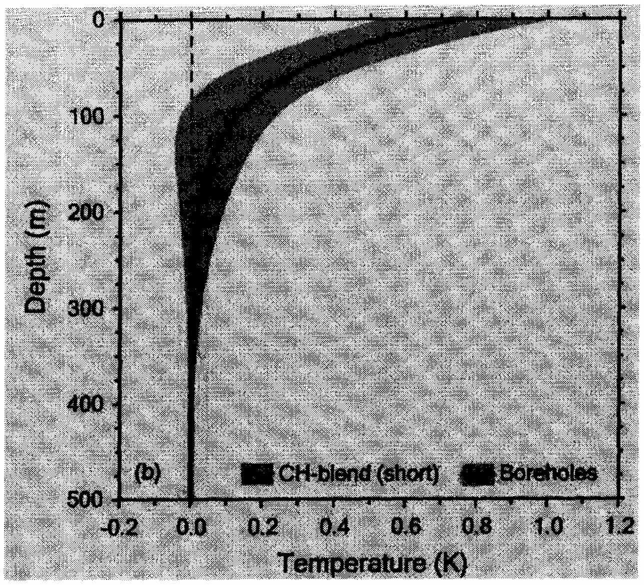

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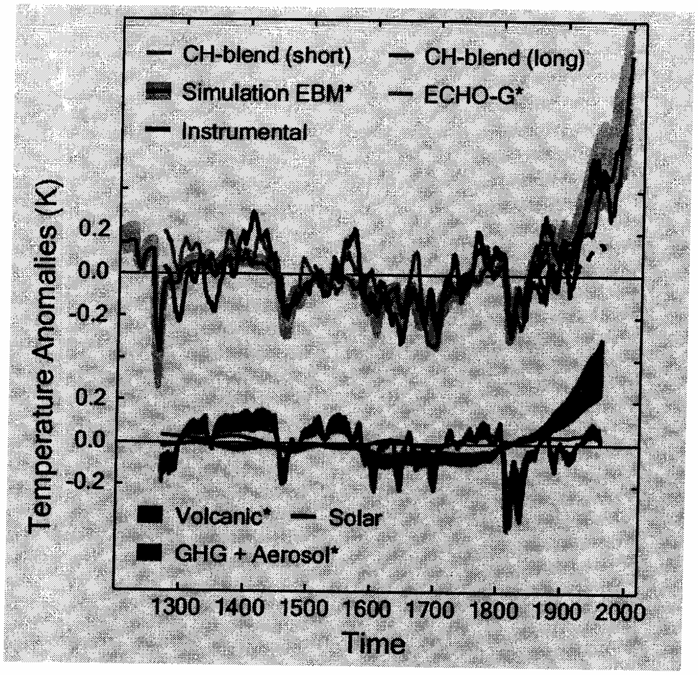

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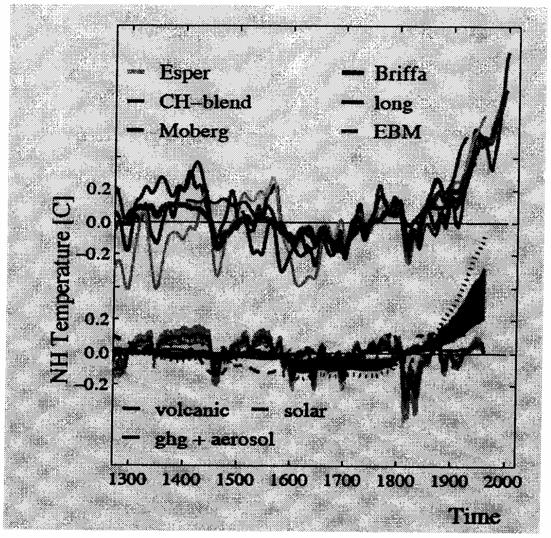

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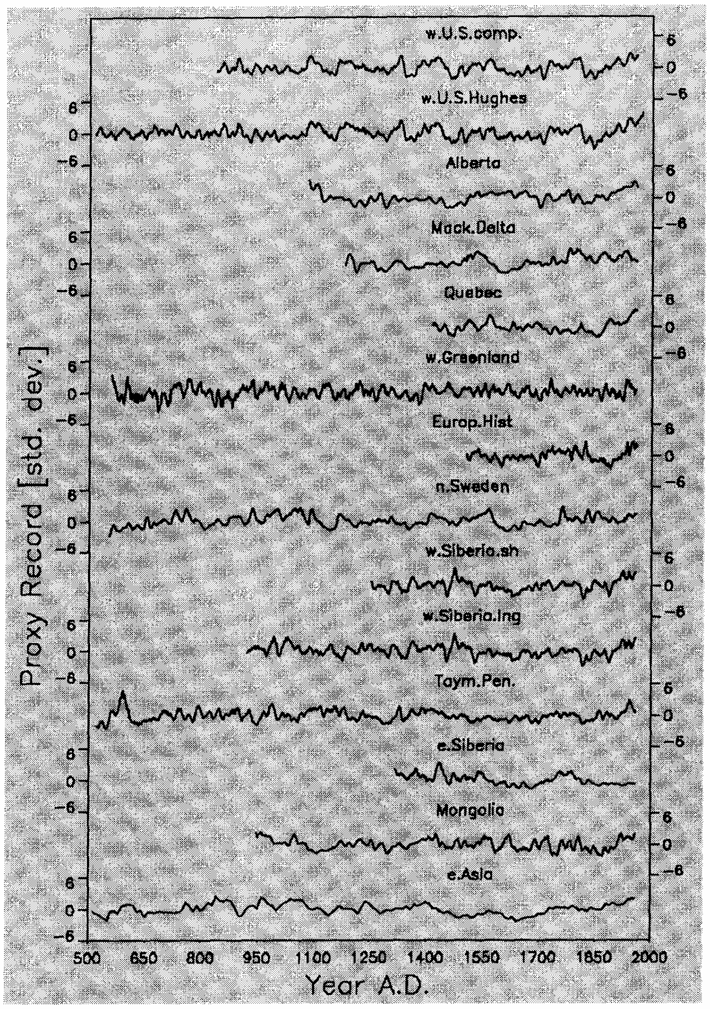

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Vol 440|20 April 2006|doi:10.1038/nature04679 nature

Climate sensitivity constrained by temperature reconstructions over the past seven centuries

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The magnitude and impact of future global warming depends on the sensitivity of the climate system to changes in greenhouse gas concentrations. The commonly accepted range for the equilibrium global mean temperature change in response to a doubling of the atmospheric carbon dioxide concentration[1], termed climate sensitivity, is 1.5–4.5 K (ref. 2). A number of observational studies[3–10], however, find a substantial probability of significantly higher sensitivities, yielding upper limits on climate sensitivity of 7.7 K to above 9 K (refs 3–8). Here we demonstrate that such observational estimates of climate sensitivity can be tightened if reconstructions of Northern Hemisphere temperature over the past several centuries are considered. We use large-ensemble energy balance modelling and simulate the temperature response to past solar, volcanic and greenhouse gas forcing to determine which climate sensitivities yield simulations that are in agreement with proxy reconstructions. After accounting for the uncertainty in reconstructions and estimates of past external forcing, we find an independent estimate of climate sensitivity that is very similar to those from instrumental data. If the latter are combined with the result from all proxy reconstructions, then the 5–95 per cent range shrinks to 1.5–6.2 K, thus substantially reducing the probability of very high climate sensitivity.

We use four palaeoreconstructions, namely a hemispheric reconstruction of mean annual temperatures[11], a maximum latewood density tree ring based reconstruction[12] for growing season temperatures over 20–90° N land, a revised and smoothed version of a record[13] that has been calibrated to 30–90° N land annual data[14], and our own new decadal reconstruction termed 'CH-blend' of annual average 30–90° N temperature[15] (Fig. 1). A version of CH-blend using 12 records extends from AD 1505 to AD 1960; and a reconstruction based on 9 sites ('CH-blend (long)') is used from AD 1270. Both reconstructions use a relatively small number of well spaced sites (often based on multiple records, including some regional reconstructions) throughout the reconstruction. CH-blend is consistent with independent estimates of temperatures from boreholes[15], and both CH-blend and CH-blend (long) agree well with a recent reconstruction[16] that incorporates records of lower temporal resolution. The reconstruction method has been tested using noise-perturbed climate model data from the same locations as used in the reconstruction[15]. Results show that the reconstruction of decadal temperatures is accurate and reliably preserves the variance of hemispheric-scale temperature variability.

For CH-blend, our estimate of climate sensitivity fully accounts for the uncertainty in the amplitude of the record[15]. For the other reconstructions, we use both the published reconstruction and a version that is recalibrated using our technique. This approach avoids introducing a low bias in our estimate of climate sensitivity based on the possibility that some reconstruction techniques underestimate past climate variability[17] (for details see Supplementary Information).

We conduct a large ensemble (>1,000) of simulations of the past 1,000 years with a 2.5-dimensional (latitude/longitude/depth) upwelling-diffusion energy balance model (EBM), with realistic land-sea distribution. The EBM is a variant of a seasonal model[18] that simulates time-dependent responses to external forcing, and includes the seasonal cycle (for details see Supplementary Information). The same model has been previously used to examine the relationship between reconstructed temperature and external forcing over the past millennium[19,20]. EBM simulations reproduce the large-scale temperature response of general circulation models, and have the advantage of being able to generate large ensembles.

The following two model parameters are important determinants of the large-scale response of climate models to external forcing[5] and have been systematically varied in our ensemble. First, the equilibrium climate sensitivity to a doubling of CO_2, α, which was varied in 0.5 K increments from 0.5 K to 10.0 K with an additional low value of 0.25 K. Second, effective ocean diffusivity κ in the upwelling-diffusive model[21], which was varied between $0.63\,\mathrm{cm^2\,s^{-1}}$ and $3.8\,\mathrm{cm^2\,s^{-1}}$.

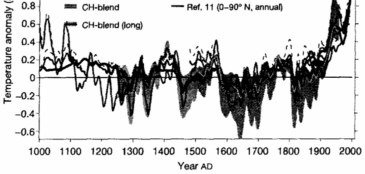

Figure 1 | Palaeoclimatic records compared to a climate model simulation. 'CH-blend' and 'CH-blend (long)' represent 30–90° N annual mean temperature (grey shading shows 10–90% ranges for uncertainty in the amplitude of the reconstruction); ref. 11 shows 0–90° N land temperature; ref. 14 shows 30–90° N land temperature; and ref. 12 shows 20–90° N land growing season temperature (dashed line indicates reconstructions rescaled[15]). The model ('Simulation') has a sensitivity of 2.5 K, mid-range ocean diffusivity and is driven with mid-range aerosol forcing. All data are smoothed to focus on multi-decadal variability and shown as anomalies relative to the period before 1800. The instrumental record for 30–90° N annual mean surface temperature ('Instrumental') is offset to match CH-blend between 1880 and 1960.

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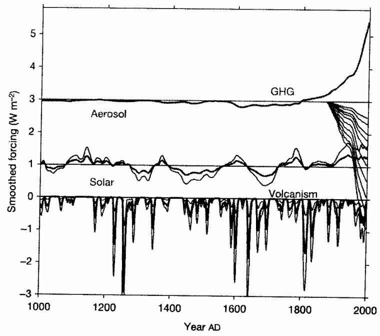

Figure 2 | Northern Hemisphere mean radiative forcing. Sub-annual forcing data are used in the climate model simulations, but a decadal filter is applied here for illustration only, to focus on timescales most relevant for the analysis. For tropospheric aerosol forcing (green), a range of forcing has been used; for solar (pink) and volcanic (blue) forcing, a best guess forcing (dark, thick line) and a gaussian uncertainty range has been used (2.5% and 97.5% limits are shown by light, thin lines, the lower limit for solar is on the zero line). For clarity, greenhouse gas ('GHG') and aerosol forcings are offset by 3 $\mathrm{W\,m^{-2}}$, and solar forcing by 1 $\mathrm{W\,m^{-2}}$.

This range embraces an observational estimate of $1.7 \pm 0.2\,\mathrm{cm^2\,s^{-1}}$ based on a global compilation of GEOSECS data of bomb tritium penetration into the world ocean[22] and a lower range[23] based on bomb ^{14}C of the order of $1\,\mathrm{cm^2\,s^{-1}}$. We have further tested our range of diffusivities by comparing simulated ocean warming with ocean heat content data[24]. We find that the smaller to mid-range values of κ yield results that compare most favourably with these data, consistent with the observation that most of the twentieth-century increase in ocean heat content is in the upper 1,000 m (Supplementary Fig. 1). Note that ocean diffusivity is of smaller importance for the simulations of the pre-industrial period, where forcings are mostly episodic and relatively small, than for the twentieth century. In the latter period, the rate of temperature increase is crucially influenced by ocean diffusivity, as large diffusivities tend to hide more warming in the oceans than small diffusivities (see Supplementary Information for more discussion). Our results are insensitive to attempts to constrain κ further. They are, however, conditional on ocean effective diffusivity being within the range we use.

Prior work[15,19,20,25] has established that various reconstructions of hemispheric temperature consistently show influence from volcanism and greenhouse gas variations, and less consistently from variations in solar radiation. We force the EBM simulations with a combination of solar, volcanic, greenhouse gas and tropospheric aerosol forcing to simulate hemispheric temperature change over the past millennium (Fig. 2). Greenhouse gas forcing is based on changes in trace gases from ice-core data, combined with IPCC estimates of radiative forcing for well-mixed greenhouse gases in the twentieth century. The estimate of solar forcing is based on ^{14}C data[26], scaled to the solar irradiance reconstruction of ref. 27 after reducing its amplitude by 20% to accommodate recent conclusions that the former estimate may have been large[28]. For volcanism, we use an update of a global reconstruction[20] based on ice-core data from Greenland and Antarctica. We account for the considerable uncertainty in solar and volcanic forcing by varying the total amplitude of each forcing time-series around its central estimate. We use Monte Carlo simulations based on a 50% standard deviation for solar forcing, and a 35% standard deviation for volcanic forcing (excluding the unphysical case of net negative forcing). The uncertainty in our results due to random errors in the magnitude of individual volcanic eruptions was estimated by sensitivity tests, indicating that errors in the magnitude of individual eruptions can cause a modest widening of the tail of the distribution (Supplementary Fig. 2; see Supplementary Information for more detail on forcings and their uncertainty).

We derive a probability density function (PDF) for climate sensitivity using a method related to one previously used for instrumental data[5,6] (see Methods section, and algorithm in Supplementary Information). Results for the CH-blend reconstruction, for which we have the most reliable uncertainty estimate[15], yield a 5–95% range for sensitivity of 1.4 K to 6.1 K and a median sensitivity of 2.6 K over the pre-instrumental period 1505–1850 (Fig. 3a). PDFs for climate sensitivity from the other reconstructions and the same period yield peak probabilities (modes) from 1.3 K to 3.6 K, and some of them suggest a moderate probability for climate sensitivity being high (see Supplementary Table 4). Reconstructions with higher amplitudes of past climate fluctuations generally suggest higher climate sensitivities than those with low variability. The range of the other free parameters, ocean diffusivity κ, and solar and volcanic forcing uncertainty, are used to fully explore uncertainties rather than to provide posterior information about best-fit values.

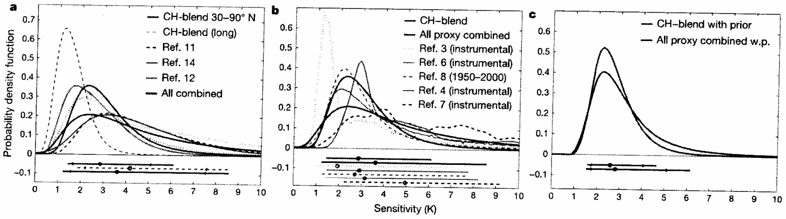

Figure 3 | Estimated probability density functions (PDFs) for equilibrium climate sensitivity to CO_2 doubling (in K). **a**, PDFs from a range of palaeoreconstructions using data to 1850 (dotted lines, based on rescaled data). The horizontal bars indicate the 5–95% range of PDFs (median is indicated by a dot, and 10th and 90th percentiles by a vertical bar). **b**, Comparison to other estimates of climate sensitivity based on instrumental data[3,4,6,7] over the twentieth century or 1950–2000[8]. All PDFs have been scaled to integrate to 1 between 0 and 10 for better comparison. **c**, Combined estimate using a result from instrumental data[8] as prior distribution, updated by the result from pre-industrial data ('w.p.').

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Consistent with that, the pre-industrial period does not provide constraints for ocean diffusivity, nor do results consistently favour a particular realization of forcing uncertainty, apart from a general preference for solar forcing on the low end of the range.

If all four reconstructions, both published and rescaled, are considered as equally likely realizations of the true hemispheric temperature evolution, the PDF that describes results from all four reconstructions combined yields a median sensitivity of 3.4 K and a 5–95% range of 1.2 K to 8.6 K. This renders negative climate feedbacks to CO_2 changes (corresponding to a climate sensitivity of under 1.1 K) very unlikely. As in other estimates of climate sensitivity, the upper tail is not well constrained. Note that the upper limit of the transient climate response, which governs the near-term magnitude of the climate response, tends to be better constrained from observations than equilibrium climate sensitivity[8].

Our results are remarkably consistent with PDFs for climate sensitivity that have been estimated from the instrumental record[3–8] (Fig. 3b) and that account for a differing level of uncertainty in forcings (most notably aerosol forcing[7]), ocean diffusivity and observations. The response of climate to pre-industrial forcing is governed (to a very reasonable approximation) by the same climate sensitivity. However, the uncertainties affecting each estimate are virtually independent, as the pre-industrial temperature reconstructions are virtually independent from instrumental temperatures for the second half of the twentieth century (decadal data before that are used for calibrating the palaeodata) and different forcing uncertainties affect each estimate. Therefore we can combine results from both to further constrain sensitivity. We use a version of the joint PDF for diffusivity and sensitivity, κ and α, from ref. 8 that is based on decadal instrumental data from 1950 to 2000 as a prior probability distribution (the use of a prior PDF of κ and α combined accounts for their dependence; Supplementary Fig. 5 shows a comparison between the published results for the entire twentieth century[8] and the prior used here). We have widened the upper tail of the ref. 8 estimate in sensitivity in order to conservatively account for further uncertainties and embrace other instrumental estimates (results are only moderately sensitive to this, for details see Supplementary Information). Bayes' theorem is then used to calculate a posterior probability based on data from the past millennium (Fig. 3c). The resulting 5–95% ranges for CH-blend shrink to 1.6 K to 4.6 K, and those for all proxy data combined to 1.5 K to 6.2 K. This result reduces the probability from 36% to 15% or less that climate sensitivity exceeds the upper limit of the IPCC range of 4.5 K.

As previously shown, the agreement between models and data is mostly driven by the temperature response to volcanism, which causes longer-term variability due to the changing statistics of volcanic eruptions (Fig. 2; see also Supplementary Table 3 for correlations between simulations and records on short and long timescales). A superposed epoch analysis previously showed that the EBM simulates the response-characteristics to volcanism very well[19]. The EBM does not simulate changes in atmospheric dynamics that have been associated with strong volcanic eruptions[29], but these changes do not much affect hemispheric annually (or growing season) averaged temperatures[30].

We also note that model uncertainties (beyond those that we account for) potentially affect all estimates of climate sensitivity. Although our results are conditional on the range of effective ocean diffusivity and the upwelling parameter being realistic, simulated ocean heat content changes in our best-fit simulation compare very well with recent data[24] (Supplementary Fig. 1). A simulation with the most likely sensitivity of 2.5 K also compares well to the low-frequency component of annual global temperatures from instrumental data (Supplementary Fig. 4).

We conclude that proxy-reconstructions of the pre-industrial period from 1270 to 1850 yield very similar estimates of climate sensitivity to those obtained from the virtually independent climate change over the twentieth century. This agreement increases our confidence in the overall reliability of these estimates based on twentieth-century changes. When both independent lines of evidence are combined, the resulting PDF for climate sensitivity narrows, yielding a very small probability for climate sensitivity exceeding 7 K (<3% based on all reconstructions combined, and <1% based on CH-blend).

METHODS

Estimating climate sensitivity. Our method of estimating the PDF of equilibrium climate sensitivity is related to a method used previously for instrumental data[5,6] and is briefly discussed here. A detailed algorithm can be found in the Supplementary Information. We simulate the time-space evolution of surface temperature over the past millennium forced with observed changes in solar, volcanic, greenhouse gas and sulphate aerosol forcing. We use a very large ensemble of EBM simulations with varying climate sensitivity α and ocean diffusivity κ, forced by different realizations of solar and volcanic forcing (f_{sol}, f_{vol}) to account for the most important uncertain parameters driving the simulated response. For the twentieth century, aerosol forcing, f_{aer}, is also varied. Each model simulation yields a time-space pattern of surface temperature response for each parameter $T(x,t,\alpha,\kappa,f_{\mathrm{sol}},f_{\mathrm{vol}},f_{\mathrm{aer}})$ that depends on time t and space x. For each palaeoreconstruction $\bar{T}_{\mathrm{palaeo}}$ (overbar denoting a spatial average), the simulated spatial temperature patterns are averaged over the latitude strip and season the reconstruction is calibrated to, and filtered to the time-resolution used for the analysis (annual reconstructions are filtered by a 5-yr running mean). All data are then centred from the beginning of the record to 1800 to focus on deviations from a mean climatic state of the past millennium and to avoid major variations in the climate state caused by anthropogenic forcing. We analyse reconstructions from the beginning of the record, but not before AD 1270 (as there are significant uncertainties in the radiative forcing effects of a very large eruption in 1258). The analysis focuses on the residual between the simulated record $\bar{T}$ and the observed record:

$$\mathrm{res}(t,\alpha,\kappa,f_{\mathrm{sol}},f_{\mathrm{vol}},f_{\mathrm{aer}}) = \bar{T}_{\mathrm{palaeo}}(t) - \bar{T}(t,\alpha,\kappa,f_{\mathrm{sol}},f_{\mathrm{vol}},f_{\mathrm{aer}}) \qquad (1)$$

Some combination of parameters will yield the residual with the smallest estimated variance, $\hat{r}^2_{\min} = \min\limits_{\text{parameters}}\left(\frac{\|(\mathrm{res}(t,\alpha,\kappa,f_{\mathrm{sol}},f_{\mathrm{vol}},f_{\mathrm{aer}}))\|^2}{n-1}\right)$, with $\|\mathrm{res}\|^2 = \sum_t \mathrm{res}(t,\alpha,\kappa,f_{\mathrm{sol}},f_{\mathrm{vol}},f_{\mathrm{aer}})^2$ and n denoting the length of the residual time series (yr). The difference between any square residual $\|\mathrm{res}\|^2$ and the minimum square residual $\|\mathrm{res}_{\min}\|^2$ will then be F-distributed[5]

$$\frac{\|\mathrm{res}(\alpha,\kappa,f_{\mathrm{aer}},f_{\mathrm{sol}},f_{\mathrm{vol}})\|^2 - \|\mathrm{res}_{\min}\|^2}{\hat{r}^2_{\min}} \propto mF(m,\nu) \qquad (2)$$

where m is the number of free parameters (4 for the pre-industrial case, 5 for the entire time-series) and ν is the number of degrees of freedom in $\mathrm{res}_{\min}$. Note that for autocorrelated data, ν will be smaller than n. Therefore we account for the number of effectively independent samples in the square residual (see Supplementary Information). Thus, the likelihood of the reconstruction given each set of model parameters and forcings $p(\mathrm{data}|\alpha,\kappa,f_{\mathrm{aer}},f_{\mathrm{vol}},f_{\mathrm{sol}})$ is estimated from the probability that its residual variability is statistically indistinguishable from the best-fit residual[5], given internal climate variability and non-climatic random errors in proxy data.

Bayes' theorem is used to derive the joint PDF of the parameters

$$p(\alpha,\kappa,f_{\mathrm{aer}},f_{\mathrm{vol}},f_{\mathrm{sol}}|\mathrm{data}) \propto p(\mathrm{data}|\alpha,\kappa,f_{\mathrm{aer}},f_{\mathrm{vol}},f_{\mathrm{sol}}) \bullet p(\alpha,\kappa,f_{\mathrm{aer}},f_{\mathrm{vol}},f_{\mathrm{sol}}) \qquad (3)$$

from the likelihood of the data and the prior probability of the parameters $p(\alpha,\kappa,f_{\mathrm{aer}},f_{\mathrm{vol}},f_{\mathrm{sol}})$. For results based on the proxy data alone, a uniform prior distribution for α is used for integration, which extends to a sensitivity of 10 K and then drops off to zero. Similarly, a prior for κ is used that is uniform over the range we cover (see main text), and normal prior distributions are used for solar and volcanic uncertainty (see Fig. 2). Where a prior distribution from the late twentieth century is used, the joint probability density function $p(\alpha,\kappa)$ from ref. 8 is applied instead of the uniform distribution.

This analysis is performed for every reconstruction. For reconstructions where the amplitude uncertainty can be fully accounted for (CH-blend and CH-blend (long)), this analysis is performed for the reconstruction scaled by a range of scaling factors β, representing uncertainty in the amplitude of the reconstruction (see Supplementary Information). We use the best-guess scaling and the 2.5th, 10th, 25th, 75th, 90th and 97.5th percentile of β. All PDFs resulting from these analyses $p(\mathrm{data}(\beta)|\alpha,\kappa,f_{\mathrm{aer}},f_{\mathrm{vol}},f_{\mathrm{sol}})$ are then averaged over β, weighted by the likelihood of each scaling β based on a normal distribution. This is a robust way of incorporating uncertainty in the reconstruction, as random errors in the reconstruction are directly accounted for in the residual.

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For reconstructions where the amplitude uncertainty cannot be fully estimated, we use both the published best guess and a rescaled best guess using our calibration method to estimate $p(\mathrm{data}|\alpha, \kappa, f_{\mathrm{aer}}, f_{\mathrm{vol}}, f_{\mathrm{sol}})$.

The resulting multi-dimensional likelihood is integrated over κ and forcing uncertainties f_{sol}, f_{vol} and f_{aer}, yielding a PDF for climate sensitivity. This is done both for each reconstruction individually, and for the average of the joint probabilities $p(\alpha, \kappa, f_{\mathrm{aer}}, f_{\mathrm{vol}}, f_{\mathrm{sol}}|\mathrm{data})$ from all reconstructions to derive an estimate of climate sensitivity from all records combined.

Our method to estimate sensitivity has been validated using synthetic data (see Supplementary Fig. 2), and tested by using an alternative method to estimate the likelihood based on scaling factors (see Supplementary Information).

Received 8 July 2005; accepted 28 February 2006.

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Supplementary Information is linked to the online version of the paper at www.nature.com/nature.

Acknowledgements We thank C. Forest for suggesting merging PDF estimates from results based on past-millennium climate changes with those based on twentieth-century climate change; J. Kenyon for technical support; M. Allen, F. Zwiers, A. Gelfand and M. Lavine for discussions; and C. Forest, N. Andronova, R. Knutti and J. Gregory for providing data. G.C.H. and T.J.C. were supported by NOAA, NOAA's office of global programmes and the DOE's office of biological and environmental research. G.C.H. was additionally supported by NSF.

Author Contributions G.C.H. developed and implemented the method to estimate sensitivity and to calibrate proxy records, T.J.C. provided the reconstruction of past forcing and developed the CH-blend reconstruction, W.T.H. performed the EBM simulation, and D.J.F. derived the prior estimate of sensitivity from instrumental data 1950–2000. G.C.H., T.J.C. and W.T.H analysed the results.

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52. We thank M. Wagener for excellent assistance with animal experiments and G. Barsh and members of the Kingsley lab for helpful comments on the manuscript. A.M.H. is a trainee of the Medical Scientist Training Program at Stanford, supported by NIH training grant 5T32GM07365 (NIGMS). The initial stages of the genetic cross were supported by a Stanford-SmithKline Beecham research award. D.M.K. is an HHMI assistant investigator.

18 April 2000; accepted 6 June 2000

Causes of Climate Change Over the Past 1000 Years

Thomas J. Crowley

Recent reconstructions of Northern Hemisphere temperatures and climate forcing over the past 1000 years allow the warming of the 20th century to be placed within a historical context and various mechanisms of climate change to be tested. Comparisons of observations with simulations from an energy balance climate model indicate that as much as 41 to 64% of preanthropogenic (pre-1850) decadal-scale temperature variations was due to changes in solar irradiance and volcanism. Removal of the forced response from reconstructed temperature time series yields residuals that show similar variability to those of control runs of coupled models, thereby lending support to the models' value as estimates of low-frequency variability in the climate system. Removal of all forcing except greenhouse gases from the ~1000-year time series results in a residual with a very large late-20th-century warming that closely agrees with the response predicted from greenhouse gas forcing. The combination of a unique level of temperature increase in the late 20th century and improved constraints on the role of natural variability provides further evidence that the greenhouse effect has already established itself above the level of natural variability in the climate system. A 21st-century global warming projection far exceeds the natural variability of the past 1000 years and is greater than the best estimate of global temperature change for the last interglacial.

The origin of the late-20th-century increase in global temperatures has prompted considerable discussion. Detailed comparisons of climate model results with observations (*1*) suggest that anthropogenic changes, particularly greenhouse gas (GHG) increases, are probably responsible for this climate change. However, there are a number of persistent questions with respect to these conclusions that involve uncertainties in the level of low-frequency unforced variability in the climate system (*2*) and whether factors such as an increase in solar irradiance or a reduction in volcanism might account for a substantial amount of the observed 20th-century warming (*1, 3–10*). Although many studies have addressed this issue from the paleoclimate perspective of the past few centuries (*3–10*), robust conclusions have been hampered by inadequate lengths of the time series being evaluated. Here I show that the agreement between model results and observations for the past 1000 years is sufficiently compelling to allow one to conclude that natural variability plays only a subsidiary role in the 20th-century warming and that the most parsimonious explanation for most of the warming is that it is due to the anthropogenic increase in GHG.

Data

The data used in this study include physically based reconstructions of Northern Hemisphere temperatures and indices of volcanism, solar variability, and changes in GHGs and tropospheric aerosols.

Northern Hemisphere temperatures. Four indices of millennial Northern Hemisphere temperature have been produced over the past 3 years (*11–14*). The analysis here uses the mean annual temperature reconstructions of Mann *et al.* (*11*) and of Crowley and Lowery (CL) (*12*), because the energy balance model used in this study calculates only this term [the other records (*13*, *14*) are estimates of warm-season temperature at mid-high latitudes]. The Mann *et al.* reconstruction was determined (*8*) by first regressing an empirical orthogonal function analysis of 20th-century mean annual temperatures against various proxy indices (such as tree rings, corals, and ice cores). Past changes in temperature are estimated from variations in the proxy data (*15*). The Mann *et al.* reconstruction has a varying number of records per unit of time (although the number in the earlier part of the record is still greater than in CL). The CL reconstruction is a more heterogeneous mix of data than the Mann *et al.* reconstruction, but the number of records is nearly constant in time. It is a simple composite of Northern Hemisphere climate records and was scaled (*12*) to temperature using the instrumental record (*16*) in the overlap interval 1860–1965. The instrumental record was substituted for the proxy record after 1860 for two reasons: (i) there were too few proxy data in the CL time series after 1965 to reconstruct temperatures for this interval, and (ii) the original CL reconstruction indicated a "warming" over the interval 1885–1925 that is at variance with the instrumental record. This difference has been attributed (*11*, *17*) to

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an early CO_2 fertilization effect on tree growth. The significance of this decision will be further discussed below; model-data correlations presented in the study include both the original proxy record and the substituted instrumental time series.

Despite the different number and types of data and different methods of estimating temperatures, comparison of the decadally smoothed variations in each reconstruction (Fig. 1) indicates good agreement ($r = 0.73$ for 11-point smoothed correlations over the preanthropogenic interval 1005–1850, with $P < 0.01$). Both records [and the Jones *et al.* (*13*) and Briffa (*14*) reconstructions] show the "Medieval Warm Period" in the interval ~1000–1300, a transition interval from about 1300–1580, the 17th-century cold period, the 18th-century recovery, and a cold period in the early 19th century. Even many of the decadal-scale variations in the Medieval Warm Period are reproducible (*12*), and both reconstructions [and (*13*, *14*)] indicate that peak Northern Hemisphere warmth during the Middle Ages was less than or at most comparable to the mid-20th-century warm period (~1935–1965). This result occurs because Medieval temperature peaks were not synchronous in all records (*12*). The two temperature reconstructions also agree closely in estimating an ~0.4°C warming between the 17th-century and the mid-20th-century warm period (*18*).

Volcanic forcing. There is increasing evidence (*3*, *7–10*) that pulses of volcanism significantly contributed to decadal-scale climate variability in the Little Ice Age. Although some earlier studies (*9*, *10*) of forced climate change back to 1400 used a composite ice core index of volcanism (*19*), which has a different number of records per unit of time, the present study primarily uses two long ice core records from Crete (*20*) and the Greenland Ice Sheet Project 2 (GISP2) (*21*) on Greenland, with a small augmentation from a study of large eruptions recorded in ice cores from both Greenland and Antarctica (*22*). This approach avoids the potential for biasing model results versus time because of changes in the number of records. Because Southern Hemisphere volcanism north of 20°S influences Northern Hemisphere temperatures, the ice core volcano census samples records down to this latitude. The volcanism record is based on electrical conductivity (*20*) or sulfate measurements (*21*), and a catalog of volcanic eruptions (*23*) was used to remove local eruptions (*24*) and identify possible candidate eruptions in order to weight the forcing according to latitude. Eruptions of unknown origin were assigned a high-latitude origin unless they also occurred in Antarctic ice core records (*22*).

The relative amplitude of volcanic peaks was converted to sulfate concentration by first scaling the peaks to the 1883 Krakatau peak in the ice cores. Although earlier studies (*9*, *10*) linearly converted these concentration changes to radiative forcing changes, subsequent comparison (*25*) of the very large 1259 eruption [eight times the concentration of sulfate in ice cores from Krakatau and three times the size of the Tambora (1815) eruption (*21*)] with reconstructed temperatures (*11–14*) failed to substantiate a response commensurate with a linearly scaled prediction of an enormous perturbation of ~25 W/m^2 (*26*). Calculations (*27*) suggest that for stratospheric sulfate loadings greater than about 15 megatons (Mt), increasing the amount of sulfate increases the size of aerosols through coagulation. Because the amount of scattered radiation is proportional to the cross-sectional area, and hence to the 2/3 power of volume (or mass), ice core concentrations estimated as >15 Mt were scaled by this amount (*25*). Aerosol optical depth was converted to changes in downward shortwave radiative forcing at the tropopause, using the relationship discussed in Sato *et al.* (*28*). There is significant agreement (*29*) between the 1000-year-long volcano time series and the concentration-modified Robock and Free (*19*) times series (Fig. 2A). Both proxy records show the general trends estimated from ground-based observations of aerosol optical depth (*28*): the pulse of eruptions in the early 20th century and the nearly 40-year quiescent period of volcanism between about 1920–1960. Because volcano peaks are more difficult to determine in the expanded firn layer of snow/ice cores, updated estimates of Northern Hemisphere radiative forcing from Sato *et al.* were used to extend proxy time series from 1960 to 1998.

Solar forcing. There has been much discussion about the effect of solar variability on decadal-to-centennial–scale climates (*3*, *6*, *8–10*). An updated version of a reconstruction by Lean *et al.* (*5*) that spans the interval 1610–1998 was used to evaluate this mechanism [for reference, Free and Robock (*10*) obtained comparable solar-temperature correlations for the interval 1700–1980 using the Lean *et al.* and alternate Hoyt and Schatten (*4*) solar reconstructions]. The Lean *et al.* time series has been extended to 1000 by splicing in different estimates of solar variability based on cosmogenic isotopes. These estimates were derived from ice core measurements (*30*) of ^{10}Be, residual ^{14}C from tree ring records (*31*), and an estimate of ^{14}C from ^{10}Be fluctuations (*30*). The justification for including the latter index is that neither of

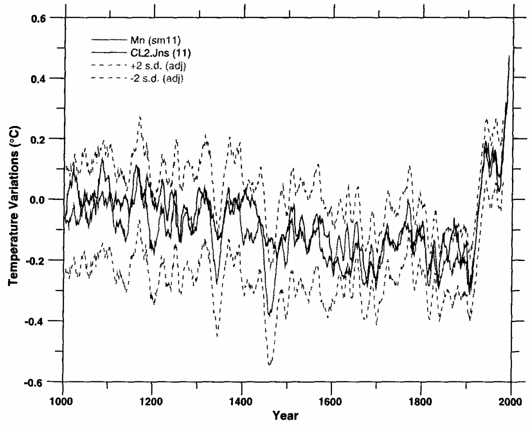

Fig. 1. Comparison of decadally smoothed Northern Hemisphere mean annual temperature records for the past millennium (1000–1993), based on reconstructions of Mann *et al.* (Mn) (*11*) and CL (*12*). The latter record has been spliced into the 11-point smoothed instrumental record (*16*) in the interval in which they overlap. CL2 refers to a new splice that gives a slightly better fit than the original (*12*). The autocorrelation of the raw Mann *et al.* time series has been used to adjust (adj) the standard deviation units for the reduction in variance on decadal scales.

the first two splices yields a Medieval solar maximum comparable to that of the present. Because of concerns about biasing results too much by the latter period, which has much more information than the former, the Bard ^{14}C calculation was included so as to obtain a greater spread of potential solar variations and to allow testing of suggestions (*32*) that solar irradiance increases could explain the Medieval warming.

Once the splices were obtained, the records were adjusted to yield the potential ~0.25% change in solar irradiance on longer time scales (*33*). Because two of the solar proxies indicate that minimum solar activity occurred in the 14th century, the 0.25% range was set from that time to the present rather than from the 17th century, as was done by Lean *et al.* [the adjustment is very small for the different solar indices in the 14th century (~0.05 W/m^2)]. The 20th-century increase in estimated net radiative forcing from low-frequency solar variability is about 10 to 30% greater than estimated from an independent method (*34*). An example of one of the splices is illustrated in Fig. 2B, and the three composites (Fig. 2C) show the pattern of potential solar variability changes used in this study.

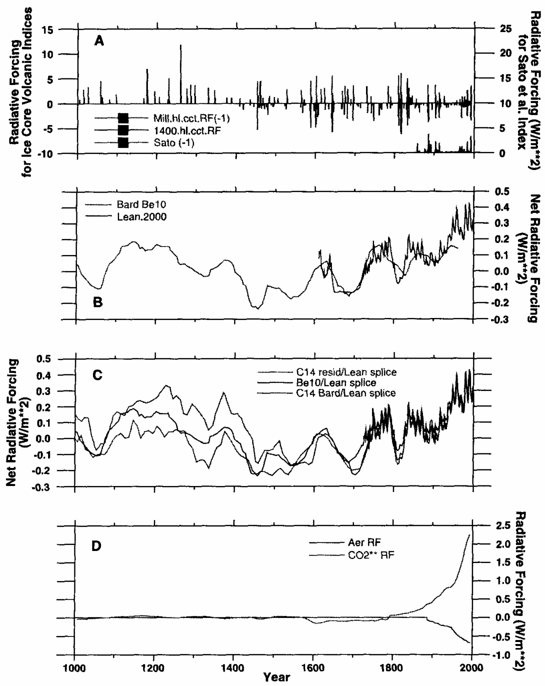

Fig. 2. Forcing time series used in the model runs (note scale changes for different panels). (**A**) (Red) Ice core millennial volcanism time series from this study (multiplied by −1 for display purposes); (blue) ice-core Robock and Free (*19*) reconstruction from 1400 to the present after adjustments discussed in (*9*) and (*25*); and (green) Sato et *al*. (*28*) Northern Hemisphere radiative forcing, updated to 1998 and multiplied by −1 for display purposes. (**B**) Example of splice for solar variability reconstructions, using the ^{10}Be-based irradiance reconstruction of (*30*) (red) and the reconstruction of solar variability from Lean *et al*. (*5*) (blue). (**C**) Comparison of three different reconstructions of solar variability based on ^{10}Be measurements (*30*) (blue), ^{14}C residuals (*31*) (red), and calculated ^{14}C changes based on ^{10}Be variations (*30*) (green). (**D**) Splice of CO_2 radiative forcing changes 1000–1850 (*35*) (red) and post-1850 anthropogenic changes in equivalent GHG forcing and tropospheric aerosols (blue).

Anthropogenic forcing. The standard equivalent radiative forcing for CO_2 and other well-mixed trace gases (methane, nitrous oxides, and chlorofluorocarbons) is used after 1850 (Fig. 2D). Pre-1850 CO_2 variations, including the small minimum from about 1600–1800, are from Etheridge *et al.* (*35*). Radiative forcing effects were computed based on updated radiative transfer calculations (*36*). The well-constrained change in GHG forcing since the middle of the last century is about four times larger than the potential changes in solar variability based on the reconstructions of Lean *et al.* (*5*) and Lockwood and Stamper (*34*).

Tropospheric aerosols consider only the direct forcing effect (that is, no cloud feedback), whose global level has been estimated as being about −0.4 W/m^2 (*37*), with the Northern-to-Southern-Hemisphere ratio being in the range of 3 to 4 (*38*). Because there is an approximate offset in the radiative effects of stratospheric and tropospheric ozone (*37*), and its total net forcing is on the order of +0.2 W/m^2 (*37*) and is applicable only to the late 20th century, this GHG was not further considered. Other anthropogenic forcing was not included because evaluations by the Intergovernmental Panel for Climate Change (IPCC) (*37*) indicate that the confidence in these estimates is very low.

Model

A linear upwelling/diffusion energy balance model (EBM) was used to calculate the mean annual temperature response to estimated forcing changes. This model (*39*) calculates the temperature of a vertically averaged mixed-layer ocean/atmosphere that is a function of forcing changes and radiative damping. The mixed layer is coupled to the deep ocean with an upwelling/diffusion equation in order to allow for heat storage in the ocean interior. The radiative damping term can be adjusted to embrace the standard range of IPCC sensitivities for a doubling of CO_2. The EBM is similar to that used in many IPCC assessments (*40*) and has been validated (*39*) against both the Wigley-Raper EBM (*40*) and two different coupled ocean-atmosphere general circulation model (GCM) simulations (*41*). All forcings for the model runs were set to an equilibrium sensitivity of 2°C for a doubling of CO_2. This is on the lower end of the IPCC range (*42*) of 1.5° to 4.5°C for a doubling of CO_2 and is slightly less than the IPCC "best guess" sensitivity of 2.5°C [the inclusion of solar variability in model calculations can decrease the best fit sensitivity (*9*)]. For both the solar and volcanism runs, the calculated temperature response is based on net radiative forcing after adjusting for the 30% albedo of the Earth-atmosphere system over visible wavelengths.

Results

The modeled responses to individual forcing terms (Fig. 3A) indicate that the post-1850 GHG and tropospheric aerosol changes are similar to those discussed in IPCC (*42*). CO_2 temperature variations are very small for the preanthropogenic interval, although there is a 0.05°C decrease in the 17th and 18th centuries that reflects the CO_2 decrease of ~6 parts per million in the original ice core record (*35*). Solar variations are on the order of 0.2°C, and volcanism causes large cooling (*43*) in the Little Ice Age (*3–7*, *9*, *10*). Averaged over the entire preanthropogenic interval (Table 1), 22 to 23% of the decadal-scale variance can be explained by volcanism ($P <$ 0.01). However, over the interval 1400–1850, the volcanic contribution increases to 41 to 49% ($P < 0.01$), thereby indicating a very important role for volcanism during the Little Ice Age.

The sun-climate correlations for the interval 1000–1850 vary substantially by choice of solar index (Table 1), with explained variance ranging from as low as 9% ($P < 0.01$) for the ^{14}C residual index (*31*) to as high as 45% ($P < 0.01$) for the Bard *et al.* (*30*) ^{14}C solar index, which reconstructs a Medieval solar warming comparable to the present century but only about 0.1°C greater than predicted by the other solar indices (Fig. 3A). The large range in correlations for the solar records emphasizes the need to determine more precisely the relative magnitude of the real Medieval solar warming peak.

The joint effects of solar variability and volcanism (Fig. 3B) indicate that the combination of these effects could have contributed 0.15° to 0.2°C to the temperature increase (Fig. 1) from about 1905–1955, but only about one-quarter to the total 20th-century warming. The combined warmth produced by solar variability and volcanism in the 1950s is similar in magnitude but shorter in duration

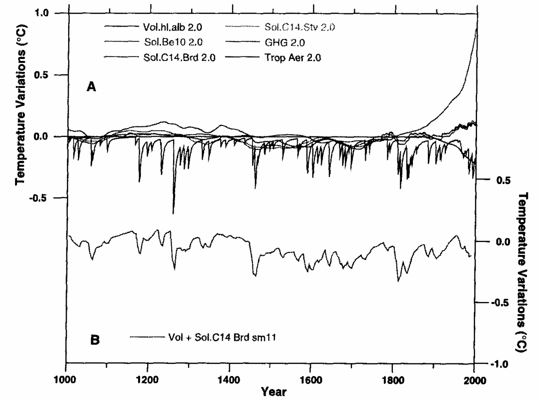

Fig. 3. (**A**) Model response to different forcings, calculated at a sensitivity of 2.0°C for a doubling of CO_2. (**B**) Example of the combined effect of volcanism and solar variability (with 11-point smoothing), using the Bard *et al.* (*30*) ^{14}C index.

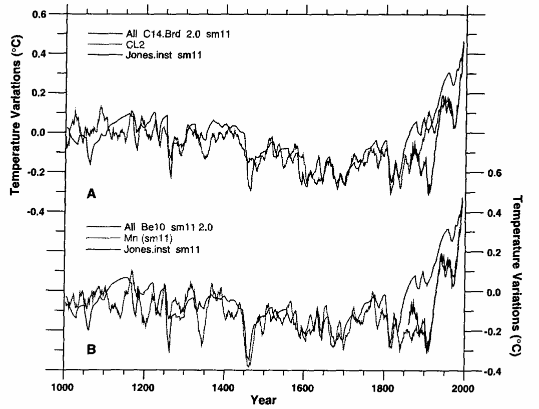

Fig. 4. Comparison of model response (blue) using all forcing terms (with a sensitivity of 2.0°C) against (**A**) the CL (*12*) data set spliced into the 11-point smoothed Jones *et al.* (*16*) Northern Hemisphere instrumental record, with rescaling as discussed in the text and in the Fig. 1 caption; and (**B**) the smoothed Mann *et al.* (*11*) reconstruction. Both panels include the Jones *et al.* instrumental record for reference. To illustrate variations in the modeled response, the ^{14}C calculation from Bard *et al.* (*30*) has been used in (A) and the ^{10}Be estimates from (*30*) have been used in (B).

Table 1. Correlations of volcanism (volc.) and solar variability (sol.) for the preanthropogenic interval, with percent variance shown in parentheses. The different solar time series reflect the three different solar indices used in this study. The Mann *et al.* time series (*11*) has been smoothed with an 11-point filter. CL was smoothed in the original analysis (*12*). Different abbreviations for solar forcing refer to the different indices discussed in the text: ^{10}Be and ^{14}C calculations are from Bard *et al.* (*30*); ^{14}C residuals are from Stuiver *et al.* (*31*).

Volc. vs. Mann *et al.* (1000–1850)	0.48 (23%)
Volc. vs. CL (1000–1850)	0.47 (22%)
Volc. vs. Mann *et al.* (1400–1850)	0.70 (49%)
Volc. vs. CL (1400–1850)	0.64 (41%)
Sol (^{10}Be) vs. Mann *et al.*	0.45 (20%)
Sol (^{14}C Bard) vs. Mann *et al.*	0.56 (31%)
Sol (^{14}C Stuiver) vs. Mann *et al.*	0.37 (14%)
Sol (^{10}Be) vs. CL	0.42 (18%)
Sol (^{14}C Bard) vs. CL	0.67 (45%)
Sol (^{14}C Stuiver) vs. CL	0.30 (9%)

than the warmth simulated by these mechanisms in the Middle Ages. The variations in the past few decades resulting from the combination of solar variability and volcanism is 0.2°C less than the 1955 peak.

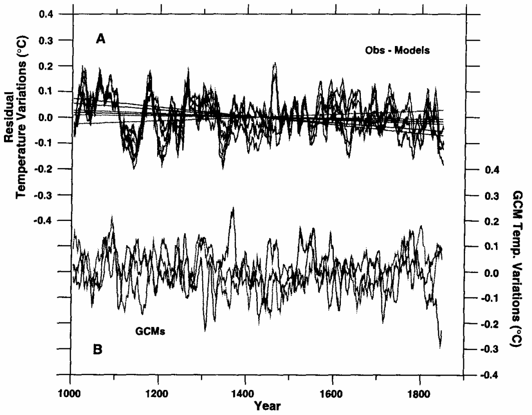

Fig. 5. Analysis of preanthropogenic residuals in the paleo records. (**A**) Estimates of residuals using all combinations of temperature reconstructions and total forcing (including three different solar indices), with trend lines fitted for each of the six residuals. (**B**) Control runs (detrended) from three different coupled ocean-atmosphere models (*46*): the NOAA/GFDL model (NOAA Geophysical Fluid Dynamics Laboratory) (orange), the HadCM3 model (Hadley Centre at the UK Meteorological Office, Bracknell, UK) (blue), and the ECHAM3/LSG model (European Centre/University of Hamburg/Max Planck Institute für Meteorologie) (brown). For the sake of comparison with the paleo data, the GCM runs have been truncated to the same length as the paleo residuals and have been plotted using the arbitrary starting year of 1000.

Table 2. Correlations between model runs with combined forcing and the Mann *et al.* (*11*) and CL (*12*) time series. Correlations have been subdivided into the following three categories: Top set: Correlations for all the preanthropogenic interval 1005–1850 of model response to combined forcing ("All") with different solar indices (Table 1) and the 11-point smoothed Mann *et al.* time series and CL2 record spliced into the 11-point smoothed Jones *et al.* (*16*) time series. Middle set: Correlations over the entire interval analyzed. Bottom set: Correlations and variance explained for the interval 1005–1993 using the original CL2 reconstruction from 1005–1965, with the smoothed Jones *et al.* (*16*) record added from 1965–1993.

Summary of pre-1850 correlations, with variance shown in parentheses	
All ^{10}Be (solar) vs. Mann (sm11)	0.64 (41%)
All ^{14}C Brd (solar) vs. Mann (sm11)	0.68 (46%)
All ^{14}C Stv (solar) vs. Mann (sm11)	0.65 (42%)
All ^{10}Be (solar) vs. CL2.Jns11	0.69 (48%)
All ^{14}C Brd (solar) vs. CL2.Jns11	0.80 (64%)
All ^{14}C Stv (solar) vs. CL2.Jns11	0.68 (47%)
Summary of correlations for 1005–1993, with variance shown in parentheses	
All ^{10}Be (solar) vs. Mann (sm11)	0.68 (46%)
All ^{14}C Brd (solar) vs. Mann (sm11)	0.73 (53%)
All ^{14}C Stv (solar) vs. Mann (sm11)	0.67 (45%)
All ^{10}Be (solar) vs. CL2.Jns11	0.66 (43%)
All ^{14}C Brd (solar) vs. CL2.Jns11	0.77 (59%)
All ^{14}C Stv (solar) vs. CL2.Jns11	0.64 (41%)
Summary of correlations for 1005–1993 against unfiltered CL time series, with 11-point smoothed Jones et al. (16) record spliced in from 1965–1993	
All ^{10}Be (solar) vs. CL2.Jns 11	0.75 (57%)
All ^{14}C Brd (solar) vs. CL2.Jns11	0.83 (69%)
All ^{14}C Stv (solar) vs. CL2.Jns11	0.74 (54%)

Combining all forcing (solar, volcanism, GHG, and tropospheric aerosols) results in some striking correspondences between the model and the data over the preanthropogenic interval (Fig. 4). Eleven-point smoothed correlations (*44*) for the preanthropogenic interval (Table 2) indicate that 41 to 64% of the total variance is forced ($P < 0.01$). The highest correlations are obtained for the CL time series, which has slightly more Medieval warmth than the Mann *et al.* reconstruction, and for the forcing time series that includes the largest solar estimate of Medieval warmth. Forced variability explains 41 to 59% of the variance ($P < 0.01$) over the entire length of the records. Although simulated temperatures agree with observations in the late 20th century, simulations exceed observations by ~0.1° to 0.15°C over the intervals 1850–1885 and 1925–1975, with a larger discrepancy between ~1885–1925 that reaches a maximum offset of ~0.3°C from ~1900–1920. However, decadal-scale patterns of warming and cooling are still simulated well in these offset intervals. A sensitivity test (*45*) comparing forcing time series with and without solar variability indicates that changes caused by volcanism and CO_2 are responsible for the simulated temperature increase from the mid- to late 19th century to the early 20th century, thereby eliminating uncertainties in solar forcing as the explanation for the temperature differences between the model and the data. Also shown in Fig. 4A is the CL reconstruction with the "anomalous" warm interval (~1885–1925) discussed above. For this reconstruction, 55 to 69% of the variance from 1005–1993 can be explained by the model ($P < 0.01$).

Another means of evaluating the role of forced variability is to determine residuals by subtracting the different model time series from the two paleo time series over the preanthropogenic interval (Fig. 5A). The trend lines for three of these residuals are virtually zero, and there is only about a ±0.1°C trend for the other three residuals. Because the pre-1850 residuals represent an estimate of the unforced variability in the climate system, it is of interest to compare the smoothed residuals with smoothed estimates of unforced variability in the climate system from control runs of coupled ocean-atmosphere models. There is significant agreement (Fig. 5B and Table 3) between the smoothed standard deviations of the GCMs (*46*) and paleo residuals (*47*). These results support a basic assumption in optimal detection studies (*1*) and previous conclusions (*48*) that the late-20th-century warming cannot be explained by unforced variability in the ocean-atmosphere system. However, a combination of GHG, natural forcing, and ocean-atmosphere variability could have contributed to the 1930–1960 warm period (*1*, *9*, *10*, *49*).

One way to highlight the unusual nature

of the late-20th-century warmth is to subtract all forcing other than CO_2 (solar, volcanism, and tropospheric aerosols) and examine the late-20th-century residuals within the context of the previous 1000 years (Fig. 6). There is an unprecedented residual warming in the late 20th century that matches the warming predicted by GHG forcing. Projection of the "Business As Usual" (BAU) scenario into the next century using the same model sensitivity of 2.0°C indicates that, when placed in the perspective of the past 1000 years, the warming will reach truly extraordinary levels (Fig. 6). The temperature estimates for 2100 also exceed the most comprehensive estimates (*50*) of global temperature change during the last interglacial (~120,000 to 130,000 years ago)—the warmest interval in the past 400,000 years.

Discussion

Forcing a linear energy balance model with independently derived time series of volcanism and solar variability indicates that 41 to 64% of the preanthropogenic low-frequency variance in temperature can be accounted for by external factors. These results were obtained without any retuning of the climate model. When the same sensitivity for the preanthropogenic interval is used for the past 150 years, there is good agreement with temperatures in the late 20th century. Some caution is needed in interpreting the agreement between models and data for a 2.0°C sensitivity, because a more detailed analysis of uncertainties (*47*) might yield slightly different sensitivities than simulated here (*51*). Also, statistical methods better constrain the minimum than the maximum sensitivity (*52*). If paleo records are shown to have had larger amplitude than used in this study (*18*), model-data correlations should still be valid but the best fit sensitivity would be greater.

The largest model-data discrepancy over the entire past 1000 years is from ~1885–1925, peaking in ~1900–1920. Although such differences could reflect random uncertainties in the paleo reconstructions (Fig. 1) or forcing fields, the consistent offset between model and data suggests the need to identify one or more specific explanations for the differences. For example, two factors that could be contributing to the model-data differences in this interval are: (i) mid-latitude land clearance may have increased albedo and caused slightly greater cooling than simulated (*53*), and (ii) warming may be underestimated in the early stage of the instrumental record because of sparse data coverage (*16*). As discussed above, there is evidence for warming in some of the high-elevation data in the original CL (*12*) reconstruction and in the comprehensive Overpeck *et al.* (*6*) Arctic synthesis. Many alpine glaciers started to retreat around 1850 (*54*). There is also some evidence for warming of tropical oceans in the late 19th century (*16*), but the data are very sparse. More data and model analyses would be required to test these and other possible explanations.

Analysis of residuals in the pre-1850 interval reveals little or no trend. If the pre-1850 temperature reconstructions, forcing estimates, and model responses are correct, the model-data agreement in this study suggests that factors such as thermohaline circulation changes (*55*) may have played only a secondary role with respect to modifying hemispheric temperatures over the past 1000 years (*56*).

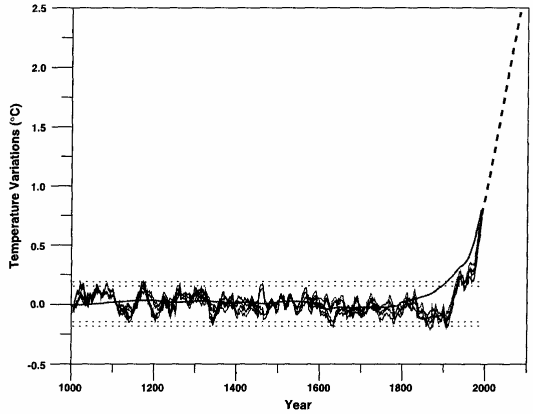

Fig. 6. Comparison of the GHG forcing response (from Fig. 3) with six residuals determined by removing all forcing except GHG from the two different temperature reconstructions in Fig. 1. As in Fig. 5, the three different estimates of solar variability were used to get one estimate of the uncertainty in the response. This figure illustrates that GHG changes can explain the 20th-century rise in the residuals; ±2 standard deviation lines (horizontal dashed lines) refer to maximum variability of residuals from Fig. 5A (inner dashes) and maximum variability (outer dashes) of the original pre-1850 time series (Fig. 1). The projected 21st-century temperature increase (heavy dashed line at right) uses the IPCC BAU scenario (the "so-called IS92a forcing") for both GHG and aerosols (sulfate and biomass burning, including indirect effects), and the model simulation was run at the same sensitivity (2.0°C for a doubling of CO_2) as other model simulations in this article. The IS92a scenario is from (*59*).

Table 3. Comparison of smoothed standard deviations (in °C) of 850-year Northern Hemisphere preanthropogenic residuals from observations with smoothed 850-year coupled ocean-atmosphere GCM control runs (*46*). All records were detrended except for the original smoothed paleo time series.

Mann *et al.* (sm11)	0.066°–0.075°C
CL	0.054°–0.061°C
ECHAM3/LSG* (sm11)	0.058°C
GFDL† (sm11)	0.072°C
HadCM3‡ (sm11)	0.086°C
Summary statistics for 11-year smoothing	
Paleo residuals	0.064° ± 0.009°C
GCMs	0.072° ± 0.014°C
Summary statistics for 51-year smoothing	
Paleo residuals	0.042° ± 0.010°C
GCMs	0.044° ± 0.010°C
Original smoothed paleo time series (1005–1850)	
Mann *et al.*	0.086°C
CL	0.092°C

*The European Centre/University of Hamburg/Max Planck Institute für Meteorologie model. †The NOAA Geophysical Fluid Dynamics Laboratory model, Princeton, New Jersey. ‡The Hadley Centre model at the UK Meteorological Office, Bracknell, UK.

Mann *et al.* (*11*) suggested that the decrease in summer insolation since the early Holocene (*57*) also could have contributed to the cooling between the Middle Ages and the Little Ice Age. However, calculations (*58*) do not support this suggestion.

There are therefore two independent lines of evidence pointing to the unusual nature of late-20th-century temperatures. First, the warming over the past century is unprecedented in the past 1000 years. Second, the same climate model that can successfully explain much of the variability in Northern Hemisphere temperature over the interval 1000–1850 indicates that only about 25% of the 20th-century temperature increase can be attributed to natural variability. The bulk of the 20th-century warming is consistent with that predicted from GHG increases. These twin lines of evidence provide further support for the idea that the greenhouse effect is already here. This assertion may seem surprising to some because of continuing uncertainties with respect to the dynamical response of the ocean-atmosphere system and radiative forcing feedbacks (both direct and indirect) of, for example, clouds, biomass burning, and mineral dust. Although regional climate change is almost certainly influenced by these complex dynamic and thermodynamic feedbacks, the striking agreement seen in this study between simple model calculations and observations indicates that on the largest scale, temperature responds almost linearly to the estimated changes in radiative forcing. The very good agreement between models and data in the preanthropogenic interval also enhances confidence in the overall ability of climate models to simulate temperature variability on the largest scales.

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43. Although the effects of volcanism are primarily manifested in the summer hemisphere, these changes map into mean annual temperature variations at a reduced level, and the EBM is tuned to simulate mean annual temperatures. The reliability of this approach is supported by good agreement between the EBM and recent GCM studies (P. Stott *et al.*, *Clim. Dyn.*, in press) of a decadally averaged cooling effect from volcanism during the late 20th century. The EBM predicts a 0.2°C cooling; the GCM predicts a 0.3°C cooling. Given the facts that the ensemble GCM run has noise in it and that the GCM has a sensitivity greater than the EBM, the EBM-GCM results are not considered to be significantly different.
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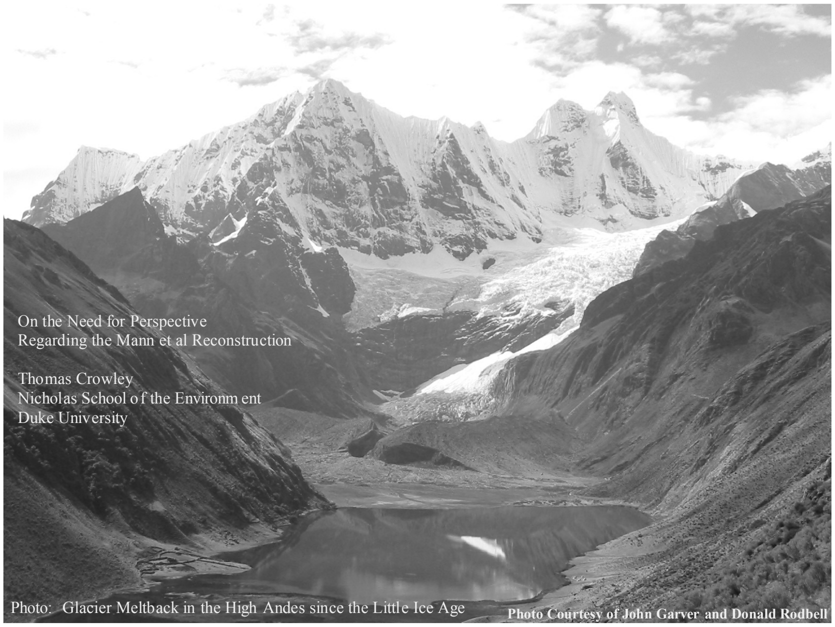
51. Preliminary results suggest that a 2.0°C sensitivity slightly overestimates the response (G. C. Hegerl and T. J. Crowley, in preparation), but it is not yet clear



On the Need for Perspective
Regarding the Mann et al Reconstruction

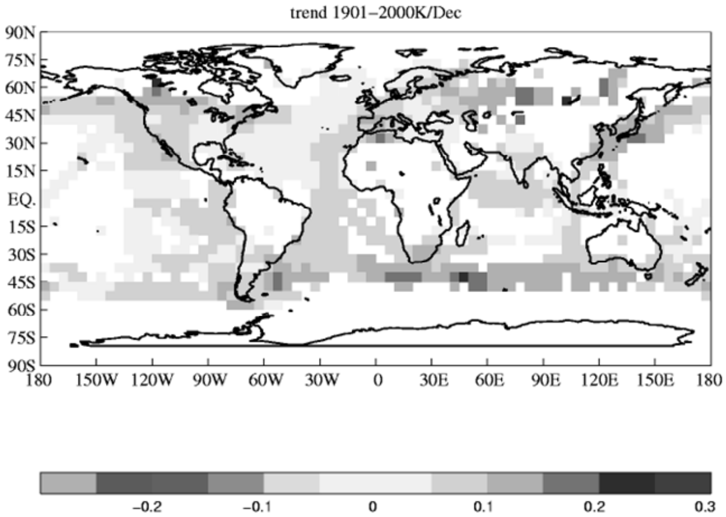
Thomas Crowley
Nicholas School of the Environment
Duke University

Photo: Glacier Meltback in the High Andes since the Little Ice Age

Photo Courtesy of John Garver and Donald Rodbell

(1) Mann et al and IPCC

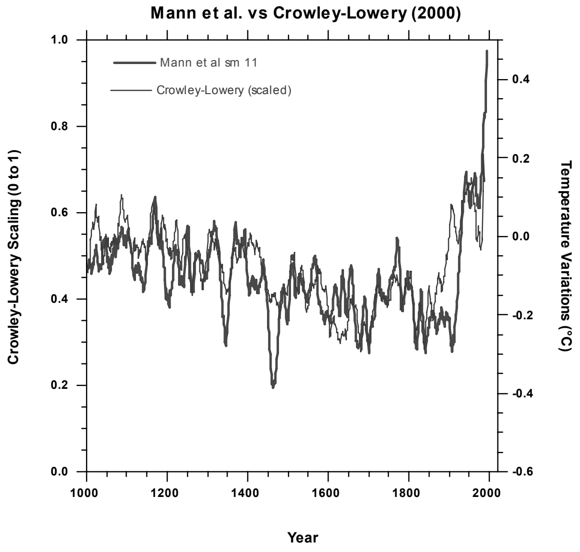
Magnitude of influence over-rated



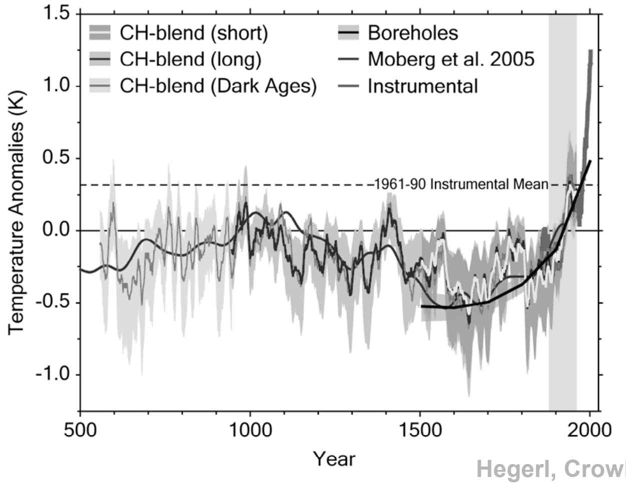
Courtesy G.Hegerl

(2) Mann et al in and of itself

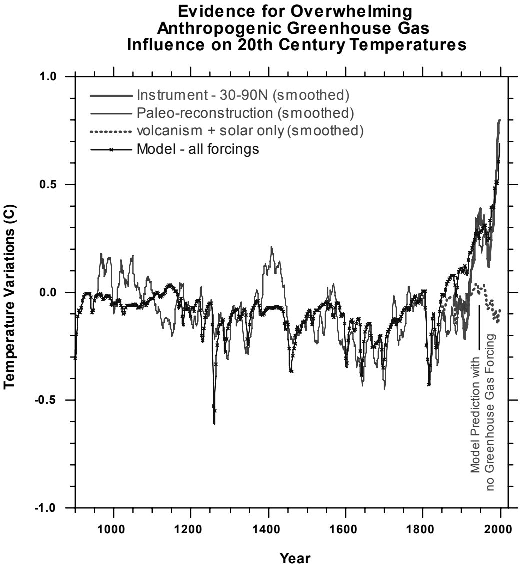
The best estimate we had at the time

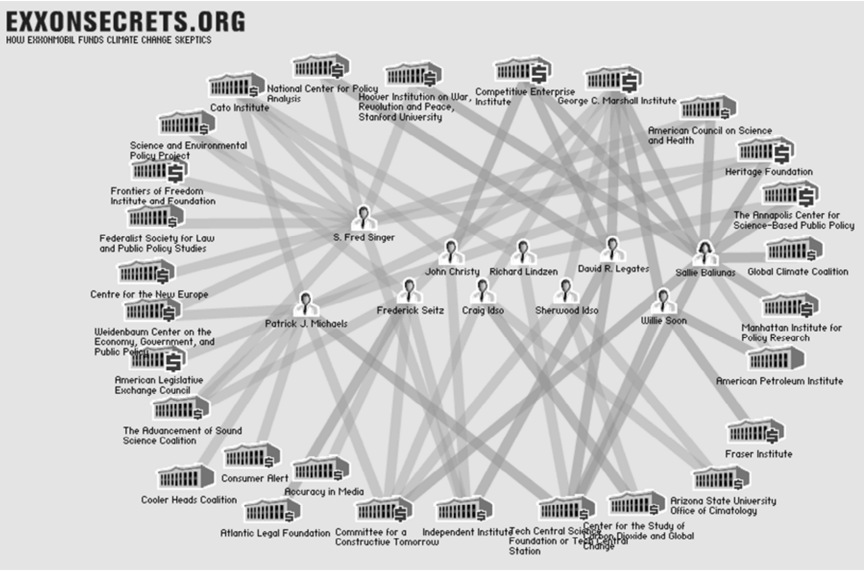


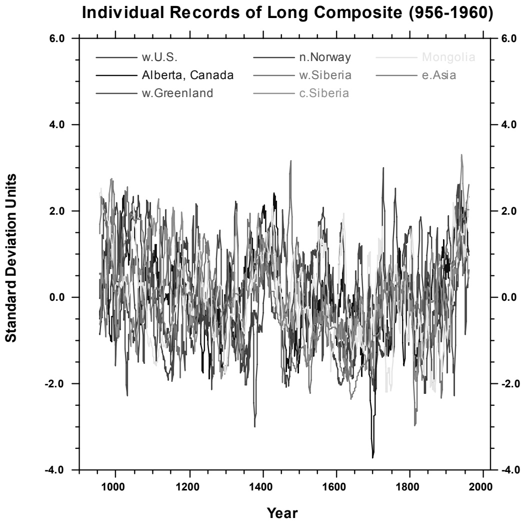
N. Hemisphere land temperatures (30-90N)



Hegerl, Crowley et al. submitted


Evidence for Overwhelming
Anthropogenic Greenhouse Gas
Influence on 20th Century Temperatures
Instrument - 30-90N (smoothed)
Paleo-reconstruction (smoothed)
volcanism + solar only (smoothed)
Model - all forcings
Temperature Variations (C)
1.0
0.5
0.0
-0.5
-1.0
1000
1200
1400
1600
1800
2000
Year
Model Prediction with
no Greenhouse Gas Forcing


EXXONSECRETS.ORG
HOW EXXONMOBIL FUNDS CLIMATE CHANGE SKEPTICS
Cato Institute
National Center for Policy Analysis
Hoover Institution on War, Revolution and Peace, Stanford University
Competitive Enterprise Institute
George C. Marshall Institute
American Council on Science and Health
Heritage Foundation
The Annapolis Center for Science-Based Public Policy
Global Climate Coalition
Manhattan Institute for Policy Research
American Petroleum Institute
Fraser Institute
Arizona State University Office of Cimatology
Center for the Study of Carbon Dioxide and Global Change
Tech Central Science Foundation or Tech Central Station
Independent Institute
Committee for a Constructive Tomorrow
Atlantic Legal Foundation
Accuracy in Media
Consumer Alert
Cooler Heads Coalition
The Advancement of Sound Science Coalition
American Legislative Exchange Council
Weidenbaum Center on the Economy, Government, and Public Policy
Centre for the New Europe
Federalist Society for Law and Public Policy Studies
Frontiers of Freedom Institute and Foundation
Science and Environmental Policy Project
S. Fred Singer
John Christy
Richard Lindzen
David R. Legates
Sallie Baliunas
Frederick Seitz
Craig Idso
Sherwood Idso
Willie Soon
Patrick J. Michaels



MR. WHITFIELD. Thank you. Dr. von Storch, you are recognized for 5 minutes.

DR. VON STORCH. Thank you very much for inviting me here. I just wanted to mention that I am joined here by my colleague, Eduardo Zorito, from the same laboratory sitting there in the back.

Next transparency, please. So I am just summarizing my paper here. So first scientific aspects. So the progression-type methods of the so-called hockey stick studies of Mann, Bradley, and Hughes suffer from a number of problems which should have been addressed before the hockey stick was elevated to an authoritative description of the temperature history of the past 1,000 years. It says 1,000 to 2,000 years but that is an error.

Second, the claim by the IPCC third assessment report, that is the 2001, that there is reliable evidence that climate is beginning to change due to human action was based on a number of different lines of argument which are insensitive to the validity of the MBH studies, that is, the present debate about the validity of the hockey stick is of marginal relevance for the detection of present anthropogenic climate change. I claim the major problems are not of a statistical nature but are related to the social practice of climate change studies.

Next transparency, please. In the Wegman report, let me say a few words about the Wegman report. We have in our working group examined how serious the error of biased centering would be on the overall results given a temperature history reminiscent of the IPCC 1990 version. The paper has been published and the effect is very minor. It does not mean that it is not a glitch but it really doesn't matter here, at least to the extent we could test it.

There are other aspects which are much more relevant I would have hoped that Dr. Wegman would have taken this up, that is, the usage of the trend as a key element for training the progression model. It is a bit funny to use the trend to train something and I will show you in a second what that means.

And second, the method of something, what is called scaling, that is, that you artificially make sure that the variance of the predictor, that is, the temperature, equals the variance of the predicted temperature, the derived temperature. So you multiply it by a number so that it just comes out as if you could explain the total variance by the proxy. You cannot. You know that you cannot do it and therefore you introduce an error which you cannot avoid.

Third, we welcome the suggestion by Wegman and his colleagues to invest much more effort to examine the error structure in deriving temperature data from proxies.

There are two main issues. First is the homogeneity of proxies. If in the year 1960 the tree ring means something for temperature--no, I mean--yes, it does not mean that this is the same information in the year 1200. It could be that the process to get out the information from a 1200 tree ring is different from the 1960. Second, the instationarities of the late proxy and temperature. We know that there are some problems at least that has been explained at the Academy hearing that nowadays the link between temperature and CO_2 seems to be damaged. When Hughes was asked what the reason could be, he gave three different hypotheses, and when he was asked, do you think it could have happened in the past, the answer was yes. So it could be that the link which we see now these days in the past 100 years or so would be different than previous times. We cannot know that and we have to think about how to model this effect.

The next transparency, please. That shows the danger of relying on trends. So you see here, a times series throughout the instrumental period, that is, the period when we think we have enough data to derive Northern Hemisphere mean annual temperature from instrumental data, and you see in yellow, that is the area when the method has been trained and then we see it has been trained from 1910--well, it has been trained for a longer time showing a 21-year running means. And the red curve is what the MBH method was indicating the temperature variation should have been in this period and the black is a new analysis of the climate research unit. It was produced after that was done, and now what you see here is that the green and the black curve are very nicely coincident during the trend but nothing else. Nothing else is reproduced and so it is just what this method is fixing up is the trend and nothing else, or it may be so. One should check that out. And so this is a bit dubious.

Next transparency, please. Understand that you are concerned about the quality control process of climate change science, and I would claim that parts of climate change science, in particular paleoclimatic reconstructions have suffered from gatekeeping and insistence usage of reviewers. I myself can say that they were always the same type of reviews we got, the same style and I am sure that it was the same person and I am sure it was the person we have spoken about here quite a bit.

And I also claim that editors in science magazines have failed to ensure the reproducibility of key results. The methods have not been described properly and their data one could not access. Part of the mess here is due to the practice of *Nature and Science* that they have a bias towards interesting results. I mean, they have--their way of operating is not only that the results are innovative and valid but they must also be interesting. Then what I think is really not good that in the IPCC process experts assess their own work.

That is, to conclude this, climate change science has suffered from limiting action of gatekeepers and the public preference for interesting results. Climate change science should provide stakeholders with a broad range of options and not narrow this range to reduce numbers of options preferred for certain world use.

I was a bit disappointed about the comment from the lady from Illinois who said aren't you afraid if you say this that this would have negative implications for the policy process. I mean, is that really--I mean, I was kind of shocked. I mean, should we really adopt what we say if that is useful for the policy process? Is that what you expect from science? If we give advice, that we first think is it useful for something. I think that is not the way we should operate, or if we do that, you should not listen to us.

Next transparency. This is not to please the people on the right-hand side. The acceptance of the IPCC in the community, this is actually--it is very well accepted and it is very hard to see this but it is the result of a survey which was asking to what extent do you agree or disagree that the IPCC report is of great use of the advancement of science. That is on the left-hand side. And then you see a statistical description of the responses. At the bottom they would say strongly agree. At the top they would say strongly disagree. And then there are--on the left-hand side there are results from 1996 and on the right-hand side 2003, one block for U.S., the other for E.U., and you see in 1996 there was a median of three. That means people, most said well, it is useful. In 2003 the median was two, so they are much more convinced that this is done well. And the same result is with a question to what extent do you agree or disagree that the IPCC reports accurately reflect the consensus of thought within the scientific community. So there is broader agreement that the community is doing right even though I don't think that the Oreskes study was done well and there have been numerous responses on that which have not been accepted by science for whatever reasons.

The last transparency, please. We have to keep in mind that climate change science takes place in a cultural context. It has something to do with what we think we have been trained at and a possibly remarkable result is that the concept of anthropogenic climate change is not new in Western culture. This is not a new invention in the history of this. We have documented very many cases and the first scientific publication we have on that is from 1781 by a physician named Williamson from Philadelphia who was speaking about the changing climate due to human action. At that time the weather in this part of the world was greatly improved because of taking away forests.

Second, climate change science is something what we call post-mormal, that means it goes along with high uncertainties and high relevance. In that case, it is quite normal that the boundaries between value-driven agendas and curiosity-driven science get blurred and we should admit that there is a considerable influence of extrascientific agendas on the scientific process of climate change studies. I think we have seen that today also. The processes of climate change studies need to be analyzed and accompanied by social and policy scientists. So this process we are seeing here, how we argue, we should be something like--yes, always an analysis by social scientists and I think what Dr. Wegman and his colleagues started to do was quite useful in this respect, that we understand to what extent we are driven by non-scientific motives, and this ends my presentation here.

[The prepared statement of Dr. Hans von Storch follows:]

PREPARED STATEMENT OF DR. HANS VON STORCH, DIRECTOR OF INSTITUTE FOR COAST RESEARCH, GKSS-RESEARCH CENTER, GERMANY

Introduction of person

I, Hans von Storch, have been actively involved in climate science since the early 1980s. I have held positions with the Meteorological Institute of the University of Hamburg and at the Max-Planck Institute for Meteorology in Hamburg. At the present time, I am a director of the Institute for Coastal Research of the GKSS Research Center in Germany. I have co-authored more than 120 peer-reviewed articles on various issues of climate dynamics, climate statistics, climate change and climate impact as well as the textbook "Statistical Analysis in Climate Research" (together with Francis Zwiers) published by Cambridge University Press. I was a lead author of Chapter 10 of the Third Assessment Report of the IPCC, but I am not involved in the Fourth Assessment Report of the IPCC.

Based on the scientific evidence, I am convinced that we are facing anthropogenic climate change brought about by the emission of greenhouse gases into the atmosphere.

For further personal details please refer to my web-page: http://w3g.gkss.de/staff/storch.

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Outline

I briefly address three aspects of the hockey-stick issue, namely

1. Scientific aspects:
   - How valid are the regression-type methodologies for reconstruction historical climates?
   - How relevant are these reconstructions for claims that we presently experience a climate change outside the range of what we consider as "normal" (no human interference).
2. The process of achieving success of a scientific knowledge claims in the climate science community:
   - Independence of the review process or presence of gatekeepers.
   - Reproducibility
   - Selection process by Nature & Science.
   - Acceptance by IPCC assessment process.
3. The social conditioning of climate science:
   - The history of perceived anthropogenic climate changes.
   - Post-normal science.

On the basis of my analysis I draw a couple of conclusions, chief being that the process of climate science must be organized in a *sustainable* manner. This means that climate science should be conducted with a low sense of subjective passion; that climate science provides "if-then" answers to questions society poses; that it presents to the society a broad range of possible policy responses and does not restrict the range of policy options to a small corridor that appeals to certain value-driven agendas.

The conditioning of science by the culture of its actors and society is unavoidable. However, the scientists can attempt to make such influences explicit by acknowledging and explicitly reflecting on such influences, especially by engaging social scientists in the process of critical self-reflection. The Wegman-report claims that a major problem in studies such as MBH would be an insufficient engagement by mainstream statisticians. I think a major problem with this study and its transformation into a policy-relevant issue is an insufficient comprehension of the social dynamics of the post-normal process of (not only) climate science.

There are three appendices to this document:

1. My responses to the "Boehlert"-questions given at the NRC hearing on March 2, 2006 in Washington.
2. A contribution to the debate about the "Barton-letters" on the "Prometheus"-weblog http://sciencepolicy.colorado.edu/prometheus/ dated July 8, 2005 (http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/000486 hans_von_storch_on_b.html)
3. An English translation of an article published in the German weekly "DER SPIEGEL" (4/2005): von Storch and Stehr: A climate of staged angst. (http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/000343 a_climate_of_staged_.html)

Scientific aspects

How valid are the regression-type methodologies for reconstruction historical climates?

The key statistical assumption of any of such methods is the uniformity of informational content in the proxies which are regressed on the climate variables (mostly temperature). In other words, are these data influenced by non-climatic variable factors (inhomogeneity), is the transfer function linking proxies and temperature constant in time (stationarity)? Likely, most if not all proxy data (tree rings, coral rings, vine harvests) suffer from some inhomogeneities and instationarities. This is unavoidable and has to be dealt with by using additional insight into the system, e.g. by data assimilation approaches combining limited theoretical (models) and empirical knowledge (uncertain data).

Regression-type models are designed so that they return only part of the full variability of the variable of interest, namely that part which can be traced back to the proxies. Not all of the variability can be accounted for in this way. The difference in variability of temperature and of proxy-derived temperature is dealt with by “scaling”, i.e., by applying a suitable normalization. If “scaling” is used, then the basic principle of regression is violated, as the part of variability in the predictand (temperature), which can not statistically traced back to the predictor (proxy), is nevertheless related to predictor-variability. Scaling is useful, when the transfer function is not regression (screening of co-variability of two variables) but based on physical arguments.

Nevertheless, attempts like those by MBH are useful and should be explored. They may provide useful estimates. The problem with MBH was that the result was presented by the IPCC and others in a manner so that one could believe a realistic description of historical temperature variations had successfully been achieved. The NRC report published in June 2006 has made clear that such a belief was incorrect.

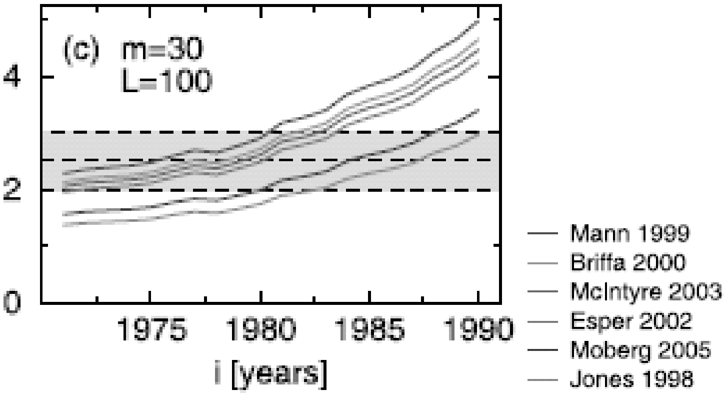
How relevant are these reconstructions for claims that we presently experience a climate change outside the range of what we consider as “normal”

Whether the present climate is influenced by non-natural factors is answered through “detection” studies. Such studies are based on the insight that the predicted signal of human-caused climate change should emerge in most recent times from the natural variability. Second, one would expect it to manifest itself with a higher “than normal” *rate of change*. Thus, the signal is expected to be a rapid warming in the most recent past. The method to test this hypothesis is to find out if we have a “steeper-than-normal” recent upward temperature trend. The hypothesis is not “we have a period which is warmer than ever in historical times”. In that sense the claim whether the last decade is the warmest of the past millennium is not relevant to detection; the question is whether the recent rate of warming is markedly stronger than what has happened in the past.

The hypothesis is tested by framing the problem as a statistical test of a null hypothesis. The null hypothesis reads “the present trend is of natural origin”. Then, one determines the range of trends consistent with natural variability – and rejects the null hypothesis (and accepts the hypothesis that the trends is not of natural origins) if the present trend is larger than, say, 97.5% of trends originating entirely from natural variability.

The crux of this approach is of course the determination of the range of trends which are observable under natural conditions. To do so, one may rely only on the instrumental period, which is contaminated by the expected signal and rather short, on multi-century reconstructions as MBH and on extended model simulations of undisturbed conditions. Obviously the determination of the range of “normal” trends is uncertain and absolute certainty can not be attained within a reasonable time.

We[1] have examined which range the different historical reconstructions suggest. To do so, the time series of reconstructions have been "modelled" as a long-memory process, and standard deviations of trends are derived. Here, the trend is defined as the difference of two 30 years means 100 years apart. Then these trends are determined from the instrumental record as given as multiples of the standard deviations derived from the different reconstructions.



The result is given in the diagram; the curves are all the same, but they differ in scale because of the unit of different standard deviations derived from the reconstructions given at the figure caption. The horizontal dashed lines mark 2, 2.5 and 3 standard deviations. Two standard deviations correspond to a risk of false rejection of the null hypothesis of 2.5%.

Obviously, in all cases, the critical 2-standard deviation mark is passed sometimes in the past decades; in case of MBH this happens very early, while in Moberg's more variable reconstruction at about 1980.

I conclude that the claim of "detection of anthropogenic climate change" is valid independently of which historical temperature reconstruction one chooses to believe in.

It should also been taken notice that the claims of successful detection on non-natural warming trends and its attribution to chiefly elevated greenhouse gas concentrations in the atmosphere in the Third Assessment report were not based on the historical reconstructions but on the analysis of the instrumental temperature record as well as on numerical experiments with climate models.

The process of achieving success of a scientific knowledge claim in the climate science community

A normal condition in the progress of science is that knowledge claims are accepted only after a *"peer-review"* process. The peer-review process attempts to assure that knowledge claims are consistent with the empirical evidence, and properly related to contemporary accepted knowledge claims, and that the methods are sound and are reproducibly described. The "peer-review" process does not eliminate the possibility that new ideas are rejected since they may contradict contemporary, powerful but possibly false knowledge claims (see Ludwik Fleck's seminal book on "Generation of a Scientific Fact"). In order to minimize such a danger, the verdict of peer-reviewers should, to first order approximation, be independent of the persons involved in the review process. Nonetheless, the danger is that a few scholars may become powerful *gatekeepers*, for example as reviewers who are regularly called upon or as editors of scientific journals. The primary goal of such gatekeepers is to fend off publications which may contradict their own thinking, and not to ensure that only internally consistent and plausible publications reach the market of knowledge claims (i.e. scientific journals). Unfortunately this seems to have happened in the field of historical global climate reconstructions, where a small group of scientists has exerted an undue control of the entire field.

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[1] Rybski, D., A. Bunde, S. Havlin,and H. von Storch, 2006: Long-term persistence in climate and the detection problem. Geophys. Res. Lett. 33, L06718, doi:10.1029/2005GL025591

Usually, a further mechanism more closely tied to the substance of research is used to quality-control scientific knowledge claims, namely *reproducibility*. This mechanism has ceased to operate in some quarters of paleo-climate science, since some scientists consider "their" data as their personal property and not that of the scientific community, so that others are unable to challenge conclusions drawn from these data by analysing the raw data in their own manner. Although such secrecy is a very human trait it violates the norms of science. Even hostile competitors should have an opportunity to independently re-examine the empirical evidence for conclusions drawn by others, in particular when they become relevant for the policy domain. Data must be become public; the methods employed must be described in algorithmic detail.

Another relevant aspect is the functioning of the two prestigious journals "*Science*" and "*Nature*". The journals enjoy high esteem within and outside of the scientific community as having the highest scientific standards, which is not always the case. The contents of Nature and Science also receive exceptional attention in the media world-wide. However, different from "normal" scientific journals, the editorial decision to accept a scientist's contribution to Science or Nature is also based on the newsworthiness of the research contribution. The presented results must not only be valid and innovative but must also be of interest for a wider community of readers. Such a criterion is reasonable from a economic point-of-view, but it clearly introduces a filter in what is reaching the public is not solely based on the scientific merit of research. Research results with stronger media appeal fare better in this competition of scientific findings; results biased towards higher sensitivity to human interference are more interesting to a broad audience than findings that report low sensitivities. In addition, there may also be a bias towards certain authors, who are well known, because they enjoy public visibility, or command appealing writing skills, "sell" well. Sometimes such contributions are invited.

Another problem with the same journals is that their articles must be relatively short so that technical aspects cannot be described in any detail; indeed, the MBH publication was cursory on the methodical side – thus the statistical method, the validation and the reproducibility, have not been seriously subject to the review process. Ironically, after publication in "Nature" the method was considered "peer-reviewed" and thus valid. However, this was not the case, as the method had not been properly described.

The *IPCC* has different levels of operation – the generation of the technical chapters, which is done by a group of "lead authors", headed by "convening lead authors", and the process of arriving at a SPM (Summary for policymakers) and other overall assessment documents, which is done by the convening lead authors and representatives of the countries.

How the selection process of lead and convening lead authors is done, I do not know – but it is clear that the "lead authors" are supposed to be *experts* in the field. This leads to the situation that the IPCC chapters are dominated by the authors of the most influential articles in their respective fields of research. Participation as a lead or convening lead author has the advantage that one can make sure that one's own work is positively covered in the IPCC report. However, most lead and convening lead author excel as honest brokers, but some level of gatekeeping may prevail. Indeed, the reputation of the IPCC among scientist has increased to very high levels in the past years.

The IPCC procedure differs markedly from the procedure adopted by the National Research Council assessment. In that case, a group of eminent scientists was chosen, who have contributed to the issue only little or not at all, but have a god understanding of the field as a whole. These scientists then invited a group of experts to present the different angles and knowledge claims. I consider the NRC procedure better in assessing the field of knowledge than the IPCC approach. It may be, however, that the NRC approach can not be used for such a complex and large field, which the IPCC is covering.

In case of the MBH temperature reconstruction one should note that in the technical chapter of the TAR different reconstructions had been presented; it was the SPM and the synthesis report, where the range of reconstructions was reduced to just one, the MBH. It would be interesting to learn how this could have happened.

The social conditioning of climate science

Science is a social process, which, as all social processes, is conditioned by the culture of the actors. This does not mean that scientists would do their analysis irrationally or in a biased manner, but it means that our questioning may by guided by culturally constructed concerns and interests. Also, we may be convinced of the validity of some findings more easily if these findings are consistent with our prior lay-knowledge.

The history of perceived anthropogenic climate changes

It has often been claimed that anthropogenic climate change is a recent concept. This is incorrect. In the history of ideas of the past 1000 years, we[2] have found a number of occasions when (western) people have used the concept to explain observed changes:

> "During the last 20 years the concept of anthropogenic climate change has left academic circles and become a major public concern. Some people consider 'global warming' as the major environmental threat to the planet. Even though mostly considered a novel threat, a look into history tells us that claims of humans deliberately or unintentionally changing climate is a frequent phenomenon in Western culture. Climate change, due to natural and anthropogenic causes, has often been discussed since classical times. Environmental change including climate change was seen by some as a biblical mandate, to 'complete the Creation'. In line with this view, the prospect of climate change was considered as a promising challenge in more modern times. Only since the middle of the 20th century, has anthropogenic climate change become a menacing prospect. The concept of anthropogenic climate change seems to be deeply embedded in popular thinking, at least in Europe, which resurfaces every now and then after scientific discoveries. Also, extreme weather phenomena have in the past often been explained by adverse human interference."[3]

This finding is insofar relevant as it points out that we, as members of the western culture, are somehow prepared to accept "anthropogenic influence" as an explanation for otherwise unexplainable events, such as a cluster of extreme events. Our common understanding is that such a human influence would be associated with negative impacts. This pre-conditioning may influence our process of drawing conclusions, in particular when we (scientists) deal with the problem of transferring scientific findings into the political arena.

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[2] von Storch, H., and N. Stehr, 2000: Climate change in perspective. Our concerns about global warming have an age-old resonance. nature 405, 615

[3] von Storch, H. and N. Stehr, 2006: Anthropogenic climate change - a reason for concern since the 18th century and earlier. Geogr. Ann., 88 A (2): 107–113.

Post-normal science.

Most of environmental science is what sociologists call “post-normal”, i.e., loaded with high uncertainty on an issue of great practical importance. Climate change science is an example of such post-normal science.[4]

A characteristic of post-normal science is that the boundaries between science and value-driven agendas get blurred; that representatives of NGOs are considered to know better about the functioning and dynamics of systems than scientists; that parliamentarian committees delve into the technicalities of science; that amateurs engage in the technical debate: and that some scientist try to force “solutions” upon policymakers and the public. In such a situation it becomes entirely possible that individual scientists emphasize those insights which are assumed to influence certain policy decisions more forcefully, while downplaying others.

Typical for such a post-normal situation is the flooding of the media with books and movies which dramatize the issue. Recent examples include: The Day After Tomorrow, State of Fear, Satanic Gases, The Revenge of Gaia, and An Inconvenient Truth.

In this situation we need a discussion, not only among scientists about the role of science for the public, which must be the provisions of options for policy, not the narrowing of the range of options to satisfy different worldviews. To limit the influence of non- or pre-scientific knowledge claims, social and policy scientists need to analyse the different processes in climate science, and the interdependence of culture, policy, politics, media and climate science. Even if science can never be fully “objective”, it may nevertheless be possible to make climate science a considerably more objective practice than what we have in these days.

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[4] Bray, D. and H. von Storch, 1999: Climate Science. An empirical example of postnormal science. Bull. Amer. Met. Soc. 80: 439-456

Appendices

(a) My answers to Chairman Boehlerts questions, given at the NRC hearing

What is the current scientific consensus on the temperature record of the last 1,000 or 2,000 years? What are the main areas of uncertainty and how significant are they?

- There is consensus on the "blade", but the claimed smoothness of the shaft is likely false.
- The main problem is the loss of information encoded in the proxy data and the shortness of the instrumental record for training the statistical models.

What is the current scientific consensus on the conclusions reached by Drs. Mann, Bradley and Hughes? What are principal scientific criticism of their work and how significant are they?

Has the information needed to replicate their work been available? Have other scientists been able to replicate their work?

- There is no consensus on the claims (which?) made by MBH. The main critique is that the method is suffering from a too large loss of variability on long time scales.
- No, the information required for replication was not made available in a suitable manner. The original publication in "nature" did not provide this information and was obviously published without careful review of the methodology.
- Yes, the details of the method were finally determined, among others by Bürger et al., who checked a wide range of combinations of details – which all gave widely different results.

How central is the debate over the paleoclimate temperature record to the overall consensus on global climate change? How central is the work of Drs. Mann, Bradley and Hughes to the consensus on the temperature record?

- The main conclusions about "detection and attribution" are drawn from the instrumental record and models; the different reconstructions do not contradict "detection".
- The MBH work is widely accepted as truth outside of people directly engaged in the issue, because of a less than satisfactory marketing by the IPCC.

(b) My posting on weblog "Prometheus" July 08, 2005 on the "Barton letters"

My reaction to Rep. Barton's requests is split. In his five letters, he is asking for information from two different groups, namely institutions with reviewing responsibilities (IPCC, NSF) and individuals with scientific responsibilities (M, B and H). I find his inquiry of the performance of the institutions IPCC and NSF valid, but the interrogative questioning of the individual scientists is inadequate.

a) Scientists. The scientists have the task to be innovative, creative, to try new avenues of analysis and the like. They have the right to err, the right to suggest explanations and interpretations which may need to be revised at a later time. They should document what they have done, so that others can replicate.

However, this documentation often can not take the form of keeping runnable old codes of the applied algorithms, simply because the software is no longer consistent with quickly replaced hardware. For instance, most of the state-of-the-art coupled AOGCMs used in the mid 1990s are simply no longer available and running at, for instance, the German Climate Computer Center. After replacing a high performance computer with a new system, the standard model codes, including community models, need to be adapted to the requirements and possibilities of the new system, and the old code will often no longer run. This has nothing to do with the norms of the community but simply with technological progress. Also specific commercial libraries of specialized algorithms may no longer be accessible. Data and codes written on old magnetic tapes or even floppies are usually no longer readable.

Therefore the documentation must take the form of a mathematical description of the algorithms used. This is in many if not most cases sufficient for replication. Also, the intention of replicability is not to exactly redo somebody's simulation and analysis, but to find the same result with a similar code and different but statistical equivalent samples. The problem is usually not that the codes contain errors (even if many of the more complex ones likely contain minor, mostly insignificant errors), but that specific elements of implementation and specific aspects of the considered sample of evidence will lead to conclusions, which do not hold if another sample is considered or a different but equally good algorithm is employed. The reason is that we want to learn about the dynamics of the real world, and these insights should not depend on random choices in sampling and implementation. We generally do not expect scientists to manufacture results, or that unintended but significant errors will affect the eventually published conclusions.

Having this situation in mind, I consider Rep. Barton's requests to the three scientists as inadequate and out-of-scale. However, the language used by Rep. Barton makes me perceiving this request as aggressive and on the verge of threatening.

The situation is different with the second groups of recipients, the:

b) "Reviewers". Reviewers have a different role, namely they shall make sure that the standards of scientific reporting are held up. They have to ensure that the proposed explanations are considered by independent experts as to whether the presented analysis seems valid and in principle reproducible. "Independent" means that the reviewers have no vested interests for or against the case presented. In the conventional set-up these interests usually refer to academic schools of thought, but in the unfortunate, post-normal case of climate science independence from the political utility of the case should be established.

In this case, I find the inquiry of Rep. Barton to be valid. The IPCC has failed to ensure that the assessment reports, which shall review the existing published knowledge and knowledge claims, should have been prepared by scientists not significantly involved in the research themselves. Instead, the IPCC has chosen to invite scientists, who dominate the debate about the considered issues, to participate in the assessment. This was already in the Second Assessment Report a contested problem, and the IPCC would have done better in inviting other, considerably more independent scientists for this task.

Instead, the IPCC has asked scientists like Professor Mann to review his own work. This does not represent an "independent" review.

The NSF seems to have failed to ensure that sufficient information is provided about work done under its auspices.

Rep. Barton should also have asked the editors of "Nature", why the original manuscript was accepted for publication even though the key aspect of replicability was obviously not met by the MBH manuscript. Actually, MBH could not meet this condition because of the strict length limitation of that journal (nowadays one would ask for extensive Supplementary Online Material). One should ask why the manuscript was accepted nevertheless - and not, as in many other cases, the manuscript was recommended to be published in a "normal" journal without the severe length limitations. I believe the reasons for Nature were the journalistic reasons - namely the expected broad interest in the subject. One should also ask why after the critique von McIntyre and McKitrik only MBH got the opportunity for a correction of his paper, whereas the short manuscript of their opponents was rejected.

To conclude - the requests to M, B and H are not fair but may unfortunately lead to a repressive atmosphere within climate science; the requests to NSF and the IPCC, however, are appropriate, as these institutions may have failed in a primary task, namely to guarantee an open scientific discourse. And, Rep. Barton should have included the editors of Nature in his analysis.

A Climate of Staged Angst

By Hans von Storch and Nico Stehr

The days are gone when climate researchers sat in their ivory towers packed to the rafters with supercomputers. Nowadays their field has become the stuff of thrillers, and they themselves have risen to take on the leading roles. The topic is so hotly contested, the prognoses so spectacular, that they are no longer merely the subject of media reports; now the specialists in staged apocalypse have moved in. Last year Roland Emmerich depicted a climatic collapse provoked by humankind in his film "The Day After Tomorrow." Since last week the belletristic counterpart has been available in German bookstores: the novel "State of Fear," by the best-selling author Michael Crichton.

The thriller is about the violent conflict between sober environmental realists and radical environmental idealists. For the idealists, the organized fear of abrupt climate change serves as a handy weapon. They interpret every somehow unusual weather event as proof of anthropogenic global warming. "You have to structure your information so that it's always confirmed, no matter what kind of weather we have," the P.R. consultant for the environmentalist organization advises. The realists, who protest that the evidence that human activity has increased meteorological extremes is thin, are fighting a losing battle. Their dry scientific arguments are unable to gain any ground against the colorful, horrific visions of the climate idealists.

Film and novel have certain aspects in common. Where Emmerich holds out the prospect of a threatening climate catastrophe, the book prophesies an economic collapse. In both cases, greenhouse gases produced by humankind are the culprit – in the film, because the emissions themselves are too much; in the book, because the fear of them is. The idealists are so obsessed with their mission that ultimately, in order to rouse the public, they themselves bring about the foretold catastrophes.

Despite a good deal of factually untrue – and thus all the more striking – compression, Crichton has quite correctly observed the dynamic of the paths of communication among scientists, environmentalist organizations, the state and the civilian population. For there is indeed a serious problem for the natural sciences: namely, the public depiction and perception of climate change. Research has landed in a crisis because its public actors assert themselves on the saturated market of discussion by overselling the topic.

Climate change of man-made origin is an important subject. But is it truly the "most important problem on the planet," as an American senator claims? Are world peace, or the conquest of poverty, not similarly daunting challenges? And what about population growth, demographic change or quite normal natural disasters?

In the U.S., only a very few remain interested in the greenhouse effect. At the end of the 1980s, the situation was still different. That was the era of the great drought of 1988, the Mississippi flood of 1993, and the climate capers ought by rights to have taken off in earnest from that point. But that never happened in the U.S., and interest petered out. According to a survey by the CBS television network in May 2003, environmental problems were no longer ranked among the six most important subjects; and even within environmental problems, the topic of climate came in only in seventh place. In Germany, so far, things are still seen differently. But for how much longer?

In order to keep the topic of "climate catastrophe" – a concept nonexistent outside the German-speaking world, by the way – continually in the public eye, the media feel obligated, exactly like the protagonists in Crichton's thriller, to keep framing the topic "a bit more attractively." At the beginning of the 1990s – severe storms had just swept through the country – one could read and hear in the German media that storms were due to become ever more severe. Since then, storms have become rarer in northern Europe. But no notice is taken of this. The fact that barometric fluctuations in Stockholm have shown no systematic change in the frequency and severity of storms since Napoleon's time is passed over in silence. Instead, there is now talk of heat waves and floods. Very much in the style of Crichton's instigators of fear, the story is now that all manner of extreme events are on the increase. Thus even drought in Brandenburg and deluge on the Oder fit the picture without apparent contradiction.

Add to this – besides normal floods and storms – other, more dramatically threatening, scenarios: the reversal of the Gulf Stream and the resultant cooling of large areas of Europe, for instance, or even the rapid melting of the Greenland ice pack. The question has already been publicly raised whether perhaps even the Asian tsunami can be attributed to the disastrous effects of human activity.

This will not be able to hold the public's attention for long. Soon people will have become accustomed to these warnings, and will return to the topics of the day: unemployment and Hartz IV, Turkey's entry to the E.U. or whether Borussia Dortmund can avert disaster on the soccer field and in the boardroom. Thus we will see firsthand how the prophets of doom will draw the climatic dangers in even more garish colours. The terrifying visions to haunt the future can already be guessed at: the breakup of the west Antarctic shelf ice, which will cause the water level to rise much more rapidly, and after a few decades of uncontrolled carbon dioxide emissions, an abrupt rise in temperatures, giving us a deadly atmosphere like that of Venus. Prospects such as these have long been in the public eye; can they not compete effortlessly with Emmerich's Hollywood images?

The costs of stirring up fear are high. It sacrifices the otherwise so highly valued principle of sustainability. A scarce resource – public attention and trust in the reliability of science – is used up without being renewed by the practice of positive examples.

But what do climate researchers themselves think, how do they deal with the media and the population?

Public statements by noted German climate researchers give the impression that the scientific bases of the climate problem have essentially been solved. Thus science has provided the prerequisites for us now to react appropriately to the goal; meaning, in this case, to reduce greenhouse gas emissions as much as possible.

This does not at all reflect the situation in the scientific community. A considerable number of climatologists are still by no means convinced that the fundamental questions have been adequately dealt with. Thus, in the last year a survey among climate researchers throughout the world found that a quarter of the respondents still harbor doubts about the human origin of the most recent climatic changes.

The majority of researchers are indeed of the opinion that global climate change caused by human activity is occurring, that it will accelerate in the future, and that it will thus become more readily apparent. This change will be accompanied by warmer temperatures and a higher water level. In the more distant future, that is, in about 100 years, a considerable increase of atmospheric greenhouse gases is foreseen, together with an increase in heavy precipitation in our latitudes; in some regions there could be more powerful storms, in others weaker ones.

But again and again, there are scientists to whom, true to the alarmists' maxim in Crichton's book, this does not sound dramatic enough. Thus, more and more often they connect current extreme weather events with anthropogenic climate change. To be sure, this is usually carefully formulated; interviews sound something like this: "Is the flooding of the Elbe, the hurricane in Florida, this year's mild winter evidence for the climate catastrophe?" Answer: "That's scientifically unproven. But many people see it that way." Neither of these statements is false. In combination, however, they suggest the conclusion: Of course these weather events are evidence. Only no one dares to say this explicitly either.

The pattern is always the same: the significance of individual events is processed to suit the media and cleverly dramatized; when prognoses for the future are cited, among all the possible scenarios it is regularly the one with the highest rates of increase in greenhouse gas emissions – and thus with the most drastic climatic consequences – that is chosen; equally plausible variations with significantly lower emission increases go unmentioned.

Whom does this serve? It is assumed that fear can motivate listeners, but it is forgotten that it mobilizes them only in the short term. Climatic changes, however, demand long-term reactions. The effect on public opinion in the short view may indeed be "better," and thus may also have a positive effect on reputation and research funding. But in order for this to function in the long run, each most recent claim about the future of the climate and of the planet must be ever more dramatic than the previous one. Once apocalyptic heat waves have been predicted, the climate-based extinction of animal species no longer attracts attention. Time to move on to the reversal of the Gulf Stream. Thus there arises a spiral of exaggeration. Each individual step may appear to be harmless; in total, however, the knowledge about climate, climate fluctuations, climate change and climatic effects that is transferred to the public becomes dramatically distorted.

Sadly, the mechanisms for correction within science itself have failed. Within the sciences, openly expressed doubts about the current evidence for climatic catastrophe are often seen as inconvenient, because they damage the "good cause," particularly since they could be "misused by skeptics." The incremental dramatization comes to be accepted, while any correction of the exaggeration is regarded as dangerous, because it is politically inopportune. Doubts are not made public; rather, people are led to believe in a solid edifice of knowledge that needs only to be completed at the outer edges.

The result of this self-censorship in scientists' minds is a deaf ear for new and surprising ideas that compete with or even contradict conventional patterns of explanation; science degenerates into being a repair shop for popular, politically opportune claims to knowledge. Thus it not only becomes sterile; it also loses its ability to advise the public objectively.

One example of this is the discussion of the so-called "hockey stick," a temperature curve that allegedly depicts the development over the last 1000 years, and whose shape resembles that of a hockey stick. In 2001 the Intergovernmental Panel on Climate Change, the committee of climate researchers appointed by UNO, rashly institutionalized this curve as the iconic symbol for anthropogenic climate change: At the end of a centuries-long period of stable temperatures, the upward-bent blade of the hockey stick represents the human influence.

In October 2004, we were able to demonstrate in the scientific journal "Science" that the methodological bases that led to this hockey-stick curve are mistaken. We wanted to reverse the spiral of exaggeration somewhat, without also relativizing the central message – that climate change caused by human activity does indeed exist. Prominent representatives of climate research, however, did not respond by taking issue with the facts. Instead, they worried that the noble cause of protecting the climate might have been done harm.

Other scientists lapse into a zeal reminiscent of nothing so much as the McCarthy era. For them, methodological criticism is the spawn of "conservative think tanks and propagandists for the oil and coal lobby," which they believe they must expose; dramatizing climate change, on the other hand, is defended as a sensible means of educating society.

What is true for other sciences should also hold for climate research: Dissent is the motor of further development, Differences of opinion are not an unpleasant family affair. The concealment of dissent and uncertainty in favor of a politically good cause takes its toll on credibility, for the public is more intelligent than is usually assumed. In the long term, these allegedly so helpful dramatizations achieve the opposite of that which they wish to achieve.

By doing so, however, both science and society will have wasted an opportunity.

Hans von Storch, 55, heads the Coastal Research Institute of the GKSS Research Centre in Geesthacht, Germany; he is considered leading experts statistical analysis of climatological data and simulations. Together with Nico Stehr, 62, sociologist at the Zeppelin University in Friedrichshafen, Germany he has conducted ongoing research into the public perception of climate change.

Translated by Paul Malone

First published in Der Spiegel No. 4, 2005.

U.S. House of Representatives Committee on Energy and Commerce, July 19, 2006

Hearing "Questions Surrounding the 'Hockey Stick' Temperature Studies: Implications for Climate Change Assessments"

Comments by **Hans von Storch**

Director of Institute for Coastal Research,
GKSS Research Center, Germany,
and
Professor at the Meteorological Institute,
University of Hamburg, Germany

Scientific aspects:

◆ The regression-type methods of the so-called "hockey-stick" studies of Mann, Bradley and Hughes (MBH) suffer from a number of problems, which should have been addressed before the "hockey-stick" was elevated to an authoritative description of the temperature history of the past 1000-2000 years.

•

◆ The claim by the IPCC TAR that there is reliable evidence that climate is beginning to change due to human action was based on a number of different lines of argument, which are insensitive to the validity of the MBH studies.

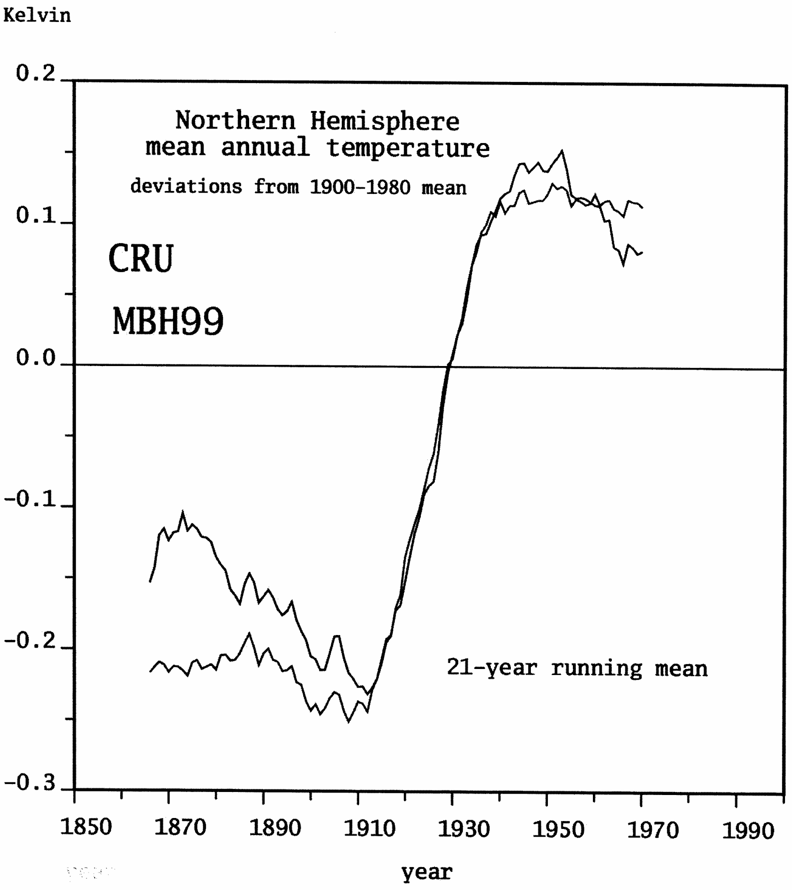
◆ **The present debate about the validity of the hockey-stick is of marginal relevance for the detection of present anthropogenic climate change. The major problems are not of statistical nature but are related to the social practice of climate change science.**

Wegman-report

◆ We have examined how serious the error of biased centering would be on the overall results, given a temperature history reminiscent to the IPCC 1990 version – the effect is very minor.

◆ Other aspects may be more relevant, such as
- the usage of the trend as key element for training the regression model,
- the method of a-posteriori "scaling" so that the variances of the temperature and the derived temperature match during the training period.

◆ We welcome the suggestion to invest much more efforts to examine the error structure in deriving temperature data from proxies – inhomogeneities in the proxies; instationarities in the link "proxy – temperature".


Kelvin
0.2
0.1
0.0
-0.1
-0.2
-0.3
Northern Hemisphere
mean annual temperature
deviations from 1900-1980 mean
CRU
MBH99
21-year running mean
1850
1870
1890
1910
1930
1950
1970
1990
year

Quality-control in the process of climate change science :

- ◆ Parts of climate change science, in particular paleo-climatic reconstructions have suffered from gate keeping and incestuous usage of reviewers.
- ◆ Editors and science managers have failed to ensure reproducibility of key results.
- ◆ Nature and Science have a bias towards “interesting results”.
- ◆ In the IPCC process experts assess their own work.
- ◆ **Climate change science has suffered from the limiting action of gate keepers and a public preference of “interesting results”. Climate change science should provide stakeholders with a broad range of options and not narrow this range to a reduced number of options preferred by certain worldviews.**

Acceptance of IPCC in the community, 1996, 2003

Fig.5.a. To what extent do you agree or disagree that the IPCC reports are of great use to the advancement of science?

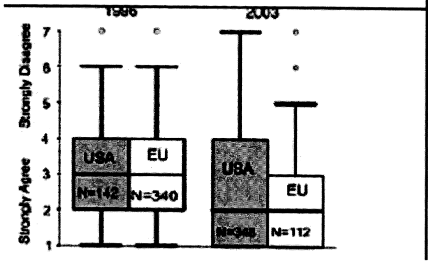
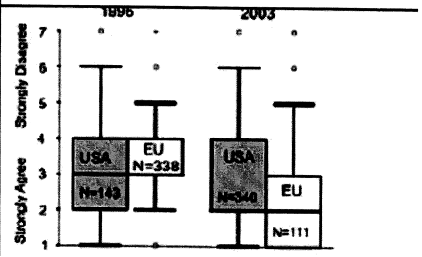


Fig.5.b. To what extent do you agree or disagree that the IPCC reports accurately reflect the consensus of thought within the scientific community?



Bray, pers. comm.

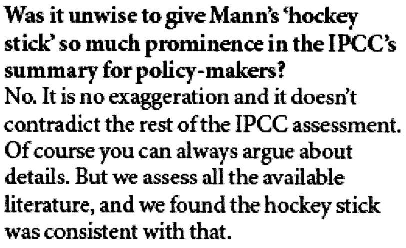
Cimate change science in the cultural context.

◆ The concept of anthropogenic climate change is not new in western culture.

◆ Climate change science is post-normal, i.e., it goes along with high uncertainties and high relevance. The boundaries between value-driven agendas and curiosity-driven science get blurred.

◆ **There is considerable influence of extra-scientific agendas on the scientific process of climate change studies. The process of climate change studies needs to be analysed and accompanied by social and policy scientists.**

Examples of stonewalling ...



Was it unwise to give Mann's 'hockey stick' so much prominence in the IPCC's summary for policy-makers?
No. It is no exaggeration and it doesn't contradict the rest of the IPCC assessment. Of course you can always argue about details. But we assess all the available literature, and we found the hockey stick was consistent with that.

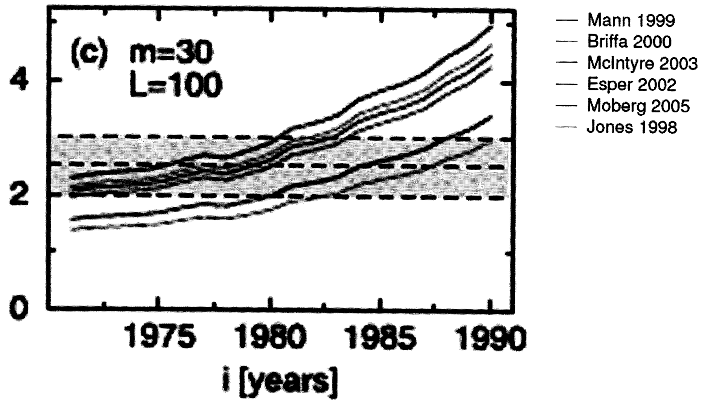
IPCC chairmann Pachauri
in "nature", 2005

"We have 25 or so years invested in the work. Why should I make the data available to you, when your aim is to try and find something wrong with it." (Comment by a prominent member of the paleoclimatic community - 21. February 2005)

Gatekeeping …

This is a deeply flawed manuscript, and it's publication would damage the reputation of this promising new journal. The authors display a disregard for existing peer-reviewed literature that unambiguously refutes their main claims. Each of their primary claims is false or misleading, as detailed below in this review. Moreover, the focus the paper is now plainly inappropriate, focusing on nearly decade-old work, the details of which and the key conclusions of which have now been independently validated by numerous other studies. The manuscript is backwardlooking, invoking flawed criticisms of now very old work, while current studies have moved well beyond this spurious debate about statistical minutia, focusing instead on real scientific issues.

http://www.cosis.net/copernicus/EGU/cpd/2/S139/cpd-2-S139.pdf
Review of a article co-authored by Bürger and Cubasch.



Temporal development of $\Delta T_i(m,L) = T_i(m) - T_{i-L}(m)$ divided by the standard deviation $\sigma(m,L)$ of the considered reconstructed temp record

for m=30 and L=100 years.

The thresholds R = 2, 2.5 and 3 are given as dashed lines.

Rybski et al., GRL, 2006

MR. WHITFIELD. Thank you, Dr. von Storch, and Mr. McIntyre, you are recognized for 5 minutes.

MR. MCINTYRE. Good afternoon, Mr. Chairman and members of the committee. My name is Steve McIntyre. I appreciate the invitation to appear today to discuss my research coauthored with Ross McKitrick of the University of Guelph which in part led to today's meeting.

I have three main messages. First, little reliance can be placed not only the original Mann reconstruction, various efforts to salvage it, or similar multi-proxy studies even ones which did not use Mann's methodology. Second, peer review as practiced by academic journals is not an audit but something much more limited. In turn, scientific overviews such as the ones produced by IPCC or even by the NAS panel are based almost entirely on literature review rather than independent testing. Third, there is already an existing data archive which is excellent, but in order to make it work scientists actually have to archive their data and code. This is not done consistently in the paleoclimate community and it makes replication virtually impossible in many cases. Much of this work is funded by the U.S. Federal government and some very simple administrative measures under existing policies could alleviate many of the problems.

In the two reports, only one topic was specifically audited in the sense of independent testing as opposed to literature review, and that was simply whether Mann's method was biased towards producing hockey stick-shaped series. Both reports verified this hotly contested result. Both panels agreed with varying emphasis that no confidence could be placed on reconstructions prior to 1600 and that Mann's statistical methods were unsatisfactory. The Wegman report considered how such an error could have remained undetected. In addition to their comments, an important reason that the IPCC does not carry out independent tests.

Some comments of Dr. Bloomfield's at the NAS press conference may lead people to believe that a hockey stick could be obtained from a simple average of all MBH proxies. This is simply not the case as you see by the graph on both screens. The NAS panel illustrated several other reconstructions but their consideration was merely a literature review. They did not attempt to replicate or audit these other studies as I have tried to do. Each one has replication problems. One of the criticisms of the Mann study recognized by the NAS panel was its use of bristle cones and closely related foxtails, a flawed proxy which the panel said should be avoided. However, they did not assess this. The impact of not using bristle cones can be substantial. Removal of merely two bristle cone series changes relative medieval modern levels in the Crowley and Lowery reconstruction that was shown to you earlier. The panel noted the so-called divergence problem in which temperatures in the last half of the 20th Century increase while tree ring widths and densities decrease. They offered no solution other than reduced confidence, but the problem is worse. How can we even trust the shape of the curve in previous warm periods if they miss the present one? Bias sampling can arise not simply from Mann's principal component methods, but by non-random and biased selection of small samples. In this graph shown here, even the selection of a single site, of a different version from a single site can have a dramatic impact on a worldwide reconstruction. Here different versions impact the Briffa 2000 reconstruction and all but one subsequent reconstruction shown in the various spaghetti graphs. The issue of the polar Urals is substantive. Naurzbaev et al., which included Mann's coauthor Hughes, whose methods were cited by the NAS panel with a approval, concluded that medieval summer temperatures in this area were over 2.3 degrees Centigrade warmer than at present.

The Wegman reported noted pervasive problems in paleoclimate research practices. A simple policy shown here already in existence at the *American Economic Review* and other journals and in fact a policy introduced by Dr. Bernanke, presently Chairman of the Federal Reserve System, would alleviate many of these problems. There is no reason for journals not to adopt similar rules for paleoclimatology where data sets are similar in size and scale to many econometric studies. In fact, the 1991 policy statement of the U.S. global change research program already requires data archiving and many agencies such as NASA have complied with these policies. However, the National Science Foundation does not and a senior NSF official wrote to me saying that dissemination of data was merely up to the professional judgment of the researchers. Ironically, even the NAS panel relied heavily on unarchived data. The Department of Energy itself does not comply. It funded the development of the well-known CRU temperature series used by IPCC but their agreements failed to ensure that even DOE has access to the supporting data.

Nothing that I say here should be construed as diminishing the seriousness of climate change as public issue. It is precisely because it is a serious issue that policymakers are entitled to the best possible information. You should not receive incorrect confidence assessments as happened with the hockey stick. You should discourage practices that interfere with efforts to verify results.

Finally, at the NAS press conference, when asked about overselling of the hockey stick, panelist Cuffy said that the IPCC sent a very misleading message through its prominent use. Yet IPCC procedures which permitted this remain unchanged for the upcoming fourth assessment report.

Thank you very much.

[The prepared statement of Mr. Stephen McIntyre follows:]

PREPARED STATEMENT OF STEPHEN MCINTYRE, TORONTO, ONTARIO, CANADA

SUMMARY

1. little reliance can be placed on the original MBH reconstruction, various efforts to salvage it or similar multiproxy studies, even ones which do not use Mann's principal components methodology;
2. peer review as practiced by academic journals is not an audit, but something much more limited. Scientific overviews, such as ones produced by IPCC or the NAS panel, are nearly entirely based on literature review rather than independent due diligence.
3. much work in dispute is funded by the U.S. federal government. Some very simple administrative measures under existing policies could alleviate many of the replication problems that plague paleoclimate.

TESTIMONY

Good morning, Mr Chairman and members of the Committee.

My name is Stephen McIntyre. I appreciate the invitation to appear today to discuss my research, coauthored with Ross McKitrick of the University of Guelph. Our publications led in part to the reports of the NAS panel and the Wegman committee.

A year ago, the University Corporation of Atmospheric Research (UCAR) issued a national news release stating that our "highly publicized criticisms of the MBH graph are unfounded." Sir John Houghton, co-chair of IPCC, gave evidence to a Senate committee, stating that our results had been shown to be "largely false". The situation today is different as both the NAS and Wegman reports have recognized our major findings while drawing different conclusions on their impact.

I would like to convey three main messages today:

1. little reliance can be placed on the original MBH reconstruction, various efforts to salvage it or similar multiproxy studies, even ones which do not use Mann's principal components methodology;
2. peer review as practiced by academic journals is not an audit, but something much more limited. Scientific overviews, such as ones produced by IPCC or the NAS panel, are nearly entirely based on literature review rather than independent due diligence.
3. much work in dispute is funded by the U.S. federal government. Some very simple administrative measures under existing policies could alleviate many of the replication problems that plague paleoclimate.

In the NAS and Wegman reports, only one topic has been specifically "audited" – in the sense of carrying out independent simulations as opposed to review of previous literature:

- Mann's principal component method is biased towards producing hockey stick shaped series.

Both audits verified this result, first published by us, but hotly contested for the past two years. Both panels agreed (with varying emphasis) that MBH confidence claims were incorrectly calculated, indeed that no confidence intervals prior to 1600 could be calculated and that MBH statistical methods were unsatisfactory.

The Wegman report considered why such an error could have remained undetected in such a prominent study, an issue not considered by the NAS panel. In addition to their comments, I note that IPCC does not verify information from the scientific literature.

The NAS panel also endorsed our important criticism of MBH dependence on proxies known not to be temperature proxies, agreeing that bristlecones should be avoided.

The NAS panel cited several other reconstructions, but their consideration was merely a literature review. They did not attempt to replicate or audit these other studies and cannot vouch for them. Having examined most of them closely, I do not believe that any of them provide robust or reliable information on relative medieval-modern levels.

For example, some comments of Dr Bloomfield's at the NAS press conference may lead people to believe that a hockey stick could be obtained from a simple average of all 415 MBH proxies. This is not the case, as shown in Figure 1 below.

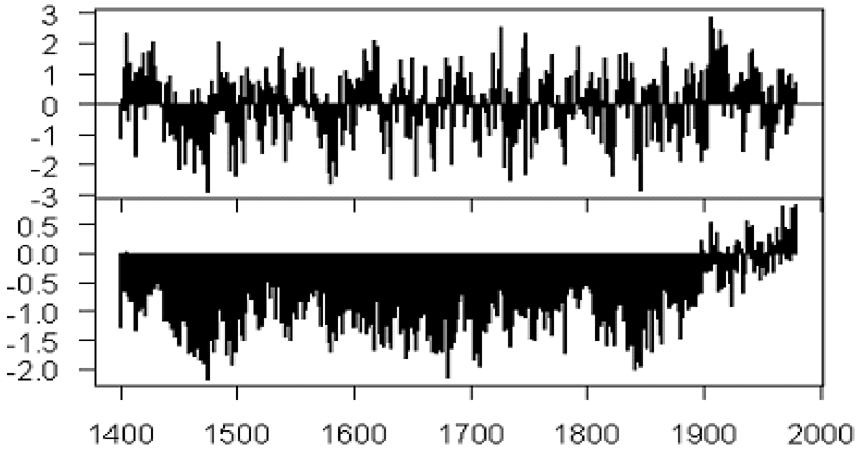


Figure 1. Top – Average of all 415 MBH proxies; bottom – MBH reconstruction.

The NAS panel illustrated four other multiproxy studies, as shown in Figure 2 below. However, all four use bristlecones or closely-related foxtails. The panel did not analyse the impact on each study of avoiding bristlecones, as they elsewhere recommended.

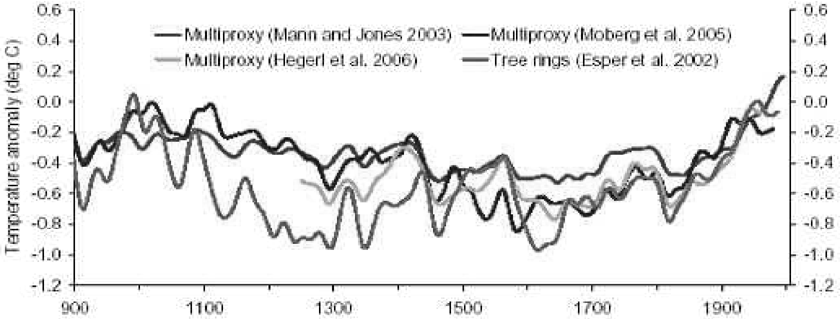


Figure 2. Excerpt from figure S-1 of NAS panel report

The impact of avoiding bristlecones in accordance with the NAS recommendation can be substantial – as shown in Figure 3 for Crowley and Lowery 2000, where the removal of two bristlecone series changes relative medieval-modern levels.

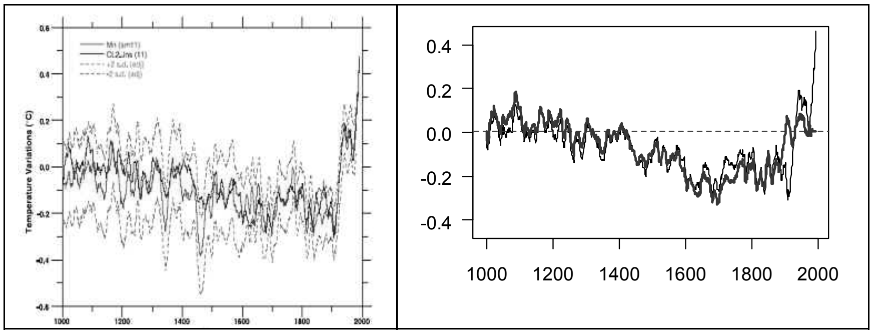


Figure 3. Left – Excerpt from Crowley (2000); right – replication with red showing effect without bristlecones and without instrumental splicing.

The NAS panel noted the so-called “Divergence Problem”, in which temperatures in the last half of the 20th century increase, while tree ring widths and densities decrease, demonstrated here for a rare large-sample (387) study of “temperature-sensitive” sites [Briffa et al 1998]. NAS offered no solution other than reduced confidence. But the problem is worse: how can we even trust the shape of the curve in previous warm intervals, if they miss the present one?

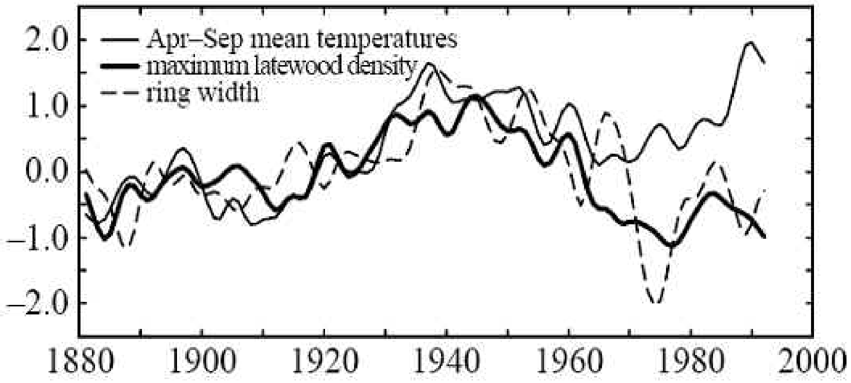


Figure 4. Ring widths and density from Briffa et al 1988.

Biased sampling can arise not simply from Mann’s principal component methods, but from non-random and biased selection of small samples. If you “mine” or “snoop” a network of red noise looking for what appear to be “temperature-sensitive” trends, an average of the picks will also yield a hockey stick shaped series. The Wegman report shows evidence of non-random picking. While the NAS panel noted the potential impact of inclusion/exclusion of even individual series, they did not investigate it. Here is an important example that affects multiple studies. The first Briffa version of the Polar Urals series said that the early 11th century was among the coldest of the millennium; updated sampling in 1998 showed the opposite, but Briffa did not report it. Instead he substituted another series from a site 70 miles away with a hockey stick shape. This substitution had a dramatic impact on the medieval-modern relationship in the Briffa (2000) reconstruction and nearly all other subsequent studies.

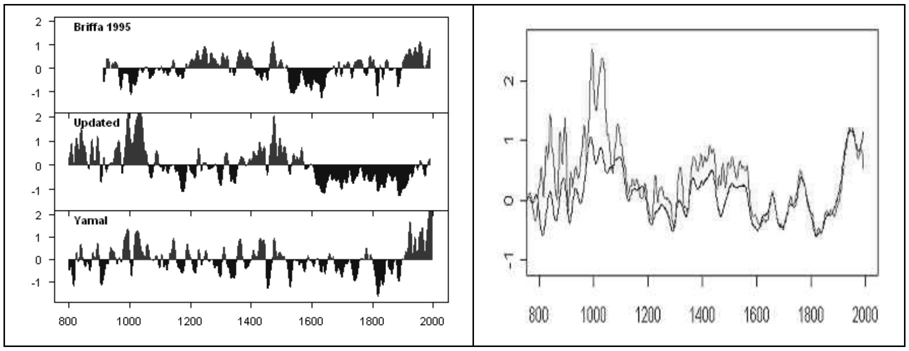


Figure 5. Left – three different versions of Polar Urals series. Top – from Briffa et al 1995; middle – from Esper et al 2002 (the only use of this version); bottom – the version in Briffa (2000) and subsequent studies other than Esper et al 2002. Right: the impact on the reconstruction in Briffa (2000). Black – Briffa (2000) version; red – using Polar Urals update. . All series in standard deviation units and 21-year gaussian smooth.

In our NAS presentation, we cited Naurzbaev et al 2004 (including MBH co-author Hughes) as offering a promising new line of handling tree ring data. NAS cited this with approval, but did not report their conclusion that medieval summer temperatures were over 2.3 deg C warmer or that medieval treelines in the Polar Urals (and elsewhere) were higher than modern treelines.

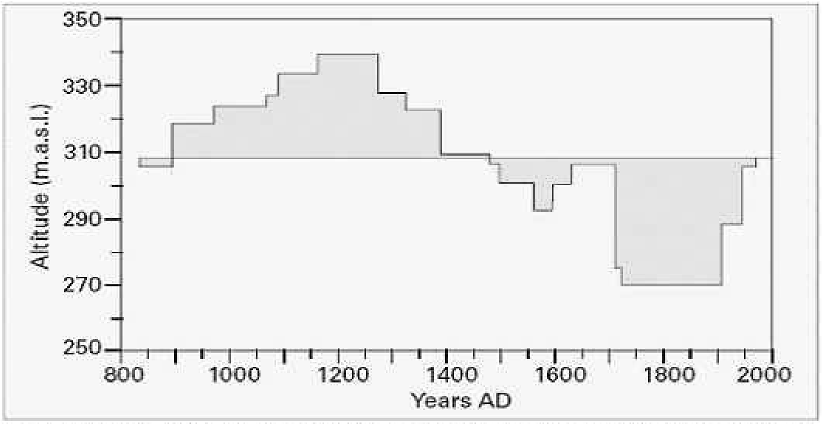


Fig. 1. Altitudinal displacement of the upper treeline in the Polar Ural Mountains during the last 1150 years.

Figure 6. Treelines at Polar Urals site (Shiyatov 1995).

While the NAS panel did not address the issue of archiving, other than in generalities, the Wegman report noted pervasive problems in paleoclimate research practices. A simple policy – already in existence at the American Economic Review and other journals - would alleviate many of these problems. There is no reason not to require similar rules for paleoclimatology, where data sets and code are similar in size and scale.

> Submitters should be aware that the Editors now routinely require, as a condition of publication, that authors of papers including empirical results (including simulations) provide to this office, in electronic form, data and code sufficient to permit replication.

To the extent that senior policy-makers have previously turned their attention to the matter, the 1991 Policy Statement of the U.S Global Change Research Program already requires data archiving after a limited period of exclusive use and, in 1997, provided recommended language for agencies to implement in grant agreements. Many agencies (e.g. NASA) have complied with these policies.

> The overall purpose of these policy statements is to facilitate full and open access to quality data for global change research. They …represent the U.S. Government's position on the access to global change research data….
>
> **For those programs in which selected principal investigators have initial periods of exclusive data use, data should be made openly available as soon as they become widely useful. In each case the funding agency should explicitly define the duration of any exclusive use period.**

Yet when I copied NSF on a request for data necessary to replicate key MBH results, a program officer not only refused to support the request, but intervened to counsel Mann against supplying the data.

> Dr. Mann and his other US colleagues are under no obligation to provide you with any additional data … His research is published in the peer-reviewed literature which has passed muster with the editors of those journals and other scientists who have reviewed his manuscripts. You are free to your analysis of climate data and he is free to his.

Subsequently, a senior NSF official said that dissemination of data was merely up to the "professional judgement" of the researchers. Ironically, the NAS panel relied heavily on unarchived data.

> In general, we allow researchers the freedom to convey their scientific results in a manner consistent with their professional judgement…

The Department of Energy funded the development of the well-known CRU instrumental temperature series, used by IPCC and others. In response to a request for supporting data, Philip Jones, a prominent researcher said:

> We have 25 or so years invested in the work. Why should I make the data available to you, when your aim is to try and find something wrong with it?

Although DOE had funded the collection, their past and present grant agreements had not ensured that even DOE had access to the supporting data and they said that they were unable to assist.

> Phil [is] not obligated under the conditions of past or present DOE proposal awards to provide these items to CDIAC. I regret we cannot furnish the materials you seek

In conclusion, I re-iterate that you can place little reliance on any existing multiproxy study; that you need to distinguish between the limited due diligence of journal peer review and the substantive due diligence of an audit; and that simple administrative measures can substantially improve paleoclimate research practices.

Both the NAS report and Wegman reports are valuable studies by accomplished authors. Nothing that I say here should be construed as diminishing the seriousness of climate change as a public issue. It is precisely because it is a serious issue that policy-makers are entitled to the best possible information and should ensure that data, code and methods be accurately and completely archived and discourage practices that interfere with scientific reproducibility.

References:

See NAS Panel report.

Presentation to the Subcommittee on Oversight and Investigations of the House Energy and Commerce Committee.

Stephen McIntyre
Toronto Ontario

Washington DC, July 19, 2006.

Overview:

1. little reliance can be placed on the original MBH reconstruction, various efforts to salvage it or on other similar studies, even ones which do not use Mann's principal components methodology;
2. peer review as practiced by academic journals is not an audit, but something much more limited. Literature review is not independent due diligence.
3. much of the work in dispute is funded by the U.S. federal government. Some very simple administrative measures could accomplish much improvement regardless of one's view on climate policy.

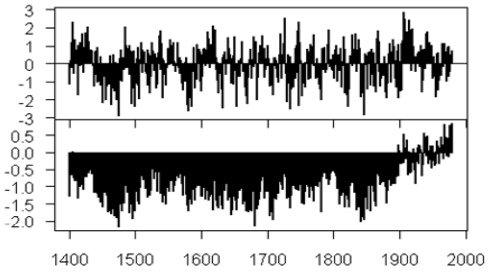
Both Panels Agreed …

- MBH principal components method was biased toward producing hockey-stick shaped series
- MBH claims to statistical significance were over-stated
- MBH claims to establish confidence intervals prior to 1600 should be rejected

The NAS panel also agreed:

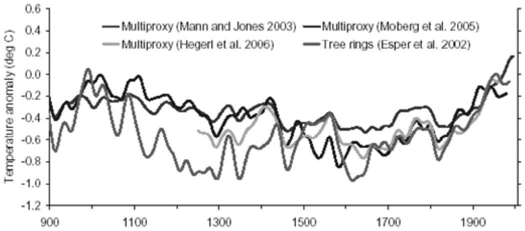
- MBH use of bristlecones should be avoided

A simple average of MBH proxies does not yield a hockey stick



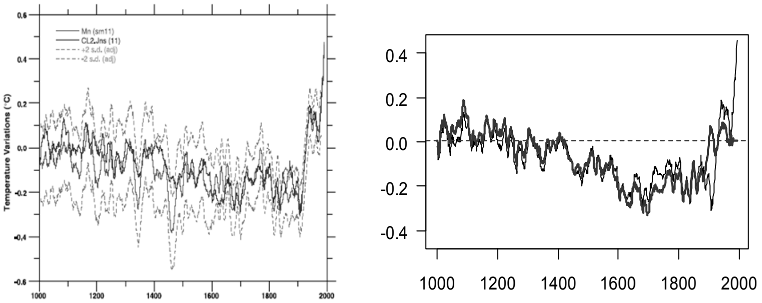
Top: average of MBH proxies. Bottom: result of MBH method.

NAS cited but did not audit other multiproxy studies



- all 4 rely on bristlecones; 2 of 4 even rely on Mann's PC method

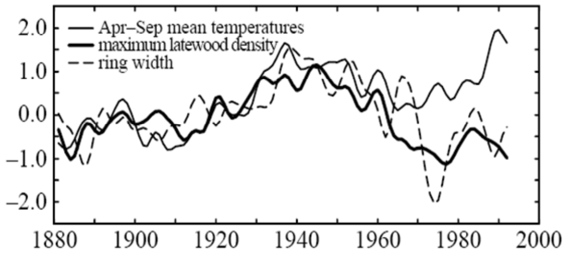
Avoiding bristlecones impacts medieval-modern levels in other studies



Left: Figure 4 of Crowley (2000) comparing that reconstruction to MBH. Instrumental data has been spliced since 1870.

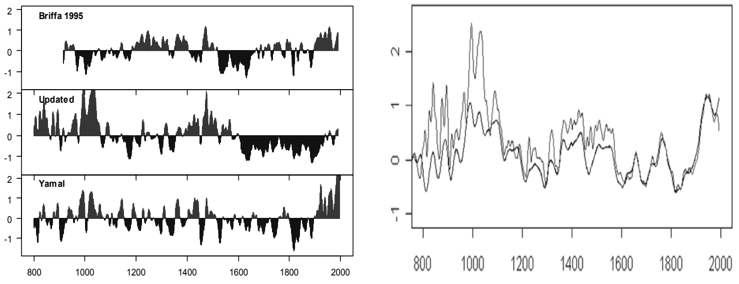
Right in red – Without bristlecones, horizontal line showing closing level with at least 5 proxies. No instrumental data is spliced.

Divergence problem:
Proxies trend down while temperatures trend up



Source: Average of 387 temperature-sensitive sites (Briffa et al 1998)

Polar Urals versions are inconsistent; selection impacts "worldwide" reconstruction



Left: Three versions of Polar Urals data used in multiproxy studies. Top and bottom series used in all but one study.

Right – red shows impact of using Polar Urals update in Briffa (2000) reconstruction

Medieval treelines were higher than at present in Siberia. Naurzbaev et al 2004 estimated that medieval summer temperature were warmer by more than 2.3 deg C.

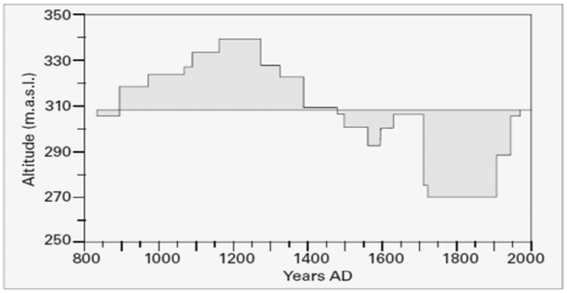


Fig. 1. Altitudinal displacement of the upper treeline in the Polar Ural Mountains during the last 1150 years.

Archiving policy at American Economic Review

Submitters should be aware that the Editors now routinely require, as a condition of publication, that authors of papers including empirical results (including simulations) provide to this office, in electronic form, data and code sufficient to permit replication.

1991 US Global Research Change Program Policy Statement

The overall purpose of these policy statements is to facilitate full and open access to quality data for global change research. They …represent the U.S. Government's position on the access to global change research data.

…

For those programs in which selected principal investigators have initial periods of exclusive data use, data should be made openly available as soon as they become widely useful. In each case the funding agency should explicitly define the duration of any exclusive use period.

NSF leaves compliance up to the researcher and the journals

Program Officer:

Dr. Mann and his other US colleagues are under no obligation to provide you with any additional data … His research is published in the peer-reviewed literature which has passed muster with the editors of those journals and other scientists who have reviewed his manuscripts. You are free to your analysis of climate data and he is free to his.

Senior Official:

In general, we allow researchers **the freedom to convey their scientific results in a manner consistent with their professional judgement…**

DOE funds collection of the most widely-used temperature data but fails to ensure access to it

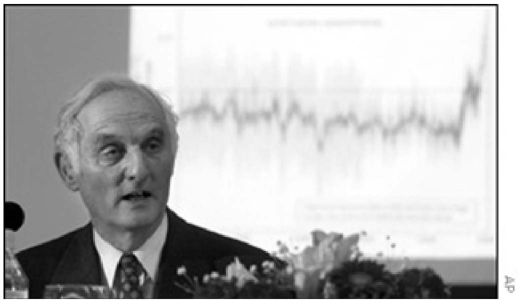
Jones:

We have 25 or so years invested in the work. Why should I make the data available to you, when your aim is to try and find something wrong with it.

DOE:

Phil [is] not obligated under the conditions of past or present DOE proposal awards to provide these items to CDIAC. I regret we cannot furnish the materials you seek

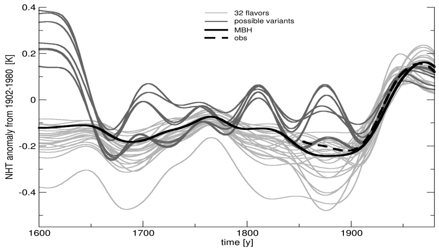
End of Presentation



Sir John Houghton at press conference releasing IPCC Third Assessment Report.

Robustness

- Burger and Cubasch (2005)



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MR. WHITFIELD. Thank you, Mr. McIntyre, and at this time I am going to recognize the full committee Chairman, Mr. Barton, for 10 minutes.

CHAIRMAN BARTON. Thank you. I want--let me thank Ms. Baldwin before she leaves. She and Mr. Inslee and Mr. Stupak have been here the entire time and I think they need to be given accolades. Mr. Whitfield and I almost have to be here but they don't, so we appreciate you all's attendance. I want to thank these witnesses for waiting 5 hours to testify. That shows a little bit of fortitude on your part.

My first question goes to you, Dr. Karl. Talking about the peer review and the acceptance, if I were to ask Mr. Inslee and Mr. Stupak and Ms. Baldwin to review the work of this committee in this Congress and then turn around and ask Mr. Whitfield and Mr. Walden and Mr. Shimkus, I would probably get two radically different assessments. Same body of work but my friends on the Democrat side would view the accomplishments in all probability substantially different than my colleagues on the Republican side because both are biased in an open and honest way and have a different worldview on some issues, not on all issues. So it shouldn't be surprising if the same people that Dr. Wegman calls a social network and are interacting all the time that they view positively the output, should it?

DR. KARL. Are you asking about whether or not the review process is skewed?

CHAIRMAN BARTON. No, I am just asking you to comment because I will stipulate that everybody in the climatology community, the environmental community, have got good faith and are trying to do what they think is right for the world. I am not--but there are biases on both sides, and one of Dr. Wegman's criticisms, and Dr. McIntyre reinforces it, is that you are not really getting independent review, and there are cases, as Dr. Crowley pointed out, there may not be anybody that can be independent because they don't understand it. If I want somebody to interview Albert Einstein's work in the 1930s, there probably weren't two or three people in the world that even knew what he was talking about, so you do get that, but what happened with Dr. Mann's study in 1998 was that it was accepted very quickly as kind of the gold standard and it was given a literary review, but it really wasn't given an independent scientific statistical review. It was just accepted. And unless Dr. McIntyre is not being true, some of these other studies that have come out that Dr. Crowley referred to, he used the same data sets and the same modeling or something that is very close to it. So how can us poor mortals that have to make the policy decisions know what to believe when the so-called scientific community could be portrayed as scratching each other's back? I mean, I am not trying to be mean about it. You know, I just am kind of puzzled.

DR. KARL. I mean, I can tell you the process that we use in IPCC. It may shed some light on it. In the IPCC report, each of the lead authors are asked to assess the published literature up until a certain time after which no more new material can be considered and what lead authors do is take a look at that material and try to write up their consistencies among what has been published, inconsistencies, what is available today compared to what was available during either the previous IPCC report or previous to that. Having done that, those writings then are subjected to international review. Anyone and everybody is open to review to report and the process takes place over several years. So there is ample time, ample review time--

CHAIRMAN BARTON. But do they really review it? Again, I am not saying that your folks don't make a good-faith effort, but it is just like my analogy. If I asked Mr. Whitfield, who is a subcommittee Chairman because I appointed him subcommittee Chairman as Chairman, if I say Ed, could you review my performance as chairman of the full committee, I bet he is going to give me a pretty high performance rating. Now, on the other hand, if I asked Mr. Inslee to review my performance as full committee Chairman, and I have consistently opposed his amendments and I have consistently made life difficult for him, which is not true but let us assume that it is true, he is not going to rate me the same. In all probability, Jay Inslee is going to be more independent and objective than Ed Whitfield, and they are both good people. But one of them is more dependent on me, interacts more, benefits more with that interaction than the other and it appears to me that what Dr. Wegman and Mr. McIntyre are saying is, it may be because there are just not enough experts, it may be for any number of reasons, but a very small set of people review each other's work and lo and behold, they all come to the same conclusions.

Now, we didn't put it into the record, but in 1975 we have the *Newsweek* story about the meteorologists all being unanimously in agreement that the world is in a world-cooling period and it has catastrophic consequences and there was unanimous agreement. It was la di la di da. Those were meteorologists. Now, that is 31 years ago. The world has changed. We are now worried about global warming but it the same thing. You know, I am not qualified to say whether the conclusions are right or wrong. I agree with what Dr. Wegman said and Dr. North said, that--I can't conclusively say what is causing it. I can admit that the statistical record in the last 150 years that the temperature is going up, but I would like to see the scientific community self-regulate itself a little bit better so that when you have these statements like Dr. Mann made that the 1990s were the warmest period in 1,000 years and 1998 is the warmest year in 1,000 years, that you can replicate that with statistically valid modeling technique that is open to the public and everybody takes their shot at. I think we have pretty conclusively proven today that that is not the case, at least in that study. That is not the case. So that is my question to you, what can the scientific community do to give us more certainty or more reliability that the conclusions of these studies are really based on fact and not on opinion.

DR. KARL. I suspect, and I don't know for sure, but if you request the records from the IPCC Bureau, for example, you could--because it is public--you could get available the disciplines of the individuals who commented on that report and I note there is an IPCC report going on now, and that may be a way for this committee to try and see the breadth and scope of--

CHAIRMAN BARTON. Well, are you willing to recommend that--one of the recommendations of Dr. Wegman is that the data be publicly available? Is that something that you would support? Because we have apparently had a real problem with Dr. Mann, getting his data and, it has been federally funded. I think it should be available, that anybody who has the scientific ability and the mathematical ability to study it, study it. Do you agree with that?

DR. KARL. Yes. Our Center actually houses the Paleoclimate World Data Center and we actually encourage researchers to archive their data, not the actual proxy itself like the tree ring or the ice core but the data from which they are derived. We are fairly successful in many instances, but I am sure there is a number of instances where we don't have data simply because of its significant investment on both the PI's time and--

CHAIRMAN BARTON. And either Dr. North--I think Dr. North's report, or it may have been Dr. Wegman's, says there are only 30 of these data sets in existence right now, that there are a fairly limited number of data sets. So we are basing a lot of decisions on a fairly narrow band.

Let me ask you something, Mr. McIntyre. Since you had the gumption to criticize Dr. Mann, how have you been received in this community. Are people patting you on the back and inviting you to their Christmas party and saying right on, way to go, we really appreciate it, or are they kind of giving you the cold shoulder and ask why the hell you did what you did?

MR. MCINTYRE. I would say cold shoulder would be overstating the friendliness of it. I would say that I have been reviled and--

CHAIRMAN BARTON. And so your skepticism for scientific truth has not been welcomed with open warms. Is that a fair statement?

MR. MCINTYRE. I would say it has been an uphill fight. Having said that one finds certain allies and certain moments of comfort. I mean, quite frankly I could understand why there would be some reluctance to take the claims seriously at the beginning. That is one of the reasons why I archived the source code and calculations so that people could replicate it. Aside from the fact that I think it is something that should be done anyway, but my position was if anybody thinks that my results are wrong, then I would like to know. I would like to be the first person to know rather than the last person to know, and--but I--for example, the University Corporation of Atmospheric Research put out a national press release saying that all our claims are unfounded. Sir John Houghton, co-chair of IPCC, testified to a Senate committee that our claims were false. So while I would say not all of our claims have been acknowledged, some of them have. Both of these reports have certainly endorsed a finding on methodology that surprised people and so, I feel a little more comfortable now. Also, some people have been very generous and welcoming. Dr. von Storch has encouraged me both publicly and privately.

CHAIRMAN BARTON. Dr. Crowley, this might be my last question. You mentioned in your oral statement--I didn't see it in your written statement but it may have been there--that there have been problems in the past with correlation of current temperature readings and their consequences with satellite readings and that those correlations are much better today. Is that true? Did I--

DR. CROWLEY. Yes, that is true.

CHAIRMAN BARTON. Now, my understanding is that what changed is that we have gone back and reprogrammed the software on the satellites so that they will conform with the model predictions. Do you agree or disagree with that?

DR. CROWLEY. I completely disagree with that.

CHAIRMAN BARTON. Can you push your--I don't know that your microphone is on. You said--I think you said--

DR. CROWLEY. I disagree with you. It is not the case of trying to get it to conform to model predictions. In fact, it stuck out like sore thumb for 10 years. The climate community took it very seriously as a disagreement and pondered over it and there was eventually a comparison between two different groups of satellite analysts in which they found a programming error in one of the algorithms for reducing the data that gave the differences in the trends because this other group actually had gotten a bigger trend in the satellite data than the one that John Christy at University--

CHAIRMAN BARTON. Do you think that that disagreement is worthy of being pursued by this subcommittee?

DR. CROWLEY. Well, what has happened is that the disagreement has diminished to the point where I am not sure it is worth the subcommittee's effort to inquire. It has been found to be a programming error, and an innocent one but that happens when you are working with satellite or any other thing. It just took a long time--

CHAIRMAN BARTON. Do you consider Dr. Mann's methodology a programming error? If you were Dr. Mann and--

DR. CROWLEY. No, because I don't think he actually wrote--I don't think his programs--when it is a programming error, it is like a coding error or something. I think that there is a methodological error, okay. There is a difference between, as you know, since you took programming, between the--you can program a methodology that could be wrong, okay. So I don't think it was programming. I think it was a methodological error.

CHAIRMAN BARTON. Well, my time--

DR. CROWLEY. Not a--yeah, a methodological.

CHAIRMAN BARTON. Thank you, Mr. Chairman.

MR. WHITFIELD. Mr. Inslee, you are recognized for 10 minutes.

MR. INSLEE. Thank you. I wanted to ask Mr. Karl about the conclusions, if I can find them here. In your testimony you talked about reviewing a variety of papers and you said of all the analysis, only one shows temperatures during medieval times higher than those of the early

20th Century and none of the analyses show temperatures higher than the last few decades of the 20th Century and into the 21st Century. So I take it that means that none of the analyses that have been done have shown temperatures at any point higher than the last few decades and into this century. Is that an exhaustive review of the analyses or is there something you might have missed or is that pretty much a total review of the literature on this?

DR. KARL. There is always a danger one could have missed a report but none of the reports I looked at, which probably seven or eight reports using the approaches from as been discussed here. I think "bonehead" is a term and RPG and various terms have been given to these things, but I don't think any of them show temperatures, except for one, that were as warm as what we saw in the mid part of the 20th Century, none of them as warm as the late part of the 20th Century and the early part of the 21st century, and in addition, I might add the error bars are frequently being discussed. If you look at the error bars, the wide error bars, the 95 percent confidence error bars, it is even hard to find in those error bars in those reports to come up to the levels as high as we see in the last couple decades.

MR. INSLEE. So is it a fair synopsis here that today we have heard some criticism of one report that suggested that these are higher temperatures we are experiencing now than we have at any time in the last 1,000 years and multiple reports that have reached the conclusion that it is likely we are having higher temperatures right now than we did at any time in the last 1,000 years. Is that sort of a fair statement of what we are hearing?

DR. KARL. I think so, and again, the word "likely" you know, I point out, we use the word "likely" with better than two to one odds and so with that kind of a caveat, I feel quite comfortable in saying that.

MR. INSLEE. Well, the way I look at this, just so you know, is that you have got about six studies showing that gravity exists and you have got one study questioning the statistical mechanisms used in one of those six studies, and I sort of conclude that both gravity and global warming due to human activity exist, and that is just how I look at it. I want to refer--you also concluded, "These analyses indicated that the later half of the 20th Century is certainly warmer than any time during the past several hundred years, parentheses, based on the length of the borehole and glacial length proxies, paren, and the past 1,200 years based on isotopic ice core records." So you indicated that these are warmer during the past several years and you say based on the length of the borehole and glacial length proxies. What are those two proxies?

DR. KARL. Those are proxies that are completely independent of the tree ring analysis which is heavily used in some of these multi-proxy reconstructions. But the borehole measurements are--there is probably about--I think the academy actually gave a number of about 679 different boreholes where the conduction of heat from the atmosphere is constantly conducting into the Earth's surface and you can go back in time to try and deduce what the actual temperatures were in the lower parts of the atmosphere. Now, you have to be careful which boreholes you look at but nonetheless, with current methods, you can go back to about 400 or more years. That was an important piece of evidence that when we did the IPCC in 2001 we intercompared those borehole measurements with the Mann record, for example.

MR. INSLEE. So as I take it then, we have got totally independent results independent from the Mann analysis that is consistent with the conclusion that it is likely that we are in warmer temperatures now than we have been in the last several hundred years. Now, you made reference to glacial length proxies. What are those?

DR. KARL. Now, the glacial length proxies, this is where a model was used to try and look at the ablation of glaciers across primarily the Northern Hemisphere and a model has been shown to be able to reproduce approximately the temperatures that would be needed to cause those glaciers to melt. Again, you have to be careful about what glaciers you select. Some of them are more sensitive to precipitation but nonetheless another independent method, and again, it shows that the later part of the 20th Century is warmer than anything we have seen in the last several hundred years.

MR. INSLEE. Now, you also reported that these analyses indicated that these temperatures we are now experiencing are warmer than in the past 1,200 years based on isotopic core records. Are the isotopic core records independent of the Mann research and could you describe what they are?

DR. KARL. Yes. They are independent as well. The difference is, they are far fewer in terms of geographic coverage. So what you are actually looking at here is the isotopic decay within these records, the same kind of records that are looked at for the air bubbles that are trapped in the ice. Now you try to relate through isotopic decay to temperatures and there are some relationships that have been developed and again you see some significant warming in the latter part of the 20th Century compared to what we saw earlier.

MR. INSLEE. So we have multiple independent scientifically sound measures to conclude these are the likeliest warmest temperatures we have had in 1,000 years independent of the Mann report. Is that correct?

DR. KARL. That is correct.

MR. INSLEE. Dr. Crowley, you talked about something that I had heard and I appreciate you talking about it, about amplitude, about the effect of how much amplitude there is in the system, how sensitive the system it is to CO_2 forcing, and I think this is interesting because basically the *Wall Street Journal* editorial staff has done everything they can to suggest this is not a problem and they have attacked the Mann research effectively saying that, but is it fair to say that actually if one would want to debunk the idea of global warming, if one would want to say we shouldn't worry about global warming, if one would want to say that we should really just continue on our path of putting megatons of CO_2 in the air without change, if one really wanted to argue that, one would really want to argue that Mann was right because Mann had a conclusion that there was less effect on temperature by CO_2 changes than some of the other studies. Is that right?

DR. CROWLEY. That is true. Another way of putting it is that those who love to hate Mann should learn to hate to love him.

MR. INSLEE. Well, that will take us about 8 minutes to figure out up here on this panel. But could you explain why that is? I just heard this yesterday for the first time. It is an intriguing thought, that this could be a reversal of approaches here, but why is it important to know how much CO_2 can affect temperature and what does the Mann research indicate versus other research?

DR. CROWLEY. Well, it is like pushing on a string. Jerry North explained this to me years ago. Suppose you have two strings, one that is very thick, coiled spring, and then another one that is very thin and weak. You push on the thick coiled spring, it is not going to move very much whereas one that is very flexible is going to move a lot, and that is really like pushing is like the climate forcing the responses to climate system, so if you have a system that has a very low sensitivity, it is not going to respond much, like the thickly coiled spring. You have one that is less thickly coiled, it is going to respond more and you are going to get bigger temperature changes and that is the thing we worry about, is whether the temperature change is being large, and the study came out recently in *Nature* where we tried to quantify that and that at least with respect to the paleoclimate records and showed objectively what I was saying--

MR. INSLEE. So if Mann was wrong, this problem that we are going to be at in 2100 when CO_2 levels are twice the rate of pre-industrial times--

DR. CROWLEY. We are going to have larger temperature variability.

MR. INSLEE. So if Mann is wrong, that means we are going to have greater increases in temperature once this CO_2 levels skyrocket like this and even some of the other researchers have predicted. Is that the situation?

DR. CROWLEY. Right.

MR. INSLEE. That will news to the *Wall Street Journal* editorial board.

DR. CROWLEY. Sure.

MR. INSLEE. Thank you.

MR. WHITFIELD. Thank you, Mr. Inslee. Dr. von Storch, in your presentation you made the comment that parts of climate change science, in particular paleoclimatic reconstructions have suffered from gatekeeping and incestuous usage of reviewers and then you talked about they have a bias toward interesting results, and we have a lot of testimony today about the *Wall Street Journal* and the oil industry and the coal industry love to debunk all of this science about global warming, which may be true, but I was interestingly reading an article the other day about a gentleman named Chris Landsea, who was on the IPCC panel and was an expert in hurricanes. And we heard testimony today in some of the opening statements that global warming is causing more hurricanes, stronger hurricanes and it is a serious problem. Henry Waxman is the one that made that comment and there was another person that made that comment. And Chris Landsea was asked by a gentleman named Dr. Kevin Trenberth to provide the write-up for the AR4 assessment, the fourth assessment report of the IPCC. He was asked to do the write-up for the Atlantic Hurricanes, and soon after he was asked, Dr. Trinberth went to Harvard University and participated in a program entitled on the topic: “experts to warn global warming likely to continue spurring more outbreaks and intense hurricane activity.” And there was big press about it and there were all sorts of articles written about it. And Landsea was so upset about this as they were just getting ready to do this assessment that he submitted his resignation. And he said, “It is beyond me why my colleagues would utilize the media to push an unsupported agenda that recenty hurricane activity has been due to global warming. Given Dr. Trenberth’s role as the IPCC’s lead author responsible for preparing the text on hurricanes, his public statements are so far outside of any scientific understanding led me to concern that it would be very difficult for the IPCC process to proceed objectively with regards to the assessment on hurricane activity.” Now, we are all human beings, we make a lot of mistakes. We are biased. We do this, we do that. But is that something that happens in the IPCC frequently or infrequently or do you have any comment about it?

DR. VON STORCH. Only through the media, and I had the impression that this was not very helpful, what has happened there, but I don’t know the details, and this would be an example where I would ask some social scientists to really go after this, what really has happened here. I think it would be worth doing it. But when we speak about this storm business, I would like to tell a little story, namely in the early 1990s we had the press in northern Europe full of messages that we would have more storms, and these storms would be proof or would be a result of global warming going on. And you have to know that when people think about climate change, anthropogenic climate change in the past, it always is associated with more storms. So if you read about the cooling in the 1970s, what the response would be, it was cooler and more stormy, so it seems that it is part of our cultural heritage that whenever we think we change climate to the worse, then we have more storms. Later on it turned out that we actually have less storms now in northern Europe. And if we believe our climate change models, and I do believe them and I am sincerely convinced that we see global warming happening. If we believe these models then we should have an intensification of storms in our part of the world with stronger wind speeds of the order of 10 percent of the end of the century, that would be a signal which cannot be detected. While if you go into the details, then you find out that several aspects are rather similar to the ongoing hurricane debate, namely that good data exists only for a short time. Satellites are flying only since the 1970s or so, and observing this, and you have decades with strong activity and decades with less strong activity. It is the same with the storms in our part of the world. And so I would say in this case one should be very careful in making definite conclusions about that. And if we believe our models, and I am not sure if we should believe in this respect our models, then we also should have a signal which is much weaker now, hardly detectable at this time. So in this case with the hurricanes, I would advise to wait a little bit before definite conclusions are drawn. And this would be an example that somehow this preconception that storms are getting worse when climate is changing is somehow controlling what we think.

MR. WHITFIELD. Mr. McIntyre, I know that you and Mr. McKitrick were the ones that first started looking at the Mann study or report. How did that come about? Was this just an area of interest that you have had, or what?

MR. MCINTYRE. Well, that is actually a fairly long story but I was just--at that time I was just a private citizen. The study was being--we were told in Canada that 1998 was the warmest year of the millennium. I have worked in the mineral exploration business for many years. I deal with geologists who were unimpressed by that statement and I just wondered one day how they knew that. When I looked at the IPCC report as somebody that is in the mineral exploration business, which is a very promotional business, I was struck at how promotional many of the statements were and particular how promotional the hockey stick graph was. I thought actually sort of in a professional way, I thought it was well designed, well presented. It was there to convey a message but I certainly felt like I was being sold when I saw that. Some months later, business was slow. I thought I would be interested in looking at the data. I assumed there was some kind of due diligence package like you would see in a business thing that they had prepared for the IPCC auditors. At that time I had no idea that such things didn't typically exist in the academic community so I e-mailed Dr. Mann out of the blue and asked him where the data was and just for the location of the data of this which I assumed to be part of the due diligence package and he said he had forgotten where the data was. So I was astonished as there had been so much publicity. He said he would have an associate locate it for me. The associate said that it wasn't in any one place, but he would get it together for me so I thought that was nice of him but just, it seemed an odd situation and I just thought well, nobody has ever looked at this and if nobody has ever looked at it, well, I will do it, so I didn't expect to be the center of an academic debate or any furor, but when I looked at it, I started finding problems and here we are today.

MR. WHITFIELD. And I would ask Dr. Crowley and Mr. McIntyre or anybody else that wants to comment: the *Wall Street Journal* that has been referred to many times today says that Dr. Mann's methodology could produce hockey sticks from random trendless data. Is that a correct statement or is that incorrect statement?

MR. MCINTYRE. Well, let me answer that. That is true, and that is the one specific item that was verified by both panels, and both the NAS panel and the Wegman report specifically confirm that his methodology would produce a hockey stick from random data.

MR. WHITFIELD. Okay. Dr. Crowley, did you want to comment on that?

DR. CROWLEY. I am not an expert in statistics so I just have to defer from that answer. All I can say is that when we took a completely different approach with the very simple averaging, we got an answer that was pretty similar.

MR. WHITFIELD. Dr. von Storch?

DR. VON STORCH. I think I have a bit of reputation in studies of climatology as I am the coauthor of I would say the leading statistics book in that field. So first of all, what Mr. McIntyre is saying is correct. You can get that. But this requires that you have no other significant signals in the field, in particular no correlation in space, and this is not the case in climatological variables and so I would say even if it is entirely true what he said and I would include it in the next version of this book we have written. I would say in very many practical situations it would not show up.

MR. WHITFIELD. Thank you. My time has expired.

CHAIRMAN BARTON. Has Mr. Stupak not gone yet?

MR. STUPAK. No.

CHAIRMAN BARTON. Well, then let us get Mr. Stupak. He has waited patiently all afternoon.

MR. STUPAK. Dr. Karl, if I may, Dr. von Storch says that the reputation of the IPCC has increased to very high levels in the past years, that most lead authors are honest brokers of the work they review and that perhaps in such a complex and large field as the IPCC is addressing, it may not be possible to have lead authors who have not contributed to the field. But then Dr. von Storch concludes that an independent review by the IPCC is not possible under the current system. How would you respond to that?

DR. KARL. Again, no human-conceived system is perfect. I don't know how you might improve it in terms of the way it operates today. The peer review process really is driven by others' availability to comment and the IPCC documents are open for everyone from every discipline to comment on including the governments of the world. I think one of the issues that has been discussed in the hearing today is one that is typical of science where you can publish something but sometimes it takes a period of years to try and come up with a different analysis, technique, or to explore the decisions that are made in a particular analysis technique. The IPCC process right now is over a period of 2 years. I don't see how you could actually open up a process more and I don't see how you could actually have a process whereby every piece of information is going to be evaluated in terms of a new analysis, and that is the reason it is done every 5 or 6 years to update, see if there are differences. So, for example, I am sure all the work being done since the 2001 IPCC assessment and the next one that is coming out next year will be included and assessed.

MR. STUPAK. Well, the 2001 IPCC report really referenced other studies other than the 1998 and 1999 Mann hockey stick study, right?

DR. KARL. Yes. In fact, as I said, it would have been--I hate to use the words "very unlikely" because those are like the words that are used in the IPCC but I don't think IPCC would have actually made a statement about the 1990s had it only been based on one article. If it was just the Mann work, I just don't think we would have had the confidence to say anything.

MR. STUPAK. I am looking at your 2001 report here, and I am on page--and in there it says new analysis of proxy data for the Northern Hemisphere indicate that the increase in temperature in the 20th Century is likely to have been the largest of any century during the past 1,000 years. It is also likely that in the Northern Hemisphere the 1990s was the warmest decade and 1998 was the warmest year. That was the conclusion of 2001 and that is based upon more than just the Mann study. Isn't that correct?

DR. KARL. That is correct.

MR. STUPAK. Okay. You used the word "likely." I know today especially when the Chairman asked questions it was like absolute based upon the Mann study and that is not the case, it based upon--your 2001 report takes some other things other than the Mann hockey stick study, right?

DR. KARL. That is correct.

MR. STUPAK. What is the significance of the word "likely"? Not working in your field, I may have a different view of "likely" but you use it twice. Can you give any further explanation of that?

DR. KARL. What we tried to do is clarify what we meant by the word "likely" because it can be taken all different ways because it is used frequently in the literature. We define "likely" as a probability of the statement being true between 66 and 90 percent of the time. That means slightly better than two to one odds at the low end, and at the high end close to nine to one odds.

MR. STUPAK. You have been here all day. Is there anything you have heard today which would make you change your mind about the conclusions of the 2001 IPCC report?

DR. KARL. No. If you ask me to give qualifications about the findings in the 2001 report with the same caveat in terms of defining likelihood, I personally would not change anything.

MR. STUPAK. And going further in this, your 2001 report, the IPCC report, they talk about the Jones et al., about having the warmest year of the past millennium in the Northern Hemisphere, Jones et al. in 1998 came to a similar conclusion from largely independent data and entirely independent methodology. Crowley and Lowery in 2000 reach a similar conclusion. Borehole data, Pollick, et cetera, in 1998 independently support this conclusion for the past 500 years. So there is plenty of other things to base that conclusion upon and not just the Mann--

DR. KARL. That is correct.

MR. STUPAK. And somewhere today someone said something like there is over 900 reports or studies on global warming. Is that correct?

DR. KARL. I am sure there is even more than that. I think that was a random sample, so there is probably in the tens of thousands.

MR. STUPAK. Okay. Thank you. Dr. Crowley, if I may, when I was asking Dr. Wegman about this chart here, which was showing the warm age there in around 1300 or so, I think he called it the cartoon graph, was his word. Is that based on any set of data or anything or--

DR. CROWLEY. That is pretty much a cartoon graph actually. This is really in the first round of IPCC. Nobody ever felt there was a need to--

had thought of whether there should be a need to have a quantitative estimate of climate for the last 1,000 years. They wanted to try to provide a perspective and they didn't realize they didn't have one and they basically talked to some people and there was a lot of anecdotal evidence for medieval warm period, that people said it was warmer than the present roughly during these years, you know, so it was really pretty much of a guesstimate, and it was only when we started looking at a number of sites that had a very good chronology so we knew where they were in time and that we realized that the timing of the warmth was not the same in different regions, that that peak collapsed.

MR. STUPAK. So it is not fair to compare this cartoon graph with Dr. Mann's hockey stick?

DR. CROWLEY. No, I don't think that was intention of Dr. Wegman. I think he was just--

MR. STUPAK. No, I guess the *Wall Street Journal* used it more as one of those. You said the Wegman report should not be a legitimate assessment of the science of global warming or as a guide to policy modification. Can you elaborate a little bit on that?

DR. CROWLEY. Well, I felt that--again, I have to--I can't remember exactly where--do you have it listed where I said that so I can--

MR. STUPAK. Let me find it here.

DR. CROWLEY. Last page. So what I said is I disagree with many in the fact sheet and also in the report itself. It is not like I disagreed with what he was saying about his analysis of the Mann et al. record there but some of the recommendations that he was making I think that I felt there was a need---I just disagreed with him and so I was concerned that in terms of recommending any changes. I am not saying that interaction with statisticians is bad. I strongly favor very enhanced interaction but a lot of that is already happening.

MR. STUPAK. Mr. McIntyre, you are not a paleoclimatologist, right?

MR. MCINTYRE. No.

MR. STUPAK. And you are not a statistician?

MR. MCINTYRE. I studied mathematics and statistics at university.

MR. STUPAK. So are you a statistician then?

MR. MCINTYRE. I have not practiced as a statistician, but this is what I have been doing for the last few years. I think that--

MR. STUPAK. You have been doing statistics the last 2 years then?

MR. MCINTYRE. I have been working at statistical analysis of multi-proxy studies for the last 3 years.

MR. STUPAK. Three years. Okay.

MR. MCINTYRE. I note that my findings have been endorsed by both the NAS panel and the Wegman report.

MR. STUPAK. In this--again, reading the *Wall Street Journal* editorial. I am not sure how accurate this is but it say you and Mr. McKitrick published an article in a peer review journal. What discipline did the peer review?

MR. MCINTYRE. We have published articles in two journals, *Geophysical Research Letters*, which is the same journal that published the original Mann article, and E*nergy and Environment*.

MR. STUPAK. Let me ask Dr. Crowley if I can. Both you and Dr. Karl and the National Research Council have stated that the Mann study was not the most influential work in the IPCC 2001 report. You testified that the papers that made the biggest differences were ones that said the influence of greenhouse gases had to be used to reconcile the data and the models and the most compelling driver was the fact that global temperatures kept going up and glacier melt was increasing. Why then is there so much emphasis on the Mann report?

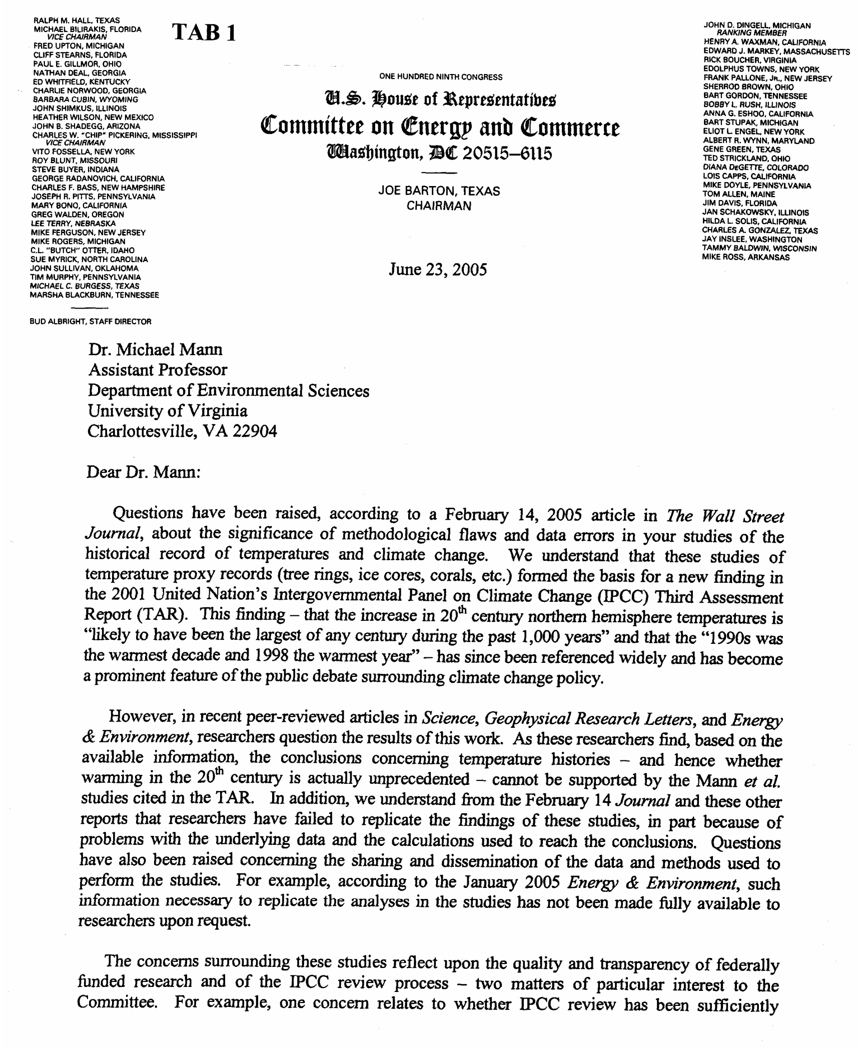
DR. CROWLEY. Well, there has been this discussion before about it being used as an icon, okay, and people say well, if it is not right, then is IPCC wrong, so there has then been that connection drawn. So I think for rightly or wrongly, I am not sure if IPCC is the only one responsible for broad--for using that as an icon but it has effectively become one and I think that is really the--what the--I guess the argument settles down to.

MR. STUPAK. Thank you. I guess my time is up. We have got 3 minutes to go vote.

MR. WHITFIELD. I want to thank all of you on the panel, one, for being here, two, for being so patient, and three, for what you do and the contributions that all of you are making. We may or may not have some more hearings on this. I know we do have an invitation out to Dr. Mann and we will see if he is going to come or not. But I want to ask unanimous consent that the document binder be submitted into the record of this hearing, unanimous consent that the document in *Newsweek* that Chairman Barton referred to about the cooling world be entered into the record and then I would like to keep the record open for 30 days for any follow-up questions we may have. So without objection, so ordered and this hearing is concluded, and thank you all again for being with us. We genuinely appreciate it.

[The information follows:]

Ex. #	Description	Date
1	Committee letters to Dr. Mann and response	6/23/05
2	Committee letters to Dr. Bradley and response	6/23/05
3	Committee letters to Dr. Hughes and response	6/23/05
4	Committee letters to Dr. Pachauri and response	6/23/05
5	Committee letters to Dr. Bement, Jr. and response	6/23/05
6	"The M&M Critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications, " by Stephen McIntyre and Ross McKitrick (Energy and Envrionment 16.1, 2005)	Jan-05
7	"Hockey Sticks, Principal Components, and Spurious Significance" Geophysical Research Letters, VOL. 32, L03710, doi:10.1029/2004GL021750, 2005	Jan-05
8	"Summary for Policymakers: A Report of Working Group 1 of the Intergovernmental Panel on Climate Change" and Excerpts of Chapter 2 of the Report "Observed Climate Variation and Change, 7.1-7.2"	Jan-01
9	"Surface Temperature Recontructions for the last 2,000 Years" (National Research Council) by The National Academies Press	
10	Ad Hoc Committee Report on the 'Hockey Stick' Global Climate Reconstruction	
11	Excertpt from Intergovernmental Panel on Climate Change: First Assessment Report	1990
12	Mr. David Vladeck, Esq. response to Committee	7/13/06
13	Mr. David Vladeck, Esq. response to Committee	7/14/06
14	Dr. Yasmin Said/Ender's Robinson Email	7/18/06
15	"Hockey Stick Hokum," *The Wall Street Journal*	7/14/06
16	Fast Moving Fronts - interview with Dr. Michael Mann	Jul-06
17	"The Cooling World," by Peter Gwynne; *Newsweek*	4/28/75
18	"While Washington Slept," *Vanity Fair*	May-06
19	Letter from National Science Academies	
20	"Finally Feeling the Heat", an Editorial by Greg Easterbook	5/24/06
21	An excerpt from Vice President Al Gore's book, An Inconvenient Truth	



TAB 1

RALPH M. HALL, TEXAS
MICHAEL BILIRAKIS, FLORIDA
VICE CHAIRMAN
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CLIFF STEARNS, FLORIDA
PAUL E. GILLMOR, OHIO
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ONE HUNDRED NINTH CONGRESS
U.S. House of Representatives
Committee on Energy and Commerce
Washington, DC 20515–6115

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June 23, 2005

Dr. Michael Mann
Assistant Professor
Department of Environmental Sciences
University of Virginia
Charlottesville, VA 22904

Dear Dr. Mann:

Questions have been raised, according to a February 14, 2005 article in *The Wall Street Journal*, about the significance of methodological flaws and data errors in your studies of the historical record of temperatures and climate change. We understand that these studies of temperature proxy records (tree rings, ice cores, corals, etc.) formed the basis for a new finding in the 2001 United Nation's Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report (TAR). This finding – that the increase in 20th century northern hemisphere temperatures is "likely to have been the largest of any century during the past 1,000 years" and that the "1990s was the warmest decade and 1998 the warmest year" – has since been referenced widely and has become a prominent feature of the public debate surrounding climate change policy.

However, in recent peer-reviewed articles in *Science*, *Geophysical Research Letters*, and *Energy & Environment*, researchers question the results of this work. As these researchers find, based on the available information, the conclusions concerning temperature histories – and hence whether warming in the 20th century is actually unprecedented – cannot be supported by the Mann *et al.* studies cited in the TAR. In addition, we understand from the February 14 *Journal* and these other reports that researchers have failed to replicate the findings of these studies, in part because of problems with the underlying data and the calculations used to reach the conclusions. Questions have also been raised concerning the sharing and dissemination of the data and methods used to perform the studies. For example, according to the January 2005 *Energy & Environment*, such information necessary to replicate the analyses in the studies has not been made fully available to researchers upon request.

The concerns surrounding these studies reflect upon the quality and transparency of federally funded research and of the IPCC review process – two matters of particular interest to the Committee. For example, one concern relates to whether IPCC review has been sufficiently independent. We understand that you were a lead author of the IPCC chapter that assessed and reported your own studies, and that two study co-authors were also contributing authors to this very same chapter. Given the prominence these studies were accorded in the IPCC TAR and your position and role in that process, we seek to learn more about the facts and circumstances that led to acceptance and prominent use of this work in the IPCC TAR and to understand what this controversy indicates about the data quality of key IPCC studies.

As you know, sharing data and research results is a basic tenet of open scientific inquiry, providing a means to judge the reliability of scientific claims. The ability to replicate a study, as the National Research Council has noted, is typically the gold standard by which the reliability of claims is judged. Given the questions reported about data access surrounding these studies, we also seek to learn whether obligations concerning the sharing of information developed or disseminated with federal support have been appropriately met.

In light of the Committee's jurisdiction over energy policy and certain environmental issues, the Committee must have full and accurate information when considering matters relating to climate change policy. We open this review because this dispute surrounding your studies bears directly on important questions about the federally funded work upon which climate studies rely and the quality and transparency of analyses used to support the IPCC assessment process. With the IPCC currently working to produce a fourth assessment report, addressing questions of quality and transparency in the process and underlying analyses supporting that assessment, both scientific and economic, are of utmost importance if Congress is eventually going to make policy decisions drawing from this work.

To assist us as we begin this review, and pursuant to Rules X and XI of the U.S. House of Representatives, please provide the following information requested below on or before July 11, 2005:

1. Your *curriculum vitae*, including, but not limited to, a list of all studies relating to climate change research for which you were an author or co-author and the source of funding for those studies.

2. List all financial support you have received related to your research, including, but not limited to, all private, state, and federal assistance, grants, contracts (including subgrants or subcontracts), or other financial awards or honoraria.

3. Regarding all such work involving federal grants or funding support under which you were a recipient of funding or principal investigator, provide all agreements relating to those underlying grants or funding, including, but not limited to, any provisions, adjustments, or exceptions made in the agreements relating to the dissemination and sharing of research results.

4. Provide the location of all data archives relating to each published study for which you were an author or co-author and indicate: (a) whether this information contains all the specific data you used and calculations your performed, including such supporting documentation as computer source code, validation information, and other ancillary

information, necessary for full evaluation and application of the data, particularly for another party to replicate your research results; (b) when this information was available to researchers; (c) where and when you first identified the location of this information; (d) what modifications, if any, you have made to this information since publication of the respective study; and (e) if necessary information is not fully available, provide a detailed narrative description of the steps somebody must take to acquire the necessary information to replicate your study results or assess the quality of the proxy data you used.

5. According to *The Wall Street Journal*, you have declined to release the exact computer code you used to generate your results. (a) Is this correct? (b) What policy on sharing research and methods do you follow? (c) What is the source of that policy? (d) Provide this exact computer code used to generate your results.

6. Regarding study data and related information that is not publicly archived, what requests have you or your co-authors received for data relating to the climate change studies, what was your response, and why?

7. The authors McIntyre and McKitrick (*Energy & Environment*, Vol. 16, No. 1, 2005) report a number of errors and omissions in Mann *et. al.*, 1998. Provide a detailed narrative explanation of these alleged errors and how these may affect the underlying conclusions of the work, including, but not limited to answers to the following questions:
    a. Did you run calculations without the bristlecone pine series referenced in the article and, if so, what was the result?
    b. Did you or your co-authors calculate temperature reconstructions using the referenced "archived Gaspe tree ring data," and what were the results?
    c. Did you calculate the R2 statistic for the temperature reconstruction, particularly for the 15th Century proxy record calculations and what were the results?
    d. What validation statistics did you calculate for the reconstruction prior to 1820, and what were the results?
    e. How did you choose particular proxies and proxy series?

8. Explain in detail your work for and on behalf of the Intergovernmental Panel on Climate Change, including, but not limited to: (a) your role in the Third Assessment Report; (b) the process for review of studies and other information, including the dates of key meetings, upon which you worked during the TAR writing and review process; (c) the steps taken by you, reviewers, and lead authors to ensure the data underlying the studies forming the basis for key findings of the report were sound and accurate; (d) requests you received for revisions to your written contribution; and (e) the identity of the people who wrote and reviewed the historical temperature-record portions of the report, particularly Section 2.3, "Is the Recent Warming Unusual?"

Thank you for your assistance. If you have any questions, please contact Peter Spencer of the Majority Committee staff at (202) 226-2424.

Sincerely,

Joe Barton
Joe Barton
Chairman

Ed Whitfield
Ed Whitfield
Chairman
Subcommittee on Oversight
and Investigations

cc: The Honorable John Dingell, Ranking Member
The Honorable Bart Stupak, Ranking Member,
Subcommittee on Oversight and Investigations

July 15, 2005

Via Federal Express

Joe Barton, Chairman
House Committee on Energy and Commerce
Ed Whitfield, Chairman
Subcommittee on Oversight and Investigations
2125 Rayburn House Office Building
Washington, D.C. 20515

Dear Chairman Barton and Chairman Whitfield:

This letter responds to your letter of June 23, 2005, which seeks information on issues relating to my research on the historical record of temperatures and climate change.[1] Your letter lays out a number of "concerns" about the research my colleagues and I have conducted about global warming. Your letter also inquires about the role I played in the preparation of the United Nation's Intergovernmental Panel on Climate Change Third Assessment Report (the so-called "TAR").

I will address each of your questions in turn. Before doing so, however, let me state that my research findings, which support the conclusion that the earth's surface is warming, and that recent warming is due in large part to human influences, are consistent with the overwhelming scientific consensus on climate change. My research has been subject to intensive peer-review. Other scientists have replicated all facets of my research and have found it accurate and reliable. The specific conclusion published by my colleagues and me that late 20th century Northern Hemisphere warmth is anomalous in the context of at least the past millennium is common to many studies. Based on multiple supporting studies, the TAR came to a similar conclusion. The TAR did not rely solely on the work of my colleagues and me in reaching this conclusion. Recent work since the TAR has provided further support for this conclusion, which is now common to more than a dozen independent studies published in the peer-reviewed scientific literature. (I have provided for reference a comprehensive review by Jones and Mann in the journal "Reviews of Geophysics" of the American Geophysical Union (AGU).) The criticisms your letter cites have been soundly rejected by the scientific community.

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[1] This response is submitted without waiving any objection I might have to the Committee's jurisdiction over the subject matter of this inquiry.

The most serious contention in your letter — namely, that my work has not been subject to replication because I have failed to make available the underlying research data — is incorrect. Your letter notes that the National Research Council's "gold standard" for scientific research is the ability of other scientists to replicate first-generation research, and I fully agree. My colleagues and I follow the National Research Council's guidance with regards to the disclosure of research data, and all of our data and methodologies have been fully disclosed and are available to anyone with a computer and an internet connection. As a result of our willingness to share our research with others, an independent team of scientists has used the research data my colleagues and I have made public to replicate our research and confirm the reliability of our findings. *See* Wahl, E.R., Ammann, C.M., *Robustness of the Mann, Bradley, Hughes Reconstruction of Surface Temperatures: Examination of Criticisms Based on the Nature and Processing of Proxy Climate Evidence,* Climate Change (2005) (forthcoming) and associated website: http://www.cgd.edu/ccr/ammann/millennium/MBH_reevaluation.html.

Let me now turn to your specific questions, which ask that I provide the following information:

Q1: Your letter first asks that I furnish the Committee my *curriculum vitae,* along with a "list of all studies relating to climate change research for which you were an author or co-author and the source of funding for those studies."

A: This material is attached.

Q2: Your letter next asks that I "[l]ist all financial support" I have received to support my research.

A: See attachment.

Q3: Your letter requests that I provide, for all "work involving federal grants or funding support under which you were a recipient of funding or a principal investigator," "all agreements relating to those underlying grants or funding, including, but not limited to, any provisions, adjustments, or exceptions made in the agreement relating to the sharing of research results."

A: These requests are not directed to the appropriate person. The committee should contact the University of Massachusetts and University of Virginia offices of grant administration for these materials. With respect to the UMass NSF research funds (which supported the 1998 *Nature* article), it should furthermore be noted that I was *not* the Principal Investigator for this NSF project, and I am not, nor have I ever, been in possession of any official paperwork related to this grant.

Q4: Your next question asks for "the location of all data archives relating to each published study for which" I was "an author or co-author" and whether such data would be sufficient to permit other researchers to replicate the work.

A: The data, descriptions of methods, and results related to my research — more than sufficient to permit other researchers to replicate the research — have been extensively archived (in many cases, in several archives) on public websites, and data links within the websites. The website addresses appear in the margin.[2]

Q5: This question begins by stating that, "[a]ccording to *The Wall Street Journal*, you have declined to release the exact computer code you used to generate your results." The question then poses a series of questions: "(a) Is that correct? (b) What policy on sharing research and methods do you follow? (c) What is the source of that policy? (d) Provide this exact computer code used to generate your results."

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[2] http://fox.rwu.edu/~rutherfo/supplements/jclim2003a/
http://www.ngdc.noaa.gov/paleo/pubs/jones2004/jones2004.html
http://www.ngdc.noaa.gov/paleo/pubs/mann2003b/mann2003b.html
http://www.ngdc.noaa.gov/paleo/pubs/mann2003/mann2003.html
http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html
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http://www.ncdc.noaa.gov/paleo/ei/ei_reconsc.html
http://www.ncdc.noaa.gov/cgi-bin/paleo/mannplot2.pl
http://www.ncdc.noaa.gov/paleo/ei/data_supp.html
http://www.ncdc.noaa.gov/paleo/ei/stats-supp-annual.html
http://www.ncdc.noaa.gov/paleo/ei/stats-supp-cold.html
http://www.ncdc.noaa.gov/paleo/ei/stats-supp-warm.html
http://www.ncdc.noaa.gov/paleo/ei/ei_nodendro.html
ftp://ftp.ngdc.noaa.gov/paleo/paleolimnology/newengland/glacial_lake_hitchcock/
ftp://eclogite.geo.umass.edu/pub/mann/MANNETAL98/nino3.dat
ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/Millennium/DATA/RECONS/
ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/
http://www.ngdc.noaa.gov/paleo/pubs/mann1998/frames.htm
ftp://eclogite.geo.umass.edu/pub/mann/MANNETAL98/
ftp://holocene.evsc.virginia.edu/pub/MBH98/
ftp://holocene.evsc.virginia.edu/pub/MANNETAL98/
ftp://holocene.evsc.virginia.edu/pub/MANNETAL98/FIGUREDATA/
ftp://holocene.evsc.virginia.edu/pub/MANNETAL98/INSTRUMENTAL/
ftp://holocene.evsc.virginia.edu/pub/MANNETAL98/METHODS/
ftp://holocene.evsc.virginia.edu/pub/MANNETAL98/PROXY/
ftp://holocene.evsc.virginia.edu/pub/mann/Filter/lowpass.m
ftp://holocene.evsc.virginia.edu/pub/mann/Filter/lowpassmin.m
http://www.atmos.ucla.edu/tcd/ssa/
http://holocene.evsc.virginia.edu/Mann/tools/MTM-RED
http://holocene.evsc.virginia.edu/Mann/tools/MTM-COHERE
http://holocene.evsc.virginia.edu/Mann/tools/CMPLXDEMOD
http://holocene.evsc.virginia.edu/Mann/tools/MTM-SVD

The question presumes that in order to replicate scientific research, a second researcher has to have access to exactly the same computer program (or "code") as the initial researcher. This premise is false. The key to replicability is unfettered access to all of the underlying data and methodologies used by the first researcher. My data and methodological information, and that of my colleagues, are available to anyone who wants them.[3] As noted above, other scientists have reproduced our results based on publicly available information.

It also bears emphasis that my computer program is a private piece of intellectual property, as the National Science Foundation and its lawyers recognized. The National Science Foundation — the government agency that establishes policy in this area — has confirmed that my colleagues and I have met every requirement of transparency and openness in our research. My research is all based on data sets regarding the Earth's climate that are freely and widely available to *all* researchers. Whether I make available my computer programs is irrelevant to whether our results can be reproduced. And whether I make my computer programs publicly available or not is a decision that is mine alone to make. Since other scientists have used the methods we described and the data we archived to replicate our results, the issue of whether my computer program is available has no bearing whatsoever on the veracity of our results. The question you posed — whether I have fully satisfied established scientific standards for data-sharing — has been fully considered by the National Science Foundation. As your letter notes, two Canadian researchers, Steve McIntyre and Ross McKitrick, contacted NSF to inquire whether I had complied with National Science Foundation requirements. The National Science Foundation twice informed them that I have, in fact, complied with all applicable transparency and openness standards and that, under long-standing Foundation policy, the computer codes referred to by *The Wall Street Journal* are considered the intellectual property of researchers and are not subject to disclosure.[4]

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[3] All of the proxy data (tree-rings, coral, ice cores, and historical documents) used in Mann *et al.* (1998) has been available since May 2000 on this public website: ftp://holocene.evsc.virginia.edu/pub/MBH98. The methodology used by my colleagues and me is described in detail in the initial publication, and further expanded upon in July 2004 on *Nature*'s supplementary website (http://www.nature.com/nature/journal/v430/n6995/suppinfo/nature02478.html) and on our own website, ftp://holocene.evsc.virginia.edu/pub/MANNETAL98. Moreover, independently-derived source codes for implementing our algorithm, and all required input data, have been posted on the website of the National Center for Atmospheric Research. *See* http://www.ucar.edu/ccr/ammann/millennium/CODES_MBH.html. For these reasons, charges that our work is not subject to replication are unfounded. The initial description of the work was sufficient to permit researchers to independently produce the key algorithms. *See, e.g.*, Zorita, E., F. Gonzalez-Rouco, and S. Legutke, *Testing the Mann et al.(1998) approach to paleoclimate reconstructions in the context of a 1000-yr control simulation with the ECHO-G Coupled Climate Model*, J. Climate, 16, 1378-1390 (2003); Von Storch, H., E. Zorita, J.M. Jones, Y. Dimitriev, F. Gonzalez-Rouco, F., and S.F.B. Tett, *Reconstructing Past Climate from Noisy Data*, Science, 306, 679-682 (2004). Not only have we replicated our results with a different methodology (Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., *Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain*, Journal of Climate (2005) (to appear in July issue), but an independent group has replicated our original methods and results (*See* Wahl, E.R. and Ammann, C.M., *Robustness of the Mann, Bradley, Hughes Reconstruction of Surface Temperatures: Examination of Criticisms Based on the Nature and Processing of Proxy Climate Evidence*, Climatic Change (2005) (forthcoming)).

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[4] For the sake of completeness, let me quote in its entirety the email message sent by Dr. David J. Verardo, Director, Paleoclimate Program, Division of Atmospheric Sciences, National Science Foundation to Mr. Steve McIntyre (copied to me), on December 17, 2003, in response to a previous email that McIntyre had sent to Dr. Verardo (copied to me):

Dear Mr. McIntyre,

I apologize if my last electronic message was not clear but let me clarify the US NSF's view in this current message. Dr. Mann and his other US colleagues are under no obligation to provide you with any additional data beyond the extensive data sets they have already made available. He is not required to provide you with computer programs, codes, etc. His research is published in the peer-reviewed literature which has passed muster with the editors of those journals and other scientists who have reviewed his manuscripts. You are free to your analysis of climate data and he is free to his. The passing of time and evolving new knowledge about Earth's climate will eventually tell the full story of changing climate. I would expect that you would respect the views of the US NSF on the issue of data access and intellectual property for US investigators as articulated by me to you in my last message under the advisement of the US NSF's Office of General Counsel.

Respectfully,
David J. Verardo
Director, Paleoclimate Program
Division of Atmospheric Sciences
National Science Foundation
4201 Wilson Blvd.
Arlington, VA 22203

Even more recently, the National Science Foundation confirmed its view that my computer codes are my intellectual property. A recent issue of the *Chronicle of Higher Education* states: "According to David Stonner, of the Congressional-

With this background in mind, let me now respond to your specific inquiries:

A (Q5A): I have made available all of the research data that I am required to under United States policy as set by the National Science Foundation. In accordance with the rules promulgated by the Foundation and supported by the Foundation's General Counsel, I maintain the right to decline to release any computer codes, which are my intellectual property.

A (Q5B): The policy regarding sharing research and methods I and my colleagues follow is to disclose any information that might be useful to other researchers, including the data, description of methodology, and so forth, that would enable a competent scientist to replicate our work. The proof here, of course, is that other scientists have in fact succeeded in replicating our work. And, as noted above, our policies are fully in keeping with those established by the National Science Foundation.

A(Q5C): The source of these policies is the National Science Foundation.

A(Q5D): My computer program is a piece of private, intellectual property, as the National Science Foundation and its lawyers recognize. It is a bedrock principle of American law that the government may not take private property "without [a] public use," and "without just compensation."

That notwithstanding, the program used to generate the original Mann *et al.* 1998 temperature reconstructions is posted at this website:
ftp://holocene.evsc.virginia.edu/pub/MANNETAL98/
(see "METHODS" subdirectory)

Q6: The Committee next asks that, "[r]egarding study data and related information that is not publicly archived, what requests have you and your co-authors received for data relating to climate change studies, what was your response, and why?"

A: I can of course only speak for myself, but I do not believe that there is any "study data" used in my published work that is not publicly archived. Having said that, I do respond diligently to any requests from scientific colleagues for data or methodological details relating to our research.

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affairs office at the National Science Foundation, Mr. McIntyre contacted the foundation last year to ask for Mr. Mann's computer code. Mr. Stonner said the agency had told Mr. McIntyre that the code was the intellectual property of Mr. Mann . . .." Richard Monastersky, *Congressman Demands Complete Records on Climate Research by 3 Scientists Who Support of Global Warming,* Chronicle of Higher Education (July 1, 2005), available at: http://chronicle.com/temp/email.php?id=dopjw74bwvqzvd3k9tekp5avlofvb2yu.

Q7: This question poses a number of questions based on an article published by McIntyre and McKitrick in *Energy & Environment.* The question states that these authors "report a number of errors and omissions in Mann *et al.* 1998 and how these may affect the underlying conclusions of the work." The question goes on to list a number of topics that I should address in a "narrative explanation."

A: I want to begin by emphasizing that nothing in McIntyre and McKitrick's article undermines the conclusion of my research. My colleagues and I stand foursquare behind our work. So does the scientific community.

The various claims of McIntyre and McKitrick — including the ones repeated in your question — have been exhaustively examined by two different groups of climate researchers who have found their objections to be unfounded.[5] *See also* National Center for Atmospheric Research, *Media Advisory: The Hockey Stick Controversy New Analysis Reproduces Graph of Late 20th Century Temperature Rise* (May 11, 2005) (available at: http://www.ucar.edu/news/releases/2005/ammann.shtml). Moreover, it is my understanding that several other groups of climate researchers have examined McIntyre and McKitrick's criticisms and also have found their criticisms lacking in merit. On the other hand, I know of no independent scientific group that has found any of McIntyre and McKitrick's claims to be valid.

Nor is that surprising. *Energy & Environment* is *not* a peer reviewed scientific journal; it is a journal primarily devoted to *policy* rather than science that appears to engage in, at most, haphazard review of its articles. And neither McIntyre nor McKitrick is a trained climate scientist. According to the biographical data on their websites, Mr. McIntyre is a mining industry executive with no formal training in any discipline related to climate research and Mr. McKitrick is an economist with no scientific training, hardly credentials that lend force to their academic arguments. *See* http://www.uoguelph.ca/~rmckitri/cv.html and http://www.uoguelph.ca/~rmckitri/research/stevebio.doc.

Adding to the problem, the editor of *Energy & Environment,* Ms. Sonja Boehmer-Christiansen, has candidly acknowledged that the publication has a clear editorial bias. In the September 5, 2003 issue of the *Chronicle of Higher Education,* Ms. Boehmer-Christiansen is quoted as describing the editorial policy of *Energy & Environment* in this way: "I'm following my political agenda – a bit, anyway. *** But isn't that the right of an editor?" As to "peer review," Ms. Boehmer-Christiansen has acknowledged in an email to Dr. Tim Osborn of the Climatic Research Unit at the University of East Anglia (U.K.), that in her rush to get the McIntyre and McKitrick piece into print for political reasons *Energy & Environment* dispensed with what scientists consider peer review ("I was rushing you to get this paper out for policy impact reasons, e.g. publication well before COP9"). As Ms. Boehmer-Christiansen added, the "paper was amended until the very last moment. There was a trade off in favour of policy." McIntyre and McKitrick's work has been discredited by ample peer-reviewed, scientific work.

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[5] *See, e.g.*, Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., *Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain*, Journal of Climate (2005) (in press, to appear in July issue); Wahl, E.R. and Ammann, C.M., *Robustness of the Mann, Bradley, Hughes Reconstruction of Surface Temperatures: Examination of Criticisms Based on the Nature and Processing of Proxy Climate Evidence*, Climatic Change (2005) (forthcoming).

Nonetheless, let me try to respond to the Committee's specific questions.

A(7A,7B): The Committee inquires about the sensitivity of the results of the Mann *et al.* 1998 study to the inclusion or omission of certain North American tree-ring data ("the bristlecone pine series" and "archived Gaspe tree ring data" referred to in the Committee's letter). For a complete scientific response, you should consult the article my co-authors and I published back in 1999 addressing precisely these issues: Mann, M.E., Bradley, R.S., and Hughes, M.K., *Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations*, Geophysical Research Letters, 26, 759-62 (1999).

The issues raised by the Committee involve a 100 year sub-interval of our reconstruction from AD 1400-1500. As my co-authors and I explained in our 1999 article cited above, given the proxy data available at that time, certain key tree-ring data (including the series mentioned above) were essential, if the reconstructed temperature record during early centuries were to have any climatologic "skill" (that is, any validity or meaningfulness). These conclusions were of course reached through analyses in which these key datasets were excluded, and the results tested for statistical validity. Our conclusions have been confirmed by Wahl and Ammann (see above). These researchers have demonstrated that the reconstructions produced by McIntyre and McKitrick result from ignoring these key data, and fail the accepted, basic tests for statistical validity. Moreover, Wahl and Ammann demonstrate that the climatologically improbable results obtained by McIntyre and McKitrick, which would suggest that the Northern Hemisphere was unusually warm during the 15th century (the middle of the so-called "Little Ice Age"), are statistically meaningless, and an artifact of both their exclusion of key proxy data (as discussed above) and the use of a flawed implementation of the Mann *et al.* 1998 method. *See* http://www.cgd.ucar.edu/ccr/ammann/millennium/CODES_MBH.html) (chart at the bottom of the page).

Since 1999 new proxy data have become available and new methodologies developed for using them. Studies using these data and methodologies have confirmed the primary conclusion of our work (e.g. Mann *et al.* 1998 and Mann *et al.* 1999) that the most recent decades were likely the warmest of the past 1,000 years for the Northern Hemisphere on the average. The most recent such study (published in *Nature*) in fact extends this conclusion to at least the past 2,000 years. Moberg, A., D.M. Sonechkin, K. Holmgren, N.M. Datsenko, and W. Karlen, *Highly Variable Northern Hemisphere*

Temperatures Reconstructed from Low- and High-resolution Proxy Data, Nature, 433, 613-617 (2005).

A(7C): The Committee inquires about the calculation of the R2 statistic for temperature reconstruction, especially for the 15th Century proxy calculations. In order to answer this question it is important to clarify that I assume that what is meant by the "R2" statistic is the squared Pearson dot-moment correlation, or r^2 (i.e., the square of the simple linear correlation coefficient between two time series) over the 1856-1901 "verification" interval for our reconstruction. My colleagues and I did not rely on this statistic in our assessments of "skill" (i.e., the reliability of a statistical model, based on the ability of a statistical model to match data not used in constructing the model) because, in our view, and in the view of other reputable scientists in the field, it is not an adequate measure of "skill." The statistic used by Mann *et al.* 1998, the reduction of error, or "*RE*" statistic, is generally favored by scientists in the field. *See, e.g.*, Luterbacher, J.D., *et al.*, *European Seasonal and Annual Temperature Variability, Trends and Extremes Since 1500*, Science 303, 1499-1503 (2004).

RE is the preferred measure of statistical skill because it takes into account not only whether a reconstruction is "correlated" with the actual test data, but also whether it can closely reproduce the mean and standard deviation of the test data. If a reconstruction cannot do that, it cannot be considered statistically valid (i.e., useful or meaningful). The linear correlation coefficient (*r*) is not a sufficient diagnostic of skill, precisely because it cannot measure the ability of a reconstruction to capture changes that occur in either the standard deviation or mean of the series outside the calibration interval. This is well known. *See* Wilks, D.S., STATISTICAL METHODS IN ATMOSPHERIC SCIENCE, chap. 7 (Academic Press 1995); Cook, *et al.*, *Spatial Regression Methods in Dendroclimatology: A Review and Comparison of Two Techniques,* International Journal of Climatology, 14, 379-402 (1994). The highest possible attainable value of r^2 (i.e., $r^2 = 1$) may result even from a reconstruction that has no statistical skill at all. *See, e.g.,* Rutherford, *et al.*, *Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain*, Journal of Climate (2005) (in press, to appear in July issue)(available at: ftp://holocene.evsc.virginia.edu/pub/mann/RuthetalJClimate-inpress05.pdf). For all of these reasons, we, and other researchers in our field, employ *RE* and not r^2 as the primary measure of reconstructive skill.

As noted above, in contrast to the work of Mann *et al.* 1998, the results of the McIntyre and McKitrick analyses fail verification tests using the accepted metric *RE*. This is a key finding of the Wahl and Ammann study cited above. This means that the reconstructions McIntyre and McKitrick produced are statistically inferior to the simplest possible statistical reconstruction: one that simply assigns the mean over the calibration period to all previous reconstructed values. It is for these reasons that Wahl and Ammann have concluded that McIntyre and McKitrick's results are "without statistical and climatological merit."

A(7D): The Committee asks "[w]hat validation statistics did you calculate for the reconstruction prior to 1820, and what were the results?" Our validation statistics were described in detail in a table provided in the supplementary information on *Nature*'s website accompanying our original nature article, Mann, M.E., Bradley, R.S., Hughes, M.K., *Global-Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries*, Nature, 392, 779-787 (1998). These statistics remain on *Nature*'s website (*see* http://www.nature.com/nature/journal/v392/n6678/suppinfo/392779a0.html) and on our own website. *See* ftp:holocene.evsc.virginia.edu/pub/Manneta198.

A(7E): The Committee asks how I "choose particular proxies and proxy series." Again, this information is furnished in detail in both our original 1998 article in *Nature*, and expanded upon in a follow-up article published in 2000. *See* Mann *et al.*, *Global Temperature Patterns in Past Centuries: An Interactive Presentation,* Earth Interactions 4-4, 1-29 (2000), specifically this link therein: http://www.ncdc.noaa.gov/paleo/ei/ei_nodendro.htlm.

As our 1998 study and the additional information mentioned above make clear, we made use of all long-term, annually-resolved proxy indicators available to us in the public domain or through colleagues at the time the research was initiated (1996-1997) that met requirements for suitable length, age model reliability, and in the case of tree ring series, replication, inter-correlation and metadata as described above.

Q8: This question asks me to "[e]xplain in detail" my work "for and on behalf of the Intergovernmental Panel on Climate Change," including my "role in the Third Assessment Report" (referred to as "TAR"), and a host of information as to how TAR was prepared and how the authors of TAR verified the soundness of the data that formed the basis for the conclusions set forth in TAR.

A: As is set forth on my *curriculum vitae,* I was one of ten lead authors of chapter 2 of TAR, and I served as a contributing author for chapters 7, 8, and 12 of the report. Given the breadth of the project, there were two layers of editorial review that oversaw the work of the lead authors for each chapter, so the chapter reflected a consensus scientific view, not merely the views of any single author. The TAR had 672 scientist reviewers. In the United States, anyone who wanted to review the drafts was allowed access to them to provide a review. I am not myself familiar with any scientific document that has been more comprehensively reviewed than the TAR.

Information concerning the "dates of key meetings," the steps taken by "reviewers, and lead authors to ensure the data . . . were sound and accurate," and the "identity of people who wrote and reviewed" portions of TAR should be obtained directly from the Intergovernmental Panel on Climate Change ("IPCC"). As I am sure you can appreciate, I am not an agent of the IPCC and I am not empowered to speak on IPCC's behalf on these matters. Nor have I been authorized by the IPCC to make public information that the IPCC itself has not chosen to make publicly available. If the Committee is interested in pursuing these matters, I would urge that the Committee contact Sir John Houghton, the head of the Working Group, at the Hadley Centre in England.

For the Committee's convenience, I have sent along with this letter copies of key scientific articles referred to in this letter. Please let me know if you have questions.

Respectfully submitted,

Michael Mann

Michael E. Mann, Ph.D.
Associate Professor and
Director of Earth System Science Center
Department of Meteorology
The Pennsylvania State University[6]

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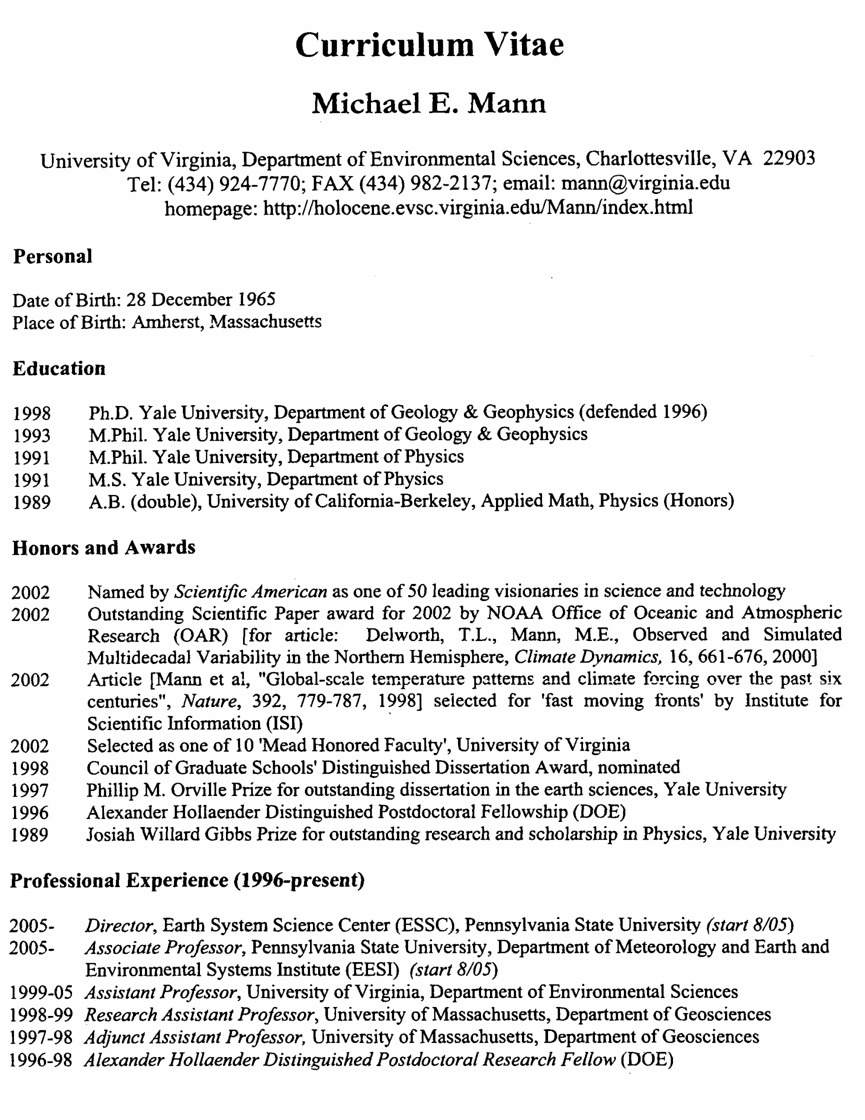
[6] I do not formally assume this position until August 22, 2005. I currently serve as Assistant Professor, Department of Environmental Sciences, University of Virginia, Charlottesville.



Curriculum Vitae

Michael E. Mann

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Personal

Date of Birth: 28 December 1965
Place of Birth: Amherst, Massachusetts

Education

1998	Ph.D. Yale University, Department of Geology & Geophysics (defended 1996)
1993	M.Phil. Yale University, Department of Geology & Geophysics
1991	M.Phil. Yale University, Department of Physics
1991	M.S. Yale University, Department of Physics
1989	A.B. (double), University of California-Berkeley, Applied Math, Physics (Honors)

Honors and Awards

2002	Named by *Scientific American* as one of 50 leading visionaries in science and technology
2002	Outstanding Scientific Paper award for 2002 by NOAA Office of Oceanic and Atmospheric Research (OAR) [for article: Delworth, T.L., Mann, M.E., Observed and Simulated Multidecadal Variability in the Northern Hemisphere, *Climate Dynamics,* 16, 661-676, 2000]
2002	Article [Mann et al, "Global-scale temperature patterns and climate forcing over the past six centuries", *Nature*, 392, 779-787, 1998] selected for 'fast moving fronts' by Institute for Scientific Information (ISI)
2002	Selected as one of 10 'Mead Honored Faculty', University of Virginia
1998	Council of Graduate Schools' Distinguished Dissertation Award, nominated
1997	Phillip M. Orville Prize for outstanding dissertation in the earth sciences, Yale University
1996	Alexander Hollaender Distinguished Postdoctoral Fellowship (DOE)
1989	Josiah Willard Gibbs Prize for outstanding research and scholarship in Physics, Yale University

Professional Experience (1996-present)

2005-	*Director*, Earth System Science Center (ESSC), Pennsylvania State University *(start 8/05)*
2005-	*Associate Professor*, Pennsylvania State University, Department of Meteorology and Earth and Environmental Systems Institute (EESI) *(start 8/05)*
1999-05	*Assistant Professor*, University of Virginia, Department of Environmental Sciences
1998-99	*Research Assistant Professor*, University of Massachusetts, Department of Geosciences
1997-98	*Adjunct Assistant Professor,* University of Massachusetts, Department of Geosciences
1996-98	*Alexander Hollaender Distinguished Postdoctoral Research Fellow* (DOE)

Courses Taught

University of Virginia:	
EVSC 181	*Climate Change: Past and Future*
EVSC 350	*Atmosphere and Weather*
EVSC 495	*Capstone Seminar: The Arctic*
EVCS 494/495/EVAT795	*Data Analysis & Climate Change*
EVSC 494/EVAT 796	*Climate and the History of Human Culture*
EVAT 554	*Ocean-Atmosphere Dynamics*
EVAT 793	*Statistical Climatology*
EVAT 795	*Modeling of Climate Variability*

University of Massachusetts:	
GEO 591	*Data Analysis & Climate Change*

Advising

Postdoctoral Researchers:
Scott Rutherford, Postdoctoral Research Associate, University of Virginia, 2000-2003
Feinbao Ni (joint w/ M. Hughes), Postdoctoral Research Associate, University of Arizona, 2002-03
Pedro Ribera, (Visiting postdoctoral research fellow, University of Vigo, Spain), 2000.
Mei-Yu (joint w/ H. Epstein), Postdoctoral Research Associate, University of Virginia, 2002-03
Graduate Students:
Julian Adams, M.S. 2003: major adviser, University of Virginia, 2000-2003
Caspar Ammann, Ph.D. 2000: co-adviser, University of Massachusetts, 1998-2000
Kaycie Billmark, Ph.D. 2003: committee member, University of Virginia, 1999-2003
Joel Carr, M.S. 2002: masters thesis committee, University of Virginia, 2000-2002
Peter Cleary, M.S. 2004: co-adviser, University of Virginia, 2002-2004
Benjamin Cook, M.S. 2004; Ph.D. candidate: co-adviser, University of Virginia, 2001-
Heidi Cullen, Ph.D. 2001: external committee member, Columbia University, 2000-01.
Dan Druckenbrod, Ph.D. 2003. masters & Ph.D thesis committee, University of Virginia, 1999-2003
Ian German-Mesner, M.S. candidate: major adviser, University of Virginia, 2002-
Kevin Jones, M.S. 2002: masters thesis committee, University of Virginia, 2001-2002
Courtney Strong, Ph.D. candidate: committee member, University of Virginia, 2003-
Bo-Min Sun, Ph.D. committee member, University of Massachusetts, 1998-99.
Silvia Venegas, Ph.D. 2001: outside committee member, 2000-01.
Anne Waple, Ph.D. candidate: co-adviser and committee member, University of Massachusetts, 1997-
Tana Wood, M.S. 2002: masters thesis committee, University of Virginia, 2001-2002
Zhihua Zhang, Ph.D. candidate: major adviser, University of Virginia, 2000-
Undergraduates:
Michelle L'Heureux, supervised research; Distinguished Majors Project, University of Virginia, 2001-02.

Primary Research Interests

(1) Climate signal detection and climate change attribution; (2) Statistical and time series analysis methods; (3) High-resolution paleoclimate reconstruction; (4) Study of forced and internal variability in coupled ocean-atmosphere models; and model/data intercomparison; (5) Simplified coupled ocean-atmosphere modeling; (6) Use of climate scenarios to drive process-oriented models of geophysical phenomena

Funded Proposals

2003-2006	*Decadal Variability in the Tropical Indo-Pacific: Integrating Paleo & Coupled Model Results*, NOAA-Climate Change Data & Detection (CCDD) Program [Principal Investigators: M.E. Mann (U.Va), J. Cole (U. Arizona), V. Mehta (CRCES)]
2002-2005	*Reconstruction and Analysis of Patterns of Climate Variability Over the Last One to Two Millennia,* NOAA-Climate Change Data & Detection (CCDD) Program [Principal Investigator: M.E. Mann, Co-Investigators: S. Rutherford, R.S. Bradley, M.K. Hughes]
2002-2005	*Remote Observations of Ice Sheet Surface Temperature: Toward Multi-Proxy Reconstruction of Antarctic Climate Variability*, NSF-Office of Polar Programs, Antarctic Oceans and Climate System [Principal Investigators: M.E. Mann (U. Va), E. Steig (U. Wash.), D. Weinbrenner (U. Wash)]
2002-2003	*Paleoclimatic Reconstructions of the Arctic Oscillation,* NOAA-Cooperative Institute for Arctic Research (CIFAR) Program [Principal Investigators: Rosanne D'Arrigo, Ed Cook (Lamont/Columbia); Co-Investigator: M.E. Mann]
2002-2003	*Global Multidecadal-to-Century-Scale Oscillations During the Last 1000 years,* NOAA-Climate Change Data & Detection (CCDD) Program [Principal Investigator: Malcolm Hughes (Univ. of Arizona); Co-Investigators: M.E. Mann; J. Park (Yale University)]
2001-2003	*Resolving the Scale-wise Sensitivities in the Dynamical Coupling Between Climate and the Biosphere,* University of Virginia-Fund for Excellence in Science and Technology (FEST) [Principal Investigator: J.D. Albertson; Co-Investigators: H. Epstein, M.E. Mann]
2001-2002	*Advancing predictive models of marine sediment transport,* Office of Naval Research [Principal Investigator: P. Wiberg, Co-Investigator: M.E. Mann]
1999-2002	*Multiproxy Climate Reconstruction: Extension in Space and Time, and Model/Data Intercomparison*, NOAA-Earth Systems History [Principal Investigator: M.E. Mann, Co-Investigators: R.S. Bradley, M.K. Hughes]
1998-2000	*Validation of Decadal-to-Multi-century climate predictions,* DOE [Principal Investigator: R.S. Bradley; Co-Investigators: H.F. Diaz, M.E. Mann]
1998-2000	*The changing seasons? Detecting and understanding climatic change*, NSF-Hydrological Science [Principal Investigators: U. Lall; Co-investigators: M.E. Mann, B. Rajagopalan, M. Cane]
1996-1999	*Patterns of Organized Climatic Variability: Spatio-Temporal Analysis of Globally Distributed Climate Proxy Records and Long-term Model Integrations*, NSF-Earth Systems History [Principal Investigator: R.S. Bradley; Co-Investigators: M.E. Mann, M.K. Hughes]
1996-1998	*Investigation of Patterns of Organized Large-Scale Climatic Variability During the Last Millennium*, DOE, Alexander Hollaender Postdoctoral Fellowship [M.E. Mann]

Professional Activities

2005-	Editorial advisory board, *The Holocene*
2005	Co-convener/organizer (w/ J. Jouzel, P. Jones, W. Dullo), special session `` Climate of the last millennium", 2nd General Assembly, *European Geophysical Union*
2004	Co-convener/organizer (w/ J. Jouzel, P. Jones, W. Dullo), special session `` Climate of the past millennium", 1st General Assembly, *European Geophysical Union*
2003-04	Member, National Academy of Sciences/National Research Council *Committee on Radiative Forcing Effects on Climate*
2003	Provided testimony to U.S. Senate ('Environment and Public Works' Committee hearing , July 29)
2003	Chair of organizing committee, National Academy of Sciences *Frontiers of Science* symposium

2003	Co-convener/organizer (w/ P. Jones, V. Masson-Delmotte), IAMAS session on `` Climate of the Holocene", *IUGG*
2003	Co-convener/organizer (w/ J. Jouzel, P. Jones, W. Dullo), special session `` Climate of the past millennium", 28th General Assembly, *European Geophysical Society*
2003-	Advisory board member for *Scientific American* 'Scientific American 50'.
2003	Invited Participant, *Workshop on Estimating Climate Sensitivity*, National Academy of Sciences
2003-	Member of steering committee, NSF Marine Earth Systems History (MESH) Panel
2003-	Committee on Probability & Statistics, American Meteorological Society
2002-	IAMAS delegate for U.S./International Commission on Climate (ICCL)
2002	Organizing committee, National Academy of Sciences *Frontiers of Science* symposium
2002	Co-convener/organizer (w/ H. Von Storch, R. Brazdil), theme session ``Understanding the Late Maunder Minimum climate anomaly", Annual Spring meeting, *American Geophysical Union*
2002	Co-convener/organizer (w/ J. Jouzel, P. Jones, W. Dullo), special session `` Climate of the past millennium", 27th General Assembly, *European Geophysical Society*
2001-02	Member, advisory board, *Earth Interactions* (American Geophysical Union)
2001	Chair/Organizer, session on "Climate Change Detection/Attribution", National Academy of Science *Frontiers of Science* annual symposium, Irvine, California, Nov 8-10, 2001.
2001	Organizer and Host, *PAGES/CLIVAR workshop on Reconstructing Late Holocene Climate*, Charlottesville, Virginia, April 17-20, 2001.
2001	Co-convener/organizer (w/ J. Jouzel, P. Jones), special session `` Climate of the past millennium", 26th General Assembly, *European Geophysical Society*
2001	Co-convener/organizer, *NASA /IPRC/ CLIVAR workshop on Decadal Climate Variability*, Manoa, Hawaii, Jan 8-12, 2001.
2000-	Member of Working Group, *International PAGES/CLIVAR*
2000-	Panel member, *NOAA Climate Change Data and Detection Program*
2000-02	Editor, *Journal of Climate*
2000	Co-convener (w/ J. Jouzel, P. Jones), special session `` Climate of the past millennium", 25th General Assembly, *European Geophysical Society*
1999-00	Scientific adviser to U.S. Government (White House OSTP) on climate change
1999	Co-convener/organizer (w/ J. Jouzel), special session ``Data and model studies of climate changes over the last millennium", 24th General Assembly, *European Geophysical Society*
1999	Invited adviser, *NOAA Global Change and Climate Panel*, Boulder, CO
1999	Guest editor, special issue of *Climatic Change*
1999	Co-convener (w/ J. Overpeck), PAGES/CLIVAR workshop on multiproxy climate reconstruction, Boulder, CO
1998-00	Lead author, Chapter 2, *Intergovernmental Panel on Climate Change (IPCC)*, Third Assessment Report
1998-00	Contributing author, Chapters 7,8,12, *Intergovernmental Panel on Climate Change (IPCC)*, Third Assessment Report
1997	Co-convener/organizer (w/ E. Cook, H. Pollack, D. Chapman) , theme session ``Multiproxy Climate Reconstruction…", Annual Fall meeting, *American Geophysical Union*

- Memberships: *American Meteorological Society*; *American Geophysical Union*; *European Geophysical Society*; *Geological Society of America*; *American Physical Society; American Association for the Advancement of Science; Sigma Xi*
- Referee for: *Nature, Science, Climatic Change, Geophysical Research Letters, Journal of Climate, JGR-Oceans, JGR-Atmospheres, Paleoceanography, Climate Dynamics, Eos, Int. J. Climatol., Water Resources Research, Holocene, Atmospheric & Solar-Terrestrial Physics;GSA Today; Earth and Planetary Science Letters; Water Resources Research;Climate Research;* NSF, NOAA, DOE grant programs

Public Outreach:

- Popular Media: *CBS, NBC, ABC, CNN, CNN headline news, The Weather Channel, BBC, NPR, PBS, WCBS, Time, Newsweek, Life, US News & World Report, Economist, Scientific American, New Scientist, National Geographic, Discover, Science News, Science, Rolling Stone, Mother Jones, Popular Science, USA Today, New York Times, New York Times "Science Times", Washington Post, Wall Street Journal, Boston Globe, London Times, Irish Times, Le Monde, AP, UPI, Reuters, Scripps Howard*, and numerous other television/print media
- Co-founded climate information website, "RealClimate.org" (12/04)

Refereed Journal Articles:

- **Mann, M.E.**, Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, *Journal of Climate*, in press, 2005.
- Rutherford, S., **Mann, M.E.**, Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, *Journal of Climate*, in press, 2005.
- Cook, B.I., Smith, T.M., **Mann, M.E.**, The North Atlantic Oscillation and Regional Phenology Prediction over Europe, *Global Change Biology*, 11, 919-926, 2005.
- Frauenfeld, O.W., Davis, R.E., **Mann, M.E.**, A Distinctly Interdecadal Signal of Pacific Ocean–Atmosphere Interaction, *Journal of Climate*, 18, 1709-1718, 2005.
- **Mann, M.E.**, Cane, M.A., Zebiak, S.E., Clement, A., Volcanic and Solar Forcing of the Tropical Pacific Over the Past 1000 Years, *Journal of Climate*, 18, 447-456, 2005.
- D'Arrigo, R.D., Cook, E.R., Wilson, R.J., Allan, R., **Mann, M.E.**, On the Variability of ENSO Over the Past Six Centuries, *Geophysical Research Letters*, 32, L03711, doi: 10.1029/2004GL022055, 2005.
- Zhang, Z., **Mann, M.E.**, Coupled Patterns of Spatiotemporal Variability in Northern Hemisphere Sea Level Pressure and Conterminous U.S. Drought, *Journal of Geophysical Research*, 110, D03108, doi: 10.1029/2004JD004896, 2005.
- Schmidt, G.A., Shindell, D.T., Miller, R.L., **Mann, M.E.**, Rind, D., General Circulation Modeling of Holocene climate variability, *Quaternary Science Reviews*, 23, 2167-2181, 2004.
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- **Mann, M.E.**, On Smoothing Potentially Non-Stationary Climate Time Series, *Geophysical Research Letters*, 31, L07214, doi: 10.1029/2004GL019569, 2004.
- Schmidt, G.A., **Mann, M.E.**, Reply to comment on ''Ground vs. surface air temperature trends: Implications for borehole surface temperature reconstructions''by D. Chapman et al., *Geophysical Research Letters*, 31, L07206, doi: 10.1029/2003GL0119144, 2004.

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- Shindell, D.T., Schmidt, G.A., **Mann, M.E.**, Faluvegi, G., Dynamic winter climate response to large tropical volcanic eruptions since 1600, *Journal of Geophysical Research*, 109, D05104, doi: 10.1029/2003JD004151, 2004.
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- Adams, J.B., **Mann, M.E.**, Ammann, C.M., Proxy evidence for an El Nino-like Response to Volcanic Forcing, *Nature*, 426, 274-278, 2003.
- Shindell, D.T., Schmidt, G.A., Miller, R., **Mann, M.E.**, Volcanic and Solar forcing of Climate Change During the Pre-Industrial era, *Journal of Climate*, 16, 4094-4107, 2003.
- **Mann, M.E.**, Jones, P.D., Global Surface Temperatures over the Past two Millennia, *Geophysical Research Letters*, 30 (15), 1820, doi: 10.1029/2003GL017814, 2003.
- **Mann, M.E.**, Schmidt, G.A., Ground vs. Surface Air Temperature Trends: Implications for Borehole Surface Temperature Reconstructions, *Geophysical Research Letters*, 30 (12), 1607, doi: 10.1029/2003GL017170, 2003.
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- **Mann, M.E.**, Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., Optimal Surface Temperature Reconstructions Using Terrestrial Borehole Data, *Journal of Geophysical Research,* 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003.
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Other Reviewed Contributions

- Luterbacher, J., **et al**,. Mediterranean Climate Variability over the Last Centuries: A Review, in *The Mediterranean Climate: an overview of the main characteristics and issues*, P. Lionello, P. Malanotte-Rizzoli, and R. Boscolo (eds), Elsevier, in press, 2005.
- **Mann, M.E.**, Climate Variability and Change, Last 1000 years, *Encyclopedia of Paleoclimatology and Ancient Environments*, in press, 2005.
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- **Mann, M.E.**, Bradley, R.S., Briffa, K., Cole, J., Hughes, M.K., Jones, J.M., Overpeck, J.T., von Storch, H., Wanner, H., Weber, S.L., Widmann, M., Reconstructing the Climate of the Late Holocene, *Eos*, 82, 553, 2001.
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- **Mann, M.E.**, Little Ice Age, MacCracken, M.C. & Perry, J.S. (eds) *Encyclopedia of Global Environmental Change,* John Wiley and Sons Ltd, London, UK, pp. 504-509, 2001.
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- Bradley, R.S., Hughes, M.K. and **Mann, M.E.,** Comments on "Detection and Attribution of Recent Climate Change: A Status Report", *Bulletin of the American Meteorological Society*, 81, 2987-2990, 2000.
- **Mann, M.E.,** Bradley, R.S., Hughes, M.K., Long-term variability in the El Nino Southern Oscillation and associated teleconnections, Diaz, H.F. & Markgraf, V. (eds) *El Nino and the Southern Oscillation: Multiscale Variability and its Impacts on Natural Ecosystems and Society,* Cambridge University Press, Cambridge, UK, 357-412, 2000.
- **Mann, M.E.**, Lessons For a New Millennium (invited 'perspective' article), *Science*, 289, 253-254, 2000.
- **Mann, M.E.**, Bradley, R.S., Hughes, M.K. and Jones, P.D., Global Temperature Patterns, *Science*, 280, 2029-2030, 1998.
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Non-reviewed Contributions

- **Mann, M.E.,** Paleoclimate Implications for Recent Human Influence on Climate, *International Seminar on Nuclear War and Planetary Emergencies, Nuclear Strategy and Peace Technology*, The Science and Culture Series, edited by A. Zichichi, R. Ragaini, pp. 86-91, 2003.
- **Mann, M.E.,** Bradley, R.S., Hughes, M.K., Large-Scale Surface Temperature Changes During the Past Millennium, *Abstracts of AMQUA annual meeting,, Fayetteville, Arkansas, May 22-24,* pp 17-12, 2000.
- Jones, P.D., Briffa, K.R., Osborn, T.J., **Mann, M.E.**, Bradley, R.S., Hughes, M.K., Cover Figure for World Meteorological Organization (WMO) 50th Year Anniversary Publication*: Temperature changes over the last Millennium*, 2000.
- M.K.Hughes, **Mann, M.E.,** Bradley, R.S., 1000 years of Northern Hemisphere temperature from natural archives. *Abstracts of PAGES Workshop on South Asian Paleoenvironments, Pune, India, February 4-5,* pp 11-15, 2000.
- M.K.Hughes, **Mann, M.E.,** Bradley, R.S., Climate variability of the last 1000 years from annual-resolution natural archives. *Proceedings of CCV99, International Conference on Climate Change and Variability, Tokyo Metropolitan University, September 1999.*

- **Mann, M.E.**, Bradley, R.S., Global Climate Variations over the Past 250 Years: Relationships With the Middle East, *Transformations of Middle Eastern Natural Environments: Legacies and Lessons*, Yale School of Forestry and Environmental Studies Bulletin Series, 103, 429-443, 1998.
- **Mann, M.E.**, On Reconstructing Past Centuries' Temperatures, *World Climate Report*, 3, 26, 6-7, 1998.
- **Mann, M.E.**, *A Study of Ocean-Atmosphere Interaction and Low Frequency Variability of the Climate System*, PhD Thesis, 283pp, Yale University, New Haven, CT, 1998.
- **Mann, M.E.**, Bradley, R.S., Hughes, M.K., Multiproxy based reconstructions of large-scale surface temperature patterns during the past several centuries, *Proceedings of the Ninth Symposium on Global Change Studies*, Phoenix, AZ, January 1998.
- Rajagopalan, B., **Mann, M.E.**, Lall,U., Climatic forecasting based on multivariate frequency and time domain-based prediction, *Proceedings of the Eighth Symposium on Global Change Studies*, Long Beach, CA, February 1997.
- **Mann, M.E.**, Bradley, R.S., Hughes, M.K., Large-Scale Climatic Recontructions Based on High-Resolution Multi-Proxy Data, *Proceedings of the Eighth symposium on Global Change Studies*, Long Beach, CA, February 1997.
- **Mann, M.E.**, Rajagopalan, B., Moon, Y.I., Lall, U., Climatic forecasting based on multivariate frequency and time domain-based predictions, *Proceedings of the 13th Conference on Probability & Statistics in the Atmospheric Sciences*, American Meteorological Society, San Francisco, CA, February 1996.
- Moon, Y.I., Lall, U., Rajagopalan, B., **Mann, M.E.**, Forecasting hydroclimatic variables using a nonparametric, nonlinear time series model, *Proceedings of the 13th Conference on Probability & Statistics in the Atmospheric Sciences*, American Meteorological Society, San Francisco, CA, February 1996.
- Marshall, S., **Mann, M.E.**, Oglesby, R.J., Saltzman, B., A comparison of the CCM1-simulated climates for pre-industrial and present-day CO2 levels, *Proceedings of the 6th Conference on Climate Variations*, American Meteorological Society, 12-15, Nashville, TN, January 1994.
- **Mann, M.E.**, Park, J., Globally correlated variability in surface temperatures, *Proceedings of the 6th Conference on Climate Variations*, American Meteorological Society, 297-301, Nashville, TN, January 1994.

Invited Lectures & Workshops

- Invited presentation, *Symposium on Energy for the 21st Century*: "Climate Change and Global Warming", Amherst, MA, Apr 9, 2005.
- Invited lecture, Blue Ridge Community College, Harrisonburg, VA, Apr 5, 2005.
- Invited lecture, Pennsylvania State University, Dept. of Geosciences, State College, PA, Mar 29, 2005.
- Invited lecture, Spring meeting of the American Statistical Association, Philadelphia Chapter (ASAP), Philadelphia, PA, Mar 24, 2005.
- Invited lecture, Purdue University, Dept. of Earth & Atmospheric Sciences, West Lafayette, IN, Mar 3, 2005.
- Invited presentation, *Symposium on Whole Earth Systems* (honoring Stephen Schneider): "Climate over the Past Millennium Or So", Palo Alto, CA, Feb 10, 2005.
- Invited presentation, *Meeting on Decadal Variability in the Sun and Climate*, Solar Radiation and Climate Experiment (SORCE) annual meeting, Oct 28, 2004.
- Invited presentation, *Workshop on Historical Reconstruction of Climate Variability in Mediterranean Regions*, Bologna, Italy, Oct 6, 2004.
- Invited lecture, University of Arizona, Atmospheric Sciences Department, Tucson, AZ, Jul 1, 2004.

- Invited presentation, *First International CLIVAR Science Conference*: "Comparisons of Observed Paleoclimate and Model-Based Studies of Climate Changes Over the Past Two Millennia" (co-author: K. Briffa), Baltimore, MD, Jun 23, 2004.
- Invited lecture, Ohio State University, Geology Department, Columbus, OH, May 6, 2004.
- Invited lecture, Ohio State University, Physics Department, Columbus, OH, May 4, 2004.
- Invited presentation, *EGU Spring meeting*: "Volcanic and Solar Forcing of El Nino Over the Past 1000 Years" (co-authors: M.A. Cane, S. E. Zebiak, A. Clement), Nice, France, Apr 27, 2004.
- Invited lecture, University of North Carolina, Carolina Environmental Program (CEP), Chapel Hill, NC, Apr 16, 2004.
- Invited presentation, *Meeting on Tree Rings and Climate*, Tucson, AZ, Apr 6, 2004.
- Invited presentation, *GSA Northeastern Section meeting*, McLean, VA, Mar 27, 2004.
- Invited lecture, Pennsylvania State University, Dept. of Meteorology, State College, PA, Mar 4, 2004.
- Invited presentation, *NASA/CLIVAR/IPRC workshop on Decadal Climate Variability*, Kona, Hawaii, Feb 23, 2004.
- Invited presentation (co-authors B. Adams, C. Ammann, R. Miller, S. Rutherford, G. Schmidt, D. Shindell), *AGU Fall meeting*: "Spatially- and seasonally-specific responses to forcing as detected in paleoclimate reconstructions of past centuries", San Francisco, CA, Dec 12, 2003.
- Invited presentation, 30th session of the *International Seminars on Planetary Emergencies*, World Federation of Scientists, Erice, Italy, Aug 20, 2003.
- Invited presentation, *AGU/EGS Joint Spring meeting*: "Ground vs. Surface Temperature Trends: Implications for Borehole Surface Temperature Reconstructions" (co-author: G. Schmidt), Nice, France, Apr 10, 2003.
- 'Vetlesen' distinguished lecturer, Graduate School of Oceanography, University of Rhode Island, Narragansett, Rhode Island, Mar 5, 2003.
- Invited presentation (co-authors G. Schmidt, D. Shindell), *AGU Fall meeting*: "Climate Changes during the Past Millennium"; San Francisco, CA, Dec 8, 2002.
- Invited lecture, Geophysical Fluid Dynamics Laboratory, Princeton, NJ, Oct, 17, 2002.
- Invited lecture, Goddard Institute for Space Studies, New York, NY, Oct 10, 2002
- Invited presentation, Chapman conference on *Volcanism and the Earth's Atmosphere*, Santorini Greece, Jun 20, 2002.
- Invited lecture, University of Copenhagen, Copenhagen, Denmark, Jun 11, 2002.
- Invited presentation, *EGS Spring meeting*: "Climate Change and Forcing over the Past 500 years" (co-authors: S. Rutherford, R. Bradley, M. Hughes), Nice, France, Apr 23, 2002.
- Invited participant, Waxter Environmental Forum, Sweet Briar College, Mar 14, 2002.
- Invited lecture, College of Marine Science, University of South Florida, Mar 8, 2002.
- Invited lecture, Center for Coastal Physical Oceanography (CCPO), Old Dominion University, Feb 18, 2002.
- Invited lecture, INSTAAR, University of Colorado, Jan 28, 2002.
- Invited lecture, Department of Geological Sciences, University of Colorado, Jan 25, 2002.
- Invited lecture, Department of Geology, University of North Carolina, Jan 18, 2002.
- Invited presentation, *AGU Fall meeting*: "Climate Reconstruction using Pseudoproxies", San Francisco, CA, December 11, 2001.
- Invited lecture, Department of Earth & Ocean Sciences, Duke University, Nov 29, 2001.
- Session introduction, Invited session on "Climate Change Detection/Attribution", National Academy of Sciences *Frontiers of Science* annual symposium, Irvine, California, Nov 9, 2001.
- Invited lecture, *Richard Foster Flint Symposium Honoring the Memory of Barry Saltzman,* Nov 3, 2001.
- Invited lecture, DCESS, University of Copenhagen, Copenhagen, Denmark, Aug 13, 2001.

- Invited presentation, *Global Change Open Science Conference*: "Variability in El Nino and the Global ENSO Phenomenon in Past Centuries", Amsterdam, Netherlands, Jul 11, 2001.
- Invited lecture, Courant Institute, New York University, New York, NY, Apr 4, 2001.
- Invited presentation, *EGS Spring meeting*: "Comparison of Large-Scale Proxy-Based Temperature Reconstructions over the Past Few Centuries" (co-authors: S. Rutherford, T. Osborn), Nice, France, March 29, 2001.
- Invited lecture, Turner Lecture Series, Department of Geological Sciences, University of Michigan, Ann Arbor, MI, Mar 16, 2001.
- Invited presentation, *NASA/CLIVAR/IPRC workshop on Decadal Climate Variability*, Honolulu, Hawaii, Jan 8, 2001.
- Invited presentation, *AGU Fall meeting*: "Use of Proxy Climate Data in Climate Change Detection", San Francisco, CA, Dec 16, 2000.
- Invited presentation, *CLIVAR Decadal Climate Predictability Workshop*, La Jolla, California, Oct 4, 2000.
- Invited presentation, 5th *EPA NHEERL symposium on Indicators in Health and Ecological Risk Assessment*, Jun 7, 2000.
- Invited presentation, AMQUA annual meeting, Fayetteville Arkansas, May 22, 2000.
- Invited presentation, *EGS Spring meeting*: "Seasonal Proxy-Reconstructed Surface Temperature Patterns in Past Centuries", Nice, France, Apr 22, 2000
- Invited public lecture, Smithsonian Environmental Research Center (SERC), Edgewater Maryland, Mar 15, 2000.
- Invited participant and speaker, workshop on *El Niño: Past Present and Future*, Seabrook Island South Carolina, Feb 28-Mar 2, 2000.
- Invited guest introductions, *US Global Change Research Program Seminar Series,* Dirksen Senate Office Building, Capitol Hill, Washington DC, Nov 23, 1999.
- Invited participant and speaker, *PAGES/CLIVAR workshop on Climate of the Last Millennium*, Venice Italy, Nov 8 - 12, 1999
- Invited lecture, Geophysical Fluid Dynamics Laboratory, Princeton, NJ, Oct, 21, 1999.
- Invited lecture, NASA Goddard Institute for Space Studies, New York, NY, Oct 15, 1999.
- Invited lecture, Colloquium on Regional Modeling and Detection, International Center for Theoretical Physics (ICTP), Trieste, Italy, Jun 9, 1999.
- Invited presentation, *US Global Change Research Program Seminar Series,* Dirksen Senate Office Building, Capitol Hill, Washington DC, May 17, 1999.
- Invited lecture, Physical Oceanography Seminar Series, Woods Hole Oceanographic Institute, Woods Hole, MA, Mar 2, 1999.
- Invited lecture, Department of Environmental Sciences, University of Virginia, Charlottesville, VA, Feb 18, 1999.
- Invited lecture, School of Marine and Atmospheric Science, University of Miami, Coral Gables, FL, Nov 13, 1998.
- Invited lecture, Department of Mathematics & Statistics, University of Massachusetts, Amherst, MA, Nov 2, 1998.
- Invited presentation, *NASA workshop on Decadal Climate Variability*, Williamsburg, VA, Sep 22-25, 1998
- Invited presentation, *US Global Change Research Program Seminar Series,* Dirksen Senate Office Building, Capitol Hill, Washington DC, Jul 20, 1998
- Invited presentation, *AGU Spring meeting*: "Low-frequency Variations in Midwestern U.S Precipitation: Diagnosing ENSO and Anthropogenic effects", Boston, MA, May 1998
- Invited lecture, NOAA-CDC/ERL/CIRES, Boulder, Colorado, May 13, 1998

- Invited lecture, Graduate School of Oceanography, University of Rhode Island, Narragansett, Rhode Island, Mar 6, 1998
- Invited lecture, Oceanography Department, Texas A&M university, College Station, Texas, Dec 4, 1997
- Invited lecture, Geology Department, West Virginia University, Morgantown, West Virginia, Nov 14, 1997
- Invited talk, conference on *Transformations of Middle Eastern Natural Environments: Legacies and Lessons*, Council on Middle East Studies, Yale Center for International and Area Studies, Yale University, New Haven, Oct 30-Nov 1, 1997
- Invited lecture, Hadley Centre of the United Kingdom Meteorological office, Bracknell, UK, Oct 14, 1997
- Invited presentation, *The Cross-Validation of Proxy Climate Data and the Instrumental Record*, Joint Institute for the Study of Atmosphere and Ocean, University of Washington, Seattle, Jun 23-25, 1997
- Invited lecture, Geophysical Fluid Dynamics Laboratory, Princeton University, Princeton, New Jersey, Mar 4, 1997
- Invited lecture, Department of Atmospheric Sciences, University of California Los Angeles, Feb 14, 1997
- Invited presentation, *SST reconstruction from Proxy data and Optimal Network Design Strategy For Corals and other Annual Records from Tropical Systems (ARTS)* , Lamont Doherty Earth Observatory, Columbia University, Jan 15-16, 1997
- Invited lecture, Lamont Doherty Earth Observatory, Columbia University, Dec 4, 1996
- Invited lecture, Department of Physics, University of Bern, Bern Switzerland, Oct 26, 1996
- Invited presentation, *Links between Variations in Solar Activity, Atmospheric Conductivity, and Clouds: An Informal Workshop*, Los Alamos National Laboratory, Los Alamos, New Mexico, Jun 20-21, 1996
- Invited participant, *Application of Statistics to Modeling the Earth's Climate System*, National Center for Atmospheric Research, Boulder, Colorado. Jul 6-19, 1994
- Invited lecture, United States Geological Survey, *USGS Global Change seminar series,* Reston Virginia, Apr 12, 1993
- Invited lecture, Geology Dept., Brown University, Providence, Rhode Island, Mar 10, 1993
- Invited lecture, Dept. of Chemistry, University of Utah, Jul 14, 1989

Abstracts and Talks

- Goosse, H., Renssen, H., Timmermann, A., Bradley, R., **Mann, M.**, Using paleoclimate proxy-data to select an optimal realisation in an ensemble of simulations of the climate of the past millennium, EGU Spring meeting, Vienna, Austria, April 2005. (solicited)
- Graham, N., Ammann, C., Tomas, R., Hoerling, M., Xu, T., Hughes, M., **Mann, M.**, Understanding late Holocene climate transitions in the Pacific: Results from proxy-guided AGCM experiments, EGU Spring meeting, Vienna, Austria, April 2005. (solicited)
- Cook, E.; Herweijer, C.; Seager, R.; **Mann, M.**, Long-term drought variability over North America and its connection to global and tropical Pacific SST forcing, EGU Spring meeting, Vienna, Austria, April 2005.
- Emile-Geay, J., Cane, M., **Mann, M.**, Bond, G., Radiative forcing of El Niño-Southern Oscillation over the Holocene: a model perspective, EGU Spring meeting, Vienna, Austria, April 2005.
- Cook, E., Cane, M., Seager, R., **Mann, M.**, Tropical Pacific Links to Long-Term Aridity Changes in the Western United States, Chapman conference on Tropical-Extratropical Teleconnections, Honolulu, HA, February 2005. (solicited)

- M. Cane, Emile-Geay, J., Seager, R., Clement, A. **Mann, M.**, Solar Influence on ENSO and the Tropics?, AGU Fall meeting, San Francisco, CA, December, 2004. (solicited)
- Cook, B.I., Smith, T.M., **Mann, M.E.**, The North Atlantic Oscillation and Regional Phenology Prediction Over Europe, AGU Fall meeting, San Francisco, CA, December, 2004.
- Steig, E.J., Schneider, D.P., **Mann, M.E.**, Rutherford, S.J., van Ommen, T., Winebrenner, D.P., Antarctic Temperatures since 1856, Western Antarctic Ice Shelf meeting, September 2004.
- Cronin, T.M., Willard, D.A., Thunell, Dwyer, G.S., **Mann, M.E.**, Seanger, C., Climate Variability from Estuarine Sediments: A Case Study of Chesapeake Bay, First International CLIVAR Science Conference, June 2004.
- Jones, P.D., **Mann, M.E.**, "Comparisons of millennial reconstructions of Northern Hemisphere (NH) temperatures from proxy data with coupled-GCM integrations" , EGU Spring meeting, Nice, France, April 2004.
- Luterbacher, J., Xoplaki, E, Fischer, E., Pauling, A., Gonzalez-Rouco, F.J., Garcia Herrera’ R., Guiot, J., Zorita, E., Jacobeit, J., Mariotti, A., Rimbu, N., Felis, T., Rodrigo’ F., Barriendos, M., **Mann, M.E.**, Touchan, R., Past mediterranean climate variability – present knowledge and Scientific Challenges For Future Research, EGU Spring meeting, Nice, France, April 2004.
- Saenger, C.P., Cronin, T.M., Thunell, R. Vann, C., Dwyer, G., Seal, R. II, **Mann, M.E.**, Eastern U.S. Holocene Climate Variability from Chesapeake Bay Sediments, Geological Society of America Annual Northeastern Section meeting, March 2004.
- Hughes, M.K., Ni, F., **Mann, M.E.**, Park, J., Global Multidecadal to Century-Scale Climate Oscillations During the Last 1000 Years, AGU Fall meeting, San Francisco, CA, December, 2003.
- Shindell, D.T, Schmidt, G.A., **Mann, M.E.**, Miller, R.L., Northern Hemisphere Regional Climate Change during the Last Millennium, AGU Fall meeting, San Francisco, CA, December, 2003.
- Schmidt, G.A., Shindell, D.T., Miller, R.L., and **Mann, M.E.**, External forcing of climate change during the Holocene, Hafslo, Norway, August 2003.
- **Mann, M.E.**, Rutherford, S., Jones, P.D., Schmidt, G.A., Shindell, D., Climate changes during the past millennium, IUGG Meeting, Sapporo, Japan, July, 2003.
- Knight, R.G., Folland, C.K., Vellinga, M., **Mann, M.E.**, The Atlantic Multidecadal Oscillation and the Thermohaline Circulation: A Global-Scale Ocean-Atmosphere Mode Simulated in a 1400 Year Coupled Model Calculation, IUGG Meeting, Sapporo, Japan, July, 2003.
- Wiberg, P., Cleary, P., **Mann, M.E.**, Coupling wave and meteorological observations with bottom-boundary-layer models to investigate spatial and temporal variability in continental-shelf sediment transport, International Geological Congress, Florence, Italy, June, 2003.
- Knight, R.G., Folland, C.K., Vellinga, M., **Mann, M.E.**, The Atlantic Multidecadal Oscillation and the Thermohaline Circulation: A Global-Scale Ocean-Atmosphere Mode Simulated in a 1400 Year Coupled Model Calculation, Conference of the Royal Meteorological Society, June, 2003.
- D'Arrigo, R.D., Cook, E.R., **Mann, M.E.**, Jacoby, G.C., Tree-ring Reconstructions of Arctic Oscillation Indices Since AD 1650, ARCUS 15th Annual Meeting And Arctic Forum, Arlington, VA, April 2003.
- Shindell, D.T., Schmidt, G.A., Miller, R., **Mann, M.E.**, "Volcanic and Solar Forcing of Global and Regional Climate During the Preindustrial Era" , AGU/EGS Joint Spring meeting, Nice, France, April 2003.
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Global-scale temperature patterns and climate forcing over the past six centuries

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Spatially resolved global reconstructions of annual surface temperature patterns over the past six centuries are based on the multivariate calibration of widely distributed high-resolution proxy climate indicators. Time-dependent correlations of the reconstructions with time-series records representing changes in greenhouse-gas concentrations, solar irradiance, and volcanic aerosols suggest that each of these factors has contributed to the climate variability of the past 400 years, with greenhouse gases emerging as the dominant forcing during the twentieth century. Northern Hemisphere mean annual temperatures for three of the past eight years are warmer than any other year since (at least) AD 1400.

Knowing both the spatial and temporal patterns of climate change over the past several centuries remains a key to assessing a possible anthropogenic impact on post-industrial climate[1]. In addition to the possibility of warming due to increased concentrations of greenhouse gases during the past century, there is evidence that both solar irradiance and explosive volcanism have played an important part in forcing climate variations over the past several centuries[2,3]. The unforced 'natural variability' of the climate system may also be quite important on multidecadal and century timescales[4,5]. If a faithful empirical description of climate variability could be obtained for the past several centuries, a more confident estimation could be made of the roles of different external forcings and internal sources of variability on past and recent climate. Because widespread instrumental climate data are available for only about one century, we must use proxy climate indicators combined with any very long instrumental records that are available to obtain such an empirical description of large-scale climate variability during past centuries. A variety of studies have sought to use a 'multiproxy' approach to understand long-term climate variations, by analysing a widely distributed set of proxy and instrumental climate indicators[1,5–8] to yield insights into long-term global climate variations. Building on such past studies, we take a new statistical approach to reconstructing global patterns of annual temperature back to the beginning of the fifteenth century, based on the calibration of multiproxy data networks by the dominant patterns of temperature variability in the instrumental record.

Using these statistically verifiable yearly global temperature reconstructions, we analyse the spatiotemporal patterns of climate change over the past 500 years, and then take an empirical approach to estimating the relationship between global temperature changes, variations in volcanic aerosols, solar irradiance and greenhouse-gas concentrations during the same period.

Data

We use a multiproxy network consisting of widely distributed high-quality annual-resolution proxy climate indicators, individually collected and formerly analysed by many palaeoclimate researchers (details and references are available: see Supplementary Information). The network includes (Fig. 1a) the collection of annual-resolution dendroclimatic, ice core, ice melt, and long historical records used by Bradley and Jones[6] combined with other coral, ice core, dendroclimatic, and long instrumental records. The long instrumental records have been formed into annual mean anomalies relative to the 1902–80 reference period, and gridded onto a 5° × 5° grid (yielding 11 temperature grid-point series and 12 precipitation grid-point series dating back to 1820 or earlier) similar to that shown in Fig. 1b. Certain densely sampled regional dendroclimatic data sets have been represented in the network by a smaller number of leading principal components (typically 3–11 depending on the spatial extent and size of the data set). This form of representation ensures a reasonably homogeneous spatial sampling in the multiproxy network (112 indicators back to 1820).

Potential limitations specific to each type of proxy data series must be carefully taken into account in building an appropriate network. Dating errors in a given record (for example, incorrectly assigned annual layers or rings) are particularly detrimental if mutual information is sought to describe climate patterns on a year-by-year basis. Standardization of certain biological proxy records relative to estimated growth trends, and the limits of constituent chronology segment lengths (for example, in dendroclimatic reconstructions), can restrict the maximum timescale of climate variability that is recorded[9], and only a limited subset of the indicators in the multiproxy network may thus 'anchor in' the longest-term trends (for example, variations on timescales greater than 500 years). However, the dendroclimatic data used were carefully screened for conservative standardization and sizeable segment lengths. Moreover, the mutual information contained in a diverse and widely distributed set of independent climate indicators can more faithfully capture the consistent climate signal that is present, reducing the compromising effects of biases and weaknesses in the individual indicators.

Monthly instrumental land air and sea surface temperature[10] grid-point data (Fig. 1b) from the period 1902–95 are used to calibrate the proxy data set. Although there are notable spatial gaps, this network covers significant enough portions of the globe to form reliable estimates of Northern Hemisphere mean temperature, and certain regional indices of particular importance such as the 'NINO3' eastern tropical Pacific surface temperature index often used to describe the El Niño phenomenon. The NINO3 index is constructed from the eight grid-points available within the conventional NINO3 box (5° S to 5° N, 90–150° W).

Multiproxy calibration

Although studies have shown that well chosen regional paleoclimate reconstructions can act as surprisingly representative surrogates for

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large-scale climate[11–13], multiproxy networks seem to provide the greatest opportunity for large-scale palaeoclimate reconstruction[6] and climate signal detection[1,5]. There is a rich tradition of multivariate statistical calibration approaches to palaeoclimate reconstruction, particularly in the field of dendroclimatology where the relative strengths and weaknesses of various approaches to multivariate calibration have been well studied[14,15]. Such approaches have been applied to regional dendroclimatic networks to reconstruct regional patterns of temperature[16,17] and atmospheric circulation[18–20] or specific climate phenomena such as the Southern Oscillation[21]. Largely because of the inhomogeneity of the information represented by different types of indicators in a true 'multiproxy' network, we found conventional approaches (for example, canonical correlation analysis, CCA, of the proxy and instrumental data sets) to be relatively ineffective. Our approach to climate pattern reconstruction relates closely to statistical approaches which have recently been applied to the problem of filling-in sparse early instrumental climate fields, based on calibration of the sparse sub-networks against the more widespread patterns of variability that can be resolved in shorter data sets[22,23]. We first decompose the twentieth-century instrumental data into its dominant patterns of variability, and subsequently calibrate the individual climate proxy indicators against the time histories of these distinct patterns during their mutual interval of overlap. One can think of the instrumental patterns as 'training' templates against which we calibrate or 'train' the much longer proxy data (that is, the 'trainee' data) during the shorter calibration period which they overlap. This calibration allows us to subsequently solve an 'inverse problem' whereby best estimates of surface temperature patterns are deduced back in time before the calibration period, from the multiproxy network alone.

Implicit in our approach are at least three fundamental assumptions. (1) The indicators in our multiproxy trainee network are linearly related to one or more of the instrumental training patterns. In the relatively unlikely event that a proxy indicator represents a truly local climate phenomenon which is uncorrelated with larger-

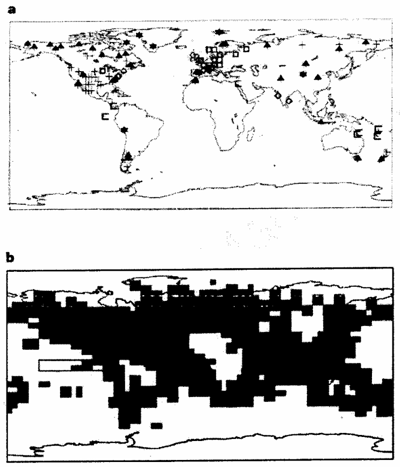

Figure 1 Data used in this study. **a**, Distribution of annual-resolution proxy indicators used in this study. Dendroclimatic reconstructions are indicated by 'tree' symbols, ice core/ice melt proxies by 'star' symbols and coral records by 'C' symbols. Long historical records and instrumental 'grid-points' series are shown by squares (temperature) or diamonds (precipitation). Groups of '+' symbols indicate principal components of dense tree-ring sub-networks, with the number of such symbols indicating the number of retained principal components. Sites are shown dating back to at least 1820 (red), 1800 (blue-green), 1750 (green), 1600 (blue) and 1400 (black). Certain sites (for example, the Quelccaya ice core) consist of multiple proxy indicators (for example, multiple cores, and both $\delta^{18}O$ isotope and accumulation measurements). **b**, Distribution of the 1,082 nearly continuous available land air/sea surface temperature grid-point data available from 1902 onward, indicated by shading. The squares indicate the subset of 219 grid-points with nearly continuous records extending back to 1854 that are used for verification. Northern Hemisphere (NH) and global (GLB) mean temperature are estimated as areally weighted (that is, cosine latitude) averages over the Northern Hemisphere and global domains respectively.

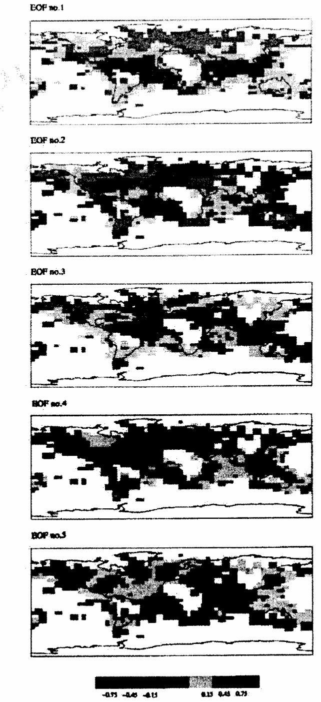

Figure 2 Empirical orthogonal functions (EOFs) for the five leading eigenvectors of the global temperature data from 1902 to 1980. The gridpoint areal weighting factor used in the PCA procedure has been removed from the EOFs so that relative temperature anomalies can be inferred from the patterns.

scale climate variations, or represents a highly nonlinear response to climate variations, this assumption will not be satisfied. (2) A relatively sparse but widely distributed sampling of long proxy and instrumental records may nonetheless sample most of the relatively small number of degrees of freedom in climate patterns at interannual and longer timescales. Regions not directly represented in the trainee network may nonetheless be indirectly represented through teleconnections with regions that are. The El Niño/Southern Oscillation (ENSO), for example, shows global-scale patterns of climatic influence[24], and is an example of a prominent pattern of variability which, if captured, can potentially describe variability in regions not directly sampled by the trainee data. (3) Patterns of variability captured by the multiproxy network have analogues in the patterns we resolve in the shorter instrumental data. This last assumption represents a fairly weak 'stationarity' requirement—we do not require that the climate itself be stationary. In fact, we expect that some sizeable trends in the climate may be resolved by our reconstructions. We do, however, assume that the fundamental spatial patterns of variation which the climate has shown during the past century are similar to those by which it has varied during past recent centuries. Studies of instrumental surface-temperature patterns suggest that such a form of stationarity holds up at least on multidecadal timescales, during the past century[23]. The statistical cross-validation exercises we describe later provide the best evidence that these key underlying assumptions hold.

We isolate the dominant patterns of the instrumental surface-temperature data through principal component analysis[25] (PCA). PCA provides a natural smoothing of the temperature field in terms of a small number of dominant patterns of variability or 'empirical eigenvectors'. Each of these eigenvectors is associated with a characteristic spatial pattern or 'empirical orthogonal function' (EOF) and its characteristic evolution in time or 'principal component' (PC). The ranking of the eigenvectors orders the fraction of variance they describe in the (standardized) multivariate data during the calibration period. The first five of these eigenvectors describe a fraction $\beta = 0.93$ (that is, 93%) of the global-mean (GLB) temperature variations, 85% of the Northern Hemisphere-mean (NH) variations, 67% of the NINO3 index, and 76% of the non-trend-related (DETR) NH variance (see Methods for a description of the β statistic used here as a measure of resolved variance). A sizeable fraction of the total multivariate spatiotemporal variance (MULT) in the raw (instrumental) data (27%) is described by these five eigenvectors, or about 30% of the standardized variance (no. 1 = 12%, no. 2 = 6.5%, no. 3 = 5%, no. 4 = 4%, no. 5 = 3.5%). Figure 2 shows the EOFs of the first five eigenvectors. The associated PCs and their reconstructed counterparts (RPCs) are discussed in the next section. The first eigenvector, associated with the significant global warming trend of the past century, describes much of the variability in the global (GLB = 88%) and hemispheric (NH = 73%) means. Subsequent eigenvectors, in contrast, describe much of the spatial variability relative to the large-scale means (that is, much of the remaining MULT). The second eigenvector is the dominant ENSO-related component, describing 41% of the variance in the NINO3 index. This eigenvector shows a modest negative trend which, in the eastern tropical Pacific, describes a 'La Niña'-like cooling trend[26], which opposes warming in the same region associated with the global warming pattern of the first eigenvector. The third eigenvector is associated largely with interannual-to-decadal scale variability in the Atlantic basin and carries the well-known temperature signature of the North Atlantic Oscillation (NAO)[27] and decadal tropical Atlantic dipole[28]. The fourth eigenvector describes a primarily multidecadal timescale variation with ENSO-scale and tropical/subtropical Atlantic features, while the fifth eigenvector is dominated by multidecadal variability in the entire Atlantic basin and neighbouring regions that has been widely noted elsewhere[29–34].

We calibrate each of the indicators in the multiproxy data network against these empirical eigenvectors at annual-mean resolution during the 1902–80 training interval. Although the seasonality of variability is potentially important—many extratropical proxy indicators, for example, reflect primarily warm-season variability[6,7]—we seek in the present study to resolve only annual-mean conditions, exploiting the seasonal climate persistence, and the fact that the mutual information from data reflecting various seasonal windows should provide complementary information regarding annual mean climate conditions[10]. Following this calibration, we apply an overdetermined optimization procedure to determine the best combination of eigenvectors represented by the multiproxy network back in time on a year-by-year basis, with a spatial coverage dictated only by the spatial extent of the instrumental training data. From the RPCs, spatial patterns and all relevant averages or indices can be readily determined. The details of the entire statistical approach are described in the Methods section.

The skill of the temperature reconstructions (that is, their statistical validity) back in time is established through a variety of complementary independent cross-validation or 'verification' exercises (see Methods). We summarize here the main results of these experiments (details of the quantitative results of the calibration and verification procedures are available; see Supplementary Information).

(1) In the reconstructions from 1820 onwards based on the full multiproxy network of 112 indicators, 11 eigenvectors are skilfully resolved (nos 1–5, 7, 9, 11, 14–16) describing ~70–80% of the variance in NH and GLB mean series in both calibration and verification. (Verification is based here on the independent 1854–1901 data set which was withheld; see Methods.) Figure 3 shows the spatial patterns of calibration β, and verification β and the squared correlation statistic r^2, demonstrating highly significant reconstructive skill over widespread regions of the reconstructed spatial domain. 30% of the full spatiotemporal variance in the gridded data set is captured in calibration, and 22% of the variance is verified in cross-validation. Some of the degradation in the verification score relative to the calibration score may reflect the decrease in instrumental data quality in many regions before the twentieth century rather than a true decrease in resolved variance. These scores thus compare favourably to the 40% total spatiotemporal variance that is described by simply filtering the raw 1902–80 instrumental data with 11 eigenvectors used in calibration, suggesting that the multiproxy calibrations are describing a level of variance in the data reasonably close to the optimal 'target' value. Although a verification NINO3 index is not available from 1854 to 1901, correlation of the reconstructed NINO3 index with the available Southern Oscillation index (SOI) data from 1865 to 1901 of $r = -0.38$ ($r^2 = 0.14$) compares reasonably with its target value given by the correlation between the actual instrumental NINO3 and SOI index from 1902 to 1980 ($r = -0.72$). Furthermore, the correspondence between the reconstructed NINO3 index warm events and historical[35] El Niño chronology back to 1820 (see Methods) is significant at the 98% level.

(2) The calibrations back to 1760, based on 93 indicators, continue to resolve at least nine eigenvectors (nos 1–5, 7, 9, 11, 15) with no degradation of calibration or verification resolved variance in NH, and only slight degradation in MULT (calibration ~27%, verification ~17%). Our reconstructions are thus largely indistinguishable in skill back to 1760.

(3) The network available back to 1700 of 74 indicators (including only two instrumental or historical indicators) skilfully resolves five eigenvectors (nos 1, 2, 5, 11, 15) and shows some significant signs of decrease in reconstructive skill. In this case, ~60–70% of NH variance is resolved in calibration and verification, ~14–18% of MULT in calibration, and 10–12% of MULT in verification. The verification r of NINO3 with the SOI is in the range of $r \approx -0.25$ to -0.35, which is statistically significant (as is the correspondence

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with the historical[35] chronology back to 1700) but notably inferior o the later calibrations. In short, both spatial patterns and large-scale means are skilfully resolved, but with significantly less resolved variance than in later calibrations.

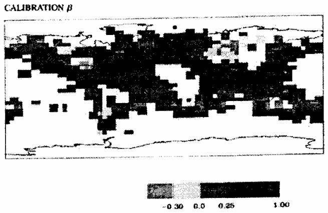
(4) The network of 57 indicators back to 1600 (including one historical record) skilfully resolves four eigenvectors (nos 1, 2, 11, 15). 67% of NH is resolved in calibration, and 53% in verification. 14% of MULT is resolved in calibration, and 12% of MULT in verification. A significant, but modest, level of ENSO-scale variability is resolved in the calibrations.

(5) The network of 24 proxy indicators back to 1450 resolves two eigenvectors (nos 1, 2) and ~40–50% of NH in calibration and verification. Only ~10% of MULT is resolved in calibration and ~5% in verification. There is no skilful reconstruction of ENSO-scale variability. Thus spatial reconstructions are of marginal usefulness this far back, though the largest-scale quantities are still skilfully resolved.

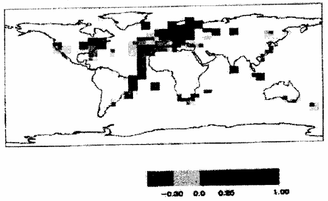
(6) The multiproxy network of 22 indicators available back to 1400 resolves only the first eigenvector, associated with 40–50% of resolved variance in NH in calibration and verification. There is no useful resolution of spatial patterns of variability this far back. The sparser networks available before 1400 show little evidence of skill in reconstructing even the first eigenvector, terminating useful reconstruction at the initial year AD 1400.

(7) Experiments using trainee networks containing only proxy (that is, no instrumental or historical) indicators establish the most

CALIBRATION β



VERIFICATION β



VERIFICATION r^2

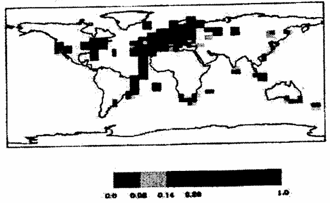
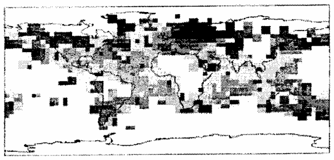
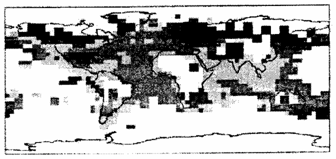


Figure 3 Spatial patterns of reconstruction statistics. Top, calibration β (based on 1902–80 data); middle, verification (based on 1854–1901 data) β; bottom, verification r^2 (also based on 1854–1901 data). For the β statistic, values that are insignificant at the 99% level are shown in grey negative; but 99% significant values are shown in yellow, and significant positive values are shown in two shades of red. For the r^2 statistic, statistically insignificant values (or any grid-points with unphysical values of correlation $r < 0$) are indicated in grey. The colour scale indicates values significant at the 90% (yellow), 99% (light red) and 99.9% (dark red) levels (these significance levels are slightly higher for the calibration statistics which are based on a longer period of time). A description of significance level estimation is provided in the Methods section.

1941 TEMPERATURE ANOMALY (RAW)



1941 TEMPERATURE ANOMALY (FILTERED)



1941 TEMPERATURE ANOMALY (RECONSTRUCTED)

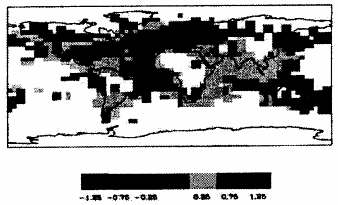


Figure 4 Comparison of the proxy-based spatial reconstructions of the anomaly pattern for 1941 versus the raw data. Comparisons based on actual (top), EOF-filtered (middle), and proxy-reconstructed (bottom) data. Anomalies (relative to 1902–80 climatology) are indicated by the colour scale shown in °C.

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truly independent cross-validation of the reconstruction as there is in this case neither spatial nor temporal dependence between the calibration and verification data sets. Such statistically significant verification is demonstrated at the grid-point level (calibration and verification resolved variance ~15% for the MULT statistic), at the largest scales (calibration and verification resolved variance ~60–65% for NH) and the NINO3-scale (90–95% statistical significance for all verification diagnostics). In contrast, networks containing only the 24 long historical or instrumental records available back to 1820 resolve only ~30% of NH in calibration or verification, and the modest multivariate calibration and verification resolved variance scores of MULT (~10%) are artificially inflated by the high degree of spatial correlation between the instrumental 'multi-proxy' predictor and instrumental predictand data. No evidence of skilful ENSO-scale reconstruction is evident in these latter reconstructions. In short, the inclusion of the proxy data in the 'multi-proxy' network is essential for the most skilful reconstructions. But certain sub-components of the proxy dataset (for example, the dendroclimatic indicators) appear to be especially important in resolving the large-scale temperature patterns, with notable decreases in the scores reported for the proxy data set if all dendroclimatic indicators are withheld from the multiproxy network. On the other hand, the long-term trend in NH is relatively robust to the inclusion of dendroclimatic indicators in the network, suggesting that potential tree growth trend biases are not influential in the multiproxy climate reconstructions. The network of all combined proxy and long instrumental/historical indicators provide the greatest cross-validated estimates of skilful reconstruction, and are used in obtaining the reconstructions described below.

Temperature reconstructions

The reconstructions discussed here are derived using all indicators available, and using the optimal eigenvector subsets determined in the calibration experiments described above (11 from 1780–1980, 9 from 1760–1779, 8 from 1750–1759, 5 from 1700–1749, 4 from 1600–1699, 2 from 1450–1599, 1 from 1400–1449). To better illustrate the workings and effectiveness of the proxy pattern reconstruction procedure, we show as an example (Fig. 4) the actual, the EOF-filtered, and the reconstructed temperature patterns for a year (1941) during the calibration interval. This year was a known ENSO year, associated with a warm eastern tropical Pacific and a cold central North Pacific. Pronounced cold anomalies were also found over large parts of Eurasia. The proxy-reconstructed pattern captures these features, although in a relatively smoothed sense (describing ~30% of the full variance in that pattern), and is remarkably similar to the raw data once it has been filtered by retaining only the 11 eigenvectors (nos 1–5, 7, 9, 11, 14–16) used in pattern reconstruction. It is thus visually apparent that the multi-proxy network is quite capable of resolving much of the structure resolved by the eigenvectors retained in the calibration process.

We consider the temporal variations in the first five RPCs (Fig. 5a). The positive trend in RPC no. 1 during the twentieth century is clearly exceptional in the context of the long-term variability in the associated eigenvector, and indeed describes much of the unprecedented warming trend evident in the NH reconstruction. The negative trend in RPC no. 2 during the past century is also anomalous in the context of the longer-term evolution of the associated eigenvector. The recent negative trend is associated with a pattern of cooling in the eastern tropical Pacific (superimposed on warming associated with the pattern of eigenvector no. 1) which may be a modulating negative feedback on global warming[26]. RPC no. 5 shows notable multidecadal variability throughout both the modern and pre-calibration interval, associated with the wavelike trend of warming and subsequent cooling of the North Atlantic this century discussed earlier[29–33] and the longer-term multidecadal oscillations in this region detected in a previous analysis of proxy climate networks[5]. This variability may be associated with ocean–atmosphere processes related to the North Atlantic thermohaline circulation[4,34].

The long-term trends in the reconstructed annual mean NH series (Fig. 5b) are quite similar to those of decadal Northern Hemisphere summer temperature reconstructions[6], showing pronounced cold periods during the mid-seventeenth and nineteenth centuries, and somewhat warmer intervals during the mid-sixteenth and late eighteenth centuries, with almost all years before the twentieth century well below the twentieth-century climatological mean. Taking into account the uncertainties in our NH reconstruction (see Methods), it appears that the years 1990, 1995 and

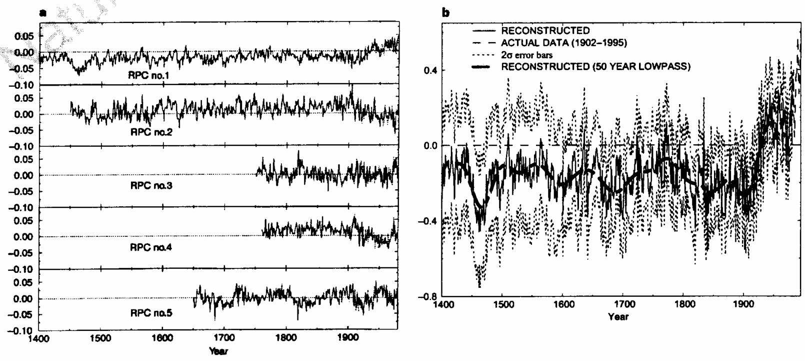

Figure 5 Time reconstructions (solid lines) along with raw data (dashed lines). **a**, For principal components (RPCs) 1–5; **b**, for Northern Hemisphere mean temperature (NH) in °C. In both cases, the zero line corresponds to the 1902–80 calibration mean of the quantity. For **b** raw data are shown up to 1995 and positive and negative 2σ uncertainty limits are shown by the light dotted lines surrounding the solid reconstruction, calculated as described in the Methods section.

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now 1997 (this value recently calculated and not shown) each show anomalies that are greater than any other year back to 1400 at 3 standard errors, or roughly a 99.7% level of certainty. We note that hemispheric mean values are not associated with globally or hemispherically uniform trends. An example of the global pattern for an historically documented[35] "very strong" El Niño year (1791) is shown in Fig. 6 top panel, demonstrating the classic warm eastern tropical Pacific and cold central North Pacific sea surface temperature patterns. Analysis of ENSO variability in these reconstructions is discussed in more detail elsewhere[36]. We also show the reconstructed pattern for 1816 (Fig. 6 bottom panel). Quite anomalous cold is evident throughout much of the Northern Hemisphere (even relative to this generally cold decade) but with a quadrupole pattern of warmth near Newfoundland and the Near East, and enhanced cold in the eastern United States and Europe consistent with the anomalous atmospheric circulation associated with the NAO pattern. Such a pattern is indeed observed in empirical[37] and model-based studies[38] of the atmospheric response to volcanic forcing. We infer in the 1816 temperature pattern a climatic response to the explosive Tambora eruption of April 1815 based on both the anomalous hemispheric coolness and the superimposed NAO-like pattern. Reconstructed time series RPCs nos 1–5, the NH series, the NINO3 index and reconstructions for specific grid-points can be obtained through the NOAA palaeoclimatology Web site (http://www.ngdc.noaa.gov/paleo/paleo.html).

Attribution of climate forcings

We take an empirical approach to detecting the possible effects of external forcings on the climate. The reconstructed NH series is taken as a diagnostic of the global climate, and we examine its relationship with three candidate external forcings during the period 1610–1995 including (1) CO_2 measurements[39] as a proxy for total greenhouse-gas changes, (2) reconstructed solar irradiance variations[2] and (3) the weighted historical 'dust veil index' (DVI) of explosive volcanism (see Fig. 31.1 in ref. 40) updated with recent data[41]. While we warn that historical series for these forcing agents are imperfectly known or measured, they do nonetheless represent our best estimates of the time-histories of the corresponding forcings. More detailed discussions of the estimation of, and potential sources of uncertainty or bias in, these series are available[2,39,40]. Industrial-aerosol forcing of the climate has also been suggested as an important forcing of recent climate[42,43], but its physical basis is still controversial[44], and difficult to estimate observationally. Noting that in any case, this forcing is not believed to be important before about 1940, its omission should be inconsequential in our long-term detection approach. Our empirical

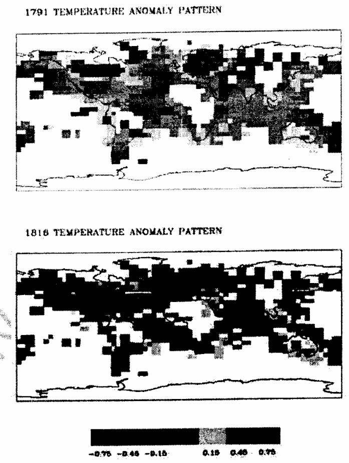
1791 TEMPERATURE ANOMALY PATTERN
1816 TEMPERATURE ANOMALY PATTERN
-0.75
-0.45
-0.15
0.15
0.45
0.75

Figure 6 Reconstructed annual temperature patterns for two example years. Top, 1791; bottom, 1816. The colours indicate regions which exceeded (either positively or negatively) the threshold indicated in °C. The zero baseline is defined by the 1902–80 climatological mean for each grid-point.

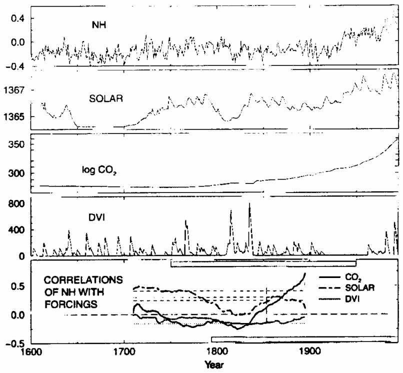

Figure 7 Relationships of Northern Hemisphere mean (NH) temperature with three candidate forcings between 1610 and 1995. Panels, (top to bottom) as follows. 'NH', reconstructed NH temperature series from 1610–1980, updated with instrumental data from 1981–95. 'Solar', reconstructed solar irradiance. 'log CO_2', greenhouse gases represented by atmospheric CO_2 measurements. 'DVI', weighted volcanic dust veil index. Bottom panel, evolving multivariate correlation of NH series with the three forcings NH, Solar, log CO_2. The time axis denotes the centre of a 200-year moving window. One-sided (positive) 90%, 95%, 99% significance levels (see text) for correlations with CO_2 and solar irradiance are shown by horizontal dashed lines, while the one-sided (negative) 90% significance threshold for correlations with the DVI series is shown by the horizontal dotted line. The grey bars indicate two difference 200-year windows of data, with the long-dashed vertical lines indicating the centre of the corresponding window.

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signal detection is complementary to that of model-based "fingerprint" signal detection studies[42,43,45]; while our empirical approach relies on the faithfulness of the reconstructed forcing series and on the assumption of a linear and contemporaneous response to forcings, it does not suffer the potential weaknesses of incomplete representations of internal feedback processes[26], poorly constrained parametrizations of climatic responses[44], and underestimated natural variability[1] in model-based studies. To the extent that the response to forcing is not contemporaneous, but rather is delayed owing to the inertia of the slow-response components of the climate system (for example, the ocean and cryosphere), our detection approach will tend to underestimate the response to forcings, making the approach a relatively conservative one.

We estimate the response of the climate to the three forcings based on an evolving multivariate regression method (Fig. 7). This time-dependent correlation approach generalizes on previous studies of (fixed) correlations between long-term Northern Hemisphere temperature records and possible forcing agents[2,3]. Normalized regression (that is, correlation) coefficients r are simultaneously estimated between each of the three forcing series and the NH series from 1610 to 1995 in a 200-year moving window. The first calculated value centred at 1710 is based on data from 1610 to 1809, and the last value, centred at 1895, is based on data from 1796 to 1995—that is, the most recent 200 years. A window width of 200 yr was chosen to ensure that any given window contains enough samples to provide good signal-to-noise ratios in correlation estimates. Nonetheless, all of the important conclusions drawn below are robust to choosing other reasonable (for example, 100-year) window widths.

We test the significance of the correlation coefficients (r) relative to a null hypothesis of random correlation arising from natural climate variability, taking into account the reduced degrees of freedom in the correlations owing to substantial trends and low-frequency variability in the NH series. The reduced degrees of freedom are modelled in terms of first-order markovian 'red noise' correlation structure of the data series, described by the lag-one autocorrelation coefficient ρ during a 200-year window. This parameter ranges from 0.48 in the first window (1610–1809) to 0.77 in the final window (1796–1995) of the moving correlation, the considerably larger recent value associated with the substantial global warming trend of the past century. This latter trend has been shown to be inconsistent with red noise[46] and could thus itself be argued as indicative of externally forced variability. An argument could in this sense, be made for using the smaller pre-industrial value $\rho = 0.48$ of the NH series in estimating the statistical degrees of freedom appropriate for the null hypothesis of natural variability. Nonetheless, we make the conservative choice of adopting the largest value $\rho = 0.77$ as representative of the natural serial correlation in the series. We use Monte Carlo simulations to estimate the likelihood of chance spurious correlations of such serially correlated noise with each of the three actual forcing series. The associated confidence limits are approximately constant between sliding 200-year windows. For (positive) correlations with both CO_2 and solar irradiance, the confidence levels are both approximately 0.24 (90%), 0.31 (95%), 0.41 (99%), while for the 'whiter', relatively trendless, DVI index, the confidence levels for (negative) correlations are somewhat lower (−0.16, −0.20, −0.27 respectively). A one-sided significance test is used in each case because the physical nature of the forcing dictates a unique expected sign to the correlations (positive for CO_2 and solar irradiance variations, negative for the DVI fluctuations).

The correlation statistics indicate highly significant detection of solar irradiance forcing in the NH series during the 'Maunder Minimum' of solar activity from the mid-seventeenth to early eighteenth century which corresponds to an especially cold period. In turn, the steady increase in solar irradiance from the early nineteenth century through to the mid-twentieth century coincides with the general warming over the period, showing peak correlation during the mid-nineteenth century. The regression against solar irradiance indicates a sensitivity to changes in the 'solar constant' of $\sim 0.1 \mathrm{~K~W^{-1}~m^{-2}}$, which is consistent with recent model-based studies[42]. Greenhouse forcing, on the other hand, shows no sign of significance until a large positive correlation sharply emerges as the moving window slides into the twentieth century. The partial correlation with CO_2 indeed dominates over that of solar irradiance for the most recent 200-year interval, as increases in temperature and CO_2 simultaneously accelerate through to the end of 1995, while solar irradiance levels off after the mid-twentieth century. It is reasonable to infer that greenhouse-gas forcing is now the dominant external forcing of the climate system. Explosive volcanism exhibits the expected marginally significant negative correlation with temperature during much of 1610–1995 period, most pronounced in the 200-year window centred near 1830 which includes the most explosive volcanic events.

A variety of general circulation[42,47] and energy-balance model experiments[43,45,48] as well as statistical comparisons of twentieth-century global temperatures with forcing series[49] suggest that, although both solar and greenhouse-gas forcings play some role in explaining twentieth-century climate trends, greenhouse gases appear to play an increasingly dominant role during this century. Such a proposition is consistent with the results of this study.

As larger numbers of high-quality proxy reconstructions become available in diverse regions of the globe, it may be possible to assimilate a more globally representative multiproxy data network. Given the high level of skill possible in large-scale reconstruction back to 1400 with the present network, it is reasonable to hope that it may soon be possible to faithfully reconstruct mean global temperatures back over the entire millennium, resolving for example the enigmatic[7] medieval period. Geothermal measurements from boreholes[50] recover long-term temperature trends without many of the complications of traditional proxy indicators and, in combination with traditional multiproxy networks, may prove helpful in better resolving trends over many centuries. With a better knowledge of how the climate has varied before the twentieth century, we will be able to place even better constraints on the importance of natural and anthropogenic factors governing the climate of the past few centuries, factors which will no doubt continue to affect climate variability in the future, in addition to any anthropogenic effects. ☐

Methods

Statistics. We use as our primary diagnostic of calibration and verification reconstructive skill the conventional 'resolved variance' statistic;

$$\beta = 1 - \sum (y_{ref} - \hat{y})^2 \Big/ \sum y_{ref}^2$$

where y_{ref} is the reference series (the raw data in the case of calibration or the verification dataset in the case of verification) and $\hat{y}$ is the series being compared to it (the proxy-reconstructed data for either calibration or verification). We compute β for each grid-point, and for the NH, GLB and MULT quantities. The sum extends over the time interval of comparison, and for the multivariate case (MULT), over all gridpoints as well. We also computed a calibration β statistic for the detrended NH series (DETR) to distinguish between explanatory variance associated with the notable trend of the twentieth century, and that related to departures from the trend.

β is a quite rigorous measure of the similarity between two variables, measuring their correspondence not only in terms of the relative departures from mean values (as does the correlation coefficient r) but also in terms of the means and absolute variance of the two series. For comparison, correlation (r) and squared-correlation (r^2) statistics are also determined. The expectation value for two random series is $\beta = -1$. Negative values of β may in fact be statistically significant for sufficient temporal degrees of freedom. Nonetheless, the threshold $\beta = 0$ defines the simple 'climatological' model in which a series is assigned its long-term mean. In this sense, statistically significant negative

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values of β might still be considered questionable in their predictive or reconstructive skill. Owing to the more rigorous 'match' between two series sought by β, highly significant values of β are possible even when r^2 is only marginally significant.

Significance levels were determined for r^2 from standard one-sided tables, accounting for decreased degrees of freedom owing to serial correlation. Significance levels for β were estimated by Monte Carlo simulations, also taking serial correlation into account. Serial correlation is assumed to follow from the null model of AR(1) red noise, and degrees of freedom are estimated based on the lag-one autocorrelation coefficients (ρ) for the two series being compared. Although the values of ρ differ from grid-point to grid-point, this variation is relatively small, making it simplest to use the ensemble average values of ρ over the domain ($\rho \approx 0.2$).

Calibration. With the spatial sampling of $M = 1{,}082$ continuous monthly grid-point surface temperature anomaly (that is, de-seasonalized) data used (Fig. 1b), the $N = 1{,}128$ months of data available from 1902 to 1995 were sufficient for a unique, overdetermined eigenvector decomposition (note that $N' = 94$ years of the annual mean data would, in contrast, not be sufficient).

For each grid-point, the mean was removed, and the series was normalized by its standard deviation. A standardized data matrix T of the data is formed by weighting each grid-point by the cosine of its central latitude to ensure areally proportional contributed variance, and a conventional Principal Component Analysis (PCA) is performed,

$$T = \sum_{k=1}^{K} \lambda_k \mathbf{u}_k^{\dagger} \mathbf{v}_k$$

decomposing the dataset into its dominant spatiotemporal eigenvectors. The M-vector or empirical orthogonal function (EOF) $\mathbf{v}_k$ describes the relative spatial pattern of the kth eigenvector, the N-vector $\mathbf{u}_k$ or principal component (PC) describes its variation over time, and the scalar λ_k describes the associated fraction of resolved (standardized and weighted) data variance.

In a given calibration exercise, we retain a specified subset of the annually averaged eigenvectors, the annually averaged PCs denoted by $\bar{u}_n^k$, where $n = 1, \ldots, \bar{N}$, $\bar{N} = 79$ is the number of annual averages used of the N-month length data set. In practice, only a small subset N_{eofs} of the highest-rank eigenvectors turn out to be useful in these exercises from the standpoint of verifiable reconstructive skill. An objective criterion was used to determine the particular set of eigenvectors which should be used in the calibration as follows. Preisendorfer's[25] selection rule 'rule N' was applied to the multiproxy network to determine the approximate number N_{eofs} of significant independent climate patterns that are resolved by the network, taking into account the spatial correlation within the multiproxy data set. Because the ordering of various eigenvectors in terms of their prominence in the instrumental data, and their prominence as represented by the multiproxy network, need not be the same, we allowed for the selection of non-contiguous sequences of the instrumental eigenvectors. We chose the optimal group of N_{eofs} eigenvectors, from among a larger set (for example, the first 16) of the highest-rank eigenvectors, as the group of eigenvectors which maximized the calibration explained variance. It was encouraging from a consistency standpoint that this subset typically corresponded quite closely to the subset which maximized the verification explained variance statistics (see below), but the objective criterion was, as it should be, independent of the verification process. We emphasize, furthermore, that statistical significance was robustly established, as neither the measures of statistical skill nor the reconstructions themselves were highly sensitive to the precise criterion for selection. In addition to the above means of cross-validation, we also tested the network for sensitivity to the inclusion or elimination of particular trainee data (for example, instrumental/historical records, non-instrumental/historical records, or dendroclimatic proxy indicators).

These N_{eofs} eigenvectors were trained against the N_{proxy} indicators, by finding the least-squares optimal combination of the N_{eofs} PCs represented by each individual proxy indicator during the $\bar{N} = 79$ year training interval from 1902 to 1980 (the training interval is terminated at 1980 because many of the proxy series terminate at or shortly after 1980). The proxy series and PCs were formed into anomalies relative to the same 1902–80 reference period mean, and the proxy series were also normalized by their standard deviations during that period. This proxy-by-proxy calibration is well posed (that is, a unique optimal solution exists) as long as $\bar{N} > N_{\mathrm{eofs}}$ (a limit never approached in this study) and can be expressed as the least-squares solution to the overdetermined matrix equation, $U\mathbf{x} = \mathbf{y}^{(\mathbf{p})}$, where

$$U = \begin{bmatrix} \bar{u}_1^{(1)} & \bar{u}_1^{(2)} & \ldots & u_1^{(N_{\mathrm{eofs}})} \\ \bar{u}_2^{(1)} & \bar{u}_2^{(2)} & \ldots & u_2^{(N_{\mathrm{eofs}})} \\ \vdots & & & \\ \bar{u}_{\bar{N}}^{(1)} & \bar{u}_{\bar{N}}^{(2)} & \ldots & u_{\bar{N}}^{(N_{\mathrm{eofs}})} \end{bmatrix}$$

is the matrix of annual PCs, and

$$\mathbf{y}^{(\mathbf{p})} = \begin{bmatrix} y^{(p)} \\ y^{(p)}{}_2^1 \\ \vdots \\ y_{\bar{N}}^{(p)} \end{bmatrix}$$

is the time series $\bar{N}$-vector for proxy record p.

The N_{eofs}-length solution vector $\mathbf{x} = \mathbf{G}^{(\mathbf{p})}$ is obtained by solving the above overdetermined optimization problem by singular value decomposition for each proxy record $p = 1, \ldots, P$. This yields a matrix of coefficients relating the different proxies to their closest linear combination of the N_{eofs} PCs;

$$G = \begin{bmatrix} G_1^{(1)} & G_2^{(1)} & \ldots & G_{N_{\mathrm{eofs}}}^{(1)} \\ G_1^{(2)} & G_2^{(2)} & \ldots & G_{N_{\mathrm{eofs}}}^{(2)} \\ \vdots & & & \\ G_1^{(P)} & G_2^{(P)} & \ldots & G_{N_{\mathrm{eofs}}}^{(P)} \end{bmatrix}$$

This set of coefficients will not provide a single consistent solution, but rather represents an overdetermined relationship between the optimal weights on each on the N_{eofs} PCs and the multiproxy network.

Proxy-reconstructed patterns are thus obtained during the pre-calibration interval by the year-by-year solution of the overdetermined matrix equation, $\mathbf{Gz} = \mathbf{y}_{(j)}$, where $\mathbf{y}_{(j)}$ is the predictor vector of values of each of the P proxy indicators during year j. The predictand solution vector $\mathbf{z} = \hat{\mathbf{U}}$ contains the least-squares optimal values of each of the N_{eofs} PCs for a given year. This optimization is overdetermined (and thus well constrained) as long as $P > N_{\mathrm{eofs}}$ which is always realized in this study. It is noteworthy that, unlike conventional palaeoclimate transfer function approaches, there is no specific relationship between a given proxy indicator and a given predictand (that is, reconstructed PC). Instead, the best common choice of values for the small number of N_{eofs} predictands is determined from the mutual information present in the multiproxy network during any given year. The reconstruction approach is thus relatively resistant to errors or biases specific to any small number of indicators during a given year.

This yearly reconstruction process leads to annual sequences of the optimal reconstructions of the retained PCs, which we term the reconstructed principal components or RPCs and denote by $\hat{\mathbf{u}}^{\mathbf{k}}$. Once the RPCs are determined, the associated temperature patterns are readily obtained through the appropriate eigenvector expansion,

$$\hat{T} = \sum_{k=1}^{N_{\mathrm{eofs}}} \lambda_k \hat{\mathbf{u}}_k^{\dagger} \mathbf{v}_k$$

while quantities of interest (for example, NH) are calculated from the appropriate spatial averages, and appropriate calibration and verification resolved variance statistics are calculated from the raw and reconstructed data.

Several checks were performed to ensure a reasonably unbiased calibration procedure. The histograms of calibration residuals were examined for possible heteroscedasticity, but were found to pass a χ^2 test for gaussian characteristics at reasonably high levels of significance (NH, 95% level; NINO3, 99% level). The spectra of the calibration residuals for these quantities were, furthermore, found to be approximately 'white', showing little evidence for preferred or deficiently resolved timescales in the calibration process. Having established reasonably unbiased calibration residuals, we were able to calculate uncertainties in the reconstructions by assuming that the unresolved variance is gaussian distributed over time. This variance increases back in time (the increasingly sparse multiproxy network calibrates smaller fractions of variance), yielding error bars which expand back in time.

Verification. Verification resolved variance statistics (β) were determined based on two distinct verification data sets including (1) the sparse subset of the

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gridded data ($M' = 219$ grid-points) for which independent values are available from 1854 to 1901 (see Fig. 1b) and (2) the small subset of 11 very long instrumental estimated temperature grid-point averages (10 in Eurasia, 1 in North America—see Fig. 1a) constructed from the longest available station measurements. Each of these 'grid-point' series shared at least 70% of their variance with the corresponding temperature grid-point available from 1854–1980, providing verification back to at least 1820 in all cases (and back through the mid and early eighteenth century in many cases). Note that this latter verification data set is only temporally, but not spatially, independent of the multiproxy network itself, which contains these long instrumental grid-point series as a small subset of the network. In case (1), NH and GLB verification statistics are computed as well as the multivariate (MULT) grid-point level verification statistic, although these quantities represent different spatial samplings from those in the full calibration data set owing to the sparser sampling of the verification period. Case (2) provides a longer-term, albeit an even less spatially representative, multivariate verification statistic (MULTb). In this case, the spatial sampling does not permit meaningful estimates of NH or GLB mean quantities. In any of these diagnostics, a positive value of β is statistically significant at >99% confidence as established from Monte Carlo simulations. Verification skills for the NINO3 reconstructions are estimated by other means, as the actual NINO3 index is not available far beyond the beginning of the calibration period. The (negative) correlation r of NINO3 with the SOI annual-mean from 1865 to 1901 (P. D. Jones, personal communication), and a squared congruence statistic g^2 measuring the categorical match between the distribution of warm NINO3 events and the distribution of warm episodes according to the historical[35] chronology (available back to the beginning of 1525), were used for statistical cross-validation based on one-sided tables and Monte Carlo simulations, respectively. The results of all calibration and verification experiments are available; see Supplementary Information.

Received 9 May 1997; accepted 27 February 1998.

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Supplementary Information is available on *Nature*'s World-Wide Web site (http://www.nature.com) or as paper copy from Mary Sheehan at the London editorial office of *Nature*.

Acknowledgements. This work benefited from discussions with M. Cane, E. Cook, M. Evans, A. Kaplan and collaborators at the Lamont Doherty Earth Observatory. We acknowledge discussions with K. Briffa, T. Crowley, P. Jones, S. Manabe, R. Saravanan and K. Trenberth, as well as the comments of G. Hegerl. We thank R. D'Arrigo, D. Fisher, G. Jacoby, J. Lean, A. Robock, D. Stahle, C. Stockton, E. Vaganov, R. Villalba and the numerous contributors to the International Tree-Ring Data Bank and other palaeoclimate researchers who have made their data available to us for use in this study; we also thank F. Keimig, M. Munro, R. Holmes and C. Aramann for their technical assistance. This work was supported by the NSF and the US Department of Energy. M.E.M. acknowledges support through the Alexander Hollaender Distinguished Postdoctoral Research Fellowship program of the Department of Energy. This work is a contribution to the NSF- and NOAA-sponsored Analysis of Rapid and Recent Climatic Change (ARRCC) project.

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Supplementary Information accompanies the paper on **www.nature.com/nature.**

Acknowledgements We thank S. You, J. Blankenship and R. Balambika for discussions. We acknowledge financial support from the NIH, The Skaggs Institute for Chemical Biology, the Lita Annenberg Hazen Foundation and the Norton B. Gilula Fellowship (to S.D.). M.G. and H.N. were supported by a grant from the NSF. P.E.D. was supported by the NIH and the Alfred P. Sloan Foundation.

Competing interests statement The authors declare that they have no competing financial interests.

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corrigendum

Global-scale temperature patterns and climate forcing over the past six centuries

Michael E. Mann, Raymond S. Bradley & Malcolm K. Hughes

Nature 392, 779–787 (1998).

It has been drawn to our attention (S. McIntyre and R. McKitrick) that the listing of the 'proxy' data set in the Supplementary Information published with this Article contained several errors. In Table 1 we provide a list of the records that were either mistakenly included in the Supplementary Information, or mistakenly left out. A small number of other corrections of the original listing include (see Table 1) corrections of the citations originally provided, or corrections of the start years for certain series.

The full, corrected listing of the data is supplied as Supplementary Information to this corrigendum. Also provided as Supplementary Information are a documented archive of the complete data (instrumental and 'proxy' climate series) used in our original study, and an expanded description of the methodological details of our original study.

None of these errors affect our previously published results[1]. □

Table 1 **Errors in 'proxy' data set listing in ref. 1**

Series (34) listed in original Supplementary Information but not used in ref. 1*
FRAN003; ITAL015 and ITAL015X; SPAI026 and SPAI047; NEWZ056; ARGE030, ARGE060 and ARGE065; CHIL015, CHIL016, CHIL017 and CHIL018; AK006 and AK006X; CA070; CANA053, CANA053X, CANA096, CANA096X, CANA099, CANA106 and CANA110; WA019, WA025, WA027, WA033, WA039, WA041, WA071, WA074, WA086, WA088 and WA091; VAGANOV55
Series (2) used in ref. 1 but not listed in original Supplementary Information
Unpublished Southwest US/Mexico Density series (D. W. Stahle, personal communication)
Unpublished Southwest US/Mexico Latewood Width series (D. W. Stahle, personal communication)
Additional minor corrections
(1) The Central England and Central European temperature records used by ref. 1 were the summer season versions of these series as used by ref. 2.
(2) The 'long instrumental' series used in ref. 1 are station temperature and precipitation station data from the NOAA Climate Data centre gridded at 5° latitude/longitude resolution.
(3) The start year for the 'Central Europe' series of ref. 1 is AD 1525.
(4) The 'Western North America Dendro density' series used in ref. 1 should properly be attributed to ref. 3.
(5) The Stahle *et al.* Southwestern/Mexico late wood width and maximum density data used in ref. 1 should properly be attributed to ref. 4 (the formal reference was not available at the time of ref. 1), or, in two cases, unpublished data (D. W. Stahle, personal communication).
(6) For one of the 12 'Northern Treeline' records of Jacoby *et al.* used in ref. 1 (the 'St Anne River' series), the values used for AD 1400–03 were equal to the value for the first available year (AD 1404).

*These series, all of which come from the International Tree Ring Data Bank (ITRDB), met all the tests used for screening of the ITRDB data used in ref. 1 (see ref. 5), except one—namely, that in 1997, either it could not be ascertained by the authors how these series had been standardized by the original contributors, or it was known that the series had been aggressively standardized, removing multidecadal to century-scale fluctuations.

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Supplementary Information accompanies this corrigendum on **www.nature.com/nature.**

GEOPHYSICAL RESEARCH LETTERS, VOL. 26, NO.6, PAGES 759-762, MARCH 15, 1999

Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations

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Abstract. Building on recent studies, we attempt hemispheric temperature reconstructions with proxy data networks for the past millennium. We focus not just on the reconstructions, but the uncertainties therein, and important caveats. Though expanded uncertainties prevent decisive conclusions for the period prior to AD 1400, our results suggest that the latter 20th century is anomalous in the context of at least the past millennium. The 1990s was the warmest decade, and 1998 the warmest year, at moderately high levels of confidence. The 20th century warming counters a millennial-scale cooling trend which is consistent with long-term astronomical forcing.

Introduction

Estimates of climate variability during past centuries must rely upon indirect "proxy" indicators–natural archives that record past climate variations. Trends over several centuries are evident in the recession of glaciers [Grove and Switsur, 1994], and the sub-surface information from boreholes [Pollack et al, 1998]. Annual climate estimates, however, require proxies such as tree rings, varved sediments, ice cores, and corals (combined with any available instrumental or historical records), which record seasonal/annual variations. Studies based on such "multiproxy" data networks [e.g., Bradley and Jones, 1993; Hughes and Diaz, 1994; Mann et al, 1995] have allowed the 20th century climate to be placed in a longer-term perspective, thus allowing for improved estimates of the influence of climate forcings [Lean et al, 1995; Crowley and Kim, 1996; Overpeck et al, 1997], and validation of the low-frequency behavior exhibited by climate models [e.g., Jones et al, 1998].

Recently, Mann et al [1998–henceforth "MBH98"] reconstructed yearly global surface temperature patterns back in time through the calibration of multiproxy networks against the modern temperature record. Skillful reconstruction of Northern Hemisphere mean annual surface temperature ("NH") was possible back to AD 1400, as the pattern of surface temperature most readily calibrated by the available multiproxy network corresponds largely to synchronous large-scale temperature variation. It has been speculated that temperatures were warmer even further back, ~1000 years ago–a period described by Lamb [1965] as the Medieval Warm Epoch (though Lamb, examining evidence mostly from western Europe, never suggested this was a global phenomenon). We here apply the methodology detailed by MBH98 to the sparser proxy data network available prior to AD 1400, to critically revisit this issue, extending NH reconstructions as far back as is currently feasible. We also reevaluate earlier estimates of uncertainties in the NH series.

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Paper number 1999GL900070.
0094-8276/99/1999GL900070$05.00

Data and Method

The multiproxy data network and instrumental temperature data used to calibrate it are discussed in detail by MBH98 (see supplementary information therein). Before AD 1400, only 12 indicators of the more than 100 described by MBH98 are available. This includes the first 3 principal components (PCs) of the (28) dendroclimatic series available back to AD 1000 in the International Tree Ring Data Bank ("ITRDB")–all from North America. The 12 indicators (14 counting two nearby ice core sites) are summarized in Table 1.

The calibration procedure (see MBH98) invokes the assumptions (1) that a linear relationship exists between proxy climate indicators and and some combination of large-scale temperature patterns, and (2) that patterns of surface tem-

Table 1. 12 Proxy Indicators Available Back to AD 1000. Description ("SERIES"–see MBH98 for details regarding data and reference), location ("LOC"–region or lat/lon coordinates, start year ("y_0") AD, and type ("TYPE") of series is indicated. These data (and the NH series discussed in the text) are available over the internet through the World Data Center-A for Paleoclimatology (http://www.ngdc.noaa.gov/paleo/paleo.html).

SERIES	LOC	y_0	TYPE
ITRDB (PC #1)	N. Amer	1000	T. Ring width
ITRDB (PC #2)	N. Amer	1000	T. Ring width
ITRDB (PC #3)	N. Amer	1000	T. Ring width
Fennoscandia	68N 23E	500	T. Ring density
Polar Urals	67N 65E	914	T. Ring density
Tasmania	43S 148E	900	T. Ring width
N. Patagonia	38S 68W	869	T. Ring width
Morocco	33N 5W	984	T. Ring width
France	44N 7E	988	T. Ring width
Greenland stacked core	77N 60W	553	ice core $\delta^{18}O$
Quelccaya (2)	14S 71W	488	ice core $\delta^{18}O$
Quelccaya (2)	14S 71W	488	ice accum.

perature in the past can be suitably described in terms of some linear combination of the dominant present-day surface temperature patterns. MBH98 performed extensive cross-validation experiments to verify the reliability of the reconstruction using global temperature data from 1854-1901 withheld from (1902-1980) calibration, and, further back, by the small number of instrumental temperature series available back through the mid-18th century.

In using the sparser dataset available over the entire millennium (Table 1), only a relatively small number of indicators are available in regions (e.g., western North America) where the primary pattern of hemispheric mean temperature variation has significant amplitude (see Fig. 2 in MBH98), and where regional variations appear to be closely tied to global-scale temperature variations in model-based experiments [Bradley, 1996]. These few indicators thus take on a particularly important role (in fact, as discussed below, one such indicator– PC #1 of the ITRDB data–is found to be essential), in contrast with the post AD 1400 reconstructions of MBH98 for which indicators are available in several key regions [e.g., the North American northern treeline ("NT") dendroclimatic chronologies of Jacoby and D'Arrigo, 1989].

Due to the leverage of ITRDB PC #1 in the millennial reconstruction, any non-climatic influence must first be removed before it can meaningfully be used in the reconstructions. Spurious increases in variance back in time associated with decreasing sample sizes [see e.g. Jones et al, 1998] are not an issue with this series, owing to the high degree of replication in the underlying chronologies back to AD 1000. A number of the highest elevation chronologies in the western U.S. do appear, however, to have exhibited long-term growth increases that are more dramatic than can be explained by instrumental temperature trends in these regions. Graybill and Idso [1993] suggest that such high-elevation, CO_2-limited trees, in moisture-stressed environments, should exhibit a growth response to increasing CO_2 levels. Though ITRDB PC #1 shows significant loadings among many of the 28 constituent series, the largest are indeed found on high-elevation western U.S. sites. The ITRDB PC#1 is shown along with that of the composite NT series, during their 1400-1980 period of overlap (Figure 1). The low-frequency coherence of the ITRDB PC#1 series and composite NT series during the initial four centuries of overlap (1400-1800)is fairly remarkable, considering that the two series record variations in entirely different environments and regions. In the 19th century, however, the series diverge. As there is no *a priori* reason to expect the CO_2 effect discussed above to apply to the NT series, and, furthermore, that series has been verified through cross-comparison with a variety of proxy series in nearby regions [Overpeck et al, 1997], it is plausible that the divergence of the two series, is related to a CO_2 influence on the ITRDB PC #1 series. The residual is indeed coherent with rising atmospheric CO_2 (Figure 1b), until it levels off in the 20th century, which we speculate may represent a saturation effect whereby a new limiting factor is established at high CO_2 levels. For our purposes, however, it suffices that we consider the residual to be non-climatic in nature, and consider the ITRDB PC #1 series "corrected"' by removing from it this residual, forcing it to align with the NT series at low frequencies throughout their mutual interval of overlap. This correction is independently justified by the fact that temperatures averaged over the NT region and western U.S. region dominating ITRDB PC #1 exhibit very similar low-frequency trends this century (not shown).

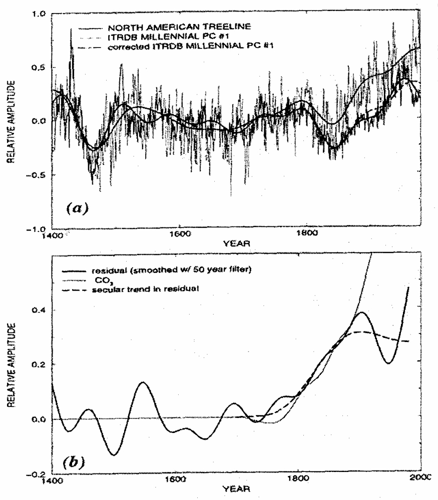

Figure 1. Comparison of ITRDB PC#1 and NT series. (a) composite NT series vs. ITRDB PC #1 series during AD 1400-1980 overlap. Thick curves indicate smoothed (75 year low-passed) versions of the series. The smoothed "corrected" ITRDB PC #1 series (see below) is shown for comparison, (b) Residual between the smoothed NT and ITRDB series, and its secular trend (retaining timescales longer than 150 years). Relative variations in atmospheric CO_2 since AD 1700 are shown for comparison.

Verification and Consistency Checks

The calibration/verification statistics for reconstructions based on the 12 indicators available back to AD 1000, are, as expected, somewhat degraded relative to those for the post AD 1400 period. The calibration and verification resolved variance (39% and 34% respectively) are consistent with each other, but lower than for reconstructions back to AD 1400 (42% and 51% respectively–see MBH98). Results further back than a millennium, based on even sparser data (see Table 1) are yet further degraded. With only a single eigenvector of the instrumental temperature data (#1– see Figure 2 in MBH98) skillfully resolved by the network available back to AD 1000, the total *spatial* variance calibrated is far more modest than that for the NH mean ($\approx$ 5% in calibration and verification). Thus, the NH series, but not the spatial details, are most meaningful in the millennial reconstructions.

Further consistency checks are required. The most basic involves checking the potential resolvability of long-term variations by the underlying data used. An indicator of climate variability should exhibit, at a minimum, the red noise

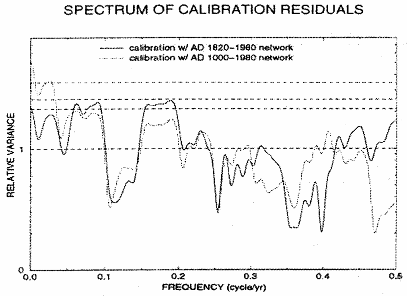

Figure 2. Spectrum of NH series calibration residuals from 1902-1980 for post-AD 1820 (solid) and AD 1000 (dotted) reconstructions (scaled by their mean white noise levels). Median and 90%,95%,and 99% significance levels (dashed lines) are shown.

spectrum the climate itself is known to exhibit [see Mann and Lees, 1996 and references therein]. A significant deficit of power relative to the median red noise level thus indicates a possible loss of true climatic variance, with a deficit of zero frequency power indicative of less trend than expected from noise alone, and the likelihood that the longest ("secular") timescales under investigation are not adequately resolved. Only 5 of the indicators (including the ITRDB PC #1, Polar Urals, Fennoscandia, and both Quelccaya series) are observed to have at least median red noise power at zero frequency for the pre-calibration (AD 1000-1901) period. It is furthermore found that only one of these series–PC #1 of the ITRDB data–exhibits a significant correlation with the time history of the dominant temperature pattern of the 1902-1980 calibration period. Positive calibration/variance scores for the NH series cannot be obtained if this indicator is removed from the network of 12 (in contrast with post-AD 1400 reconstructions for which a variety of indicators are available which correlate against the instrumental record). Though, as discussed earlier, ITRDB PC#1 represents a vital region for resolving hemispheric temperature trends, the assumption that this relationship holds up over time nonetheless demands circumspection. Clearly, a more widespread network of quality millennial proxy climate indicators will be required for more confident inferences.

A further consistency check involves examining the calibration residuals. In Figure 2 we show the power spectrum of the residuals of the NH calibration from 1902-1980 for both the calibrations based on all indicators in the network available back to 1820 (see MBH98), and the calibrations based on the 12 indicators available back to AD 1000. Not only (as indicated earlier) is the calibrated variance lower for the millennial reconstruction, but there is evidence of possible bias. While the residuals for the post-AD 1820 reconstructions are consistent with white noise (at no frequency does the spectrum of the residuals breach the 95% significance level for white noise–this holds in fact back to AD 1600), a roughly five-fold increase in unresolved variance is observed at secular frequencies (>99% significant) for the millennial reconstruction. In contrast to MBH98 where uncertainties were self-consistently estimated based on the observation of Gaussian residuals, we here take account of the spectrum of unresolved variance, separately treating unresolved components of variance in the secular (longer than the 79 year calibration interval in this case) and higher-frequency bands. To be conservative, we take into account the slight, though statistically insignificant inflation of unresolved secular variance for the post-AD 1600 reconstructions. This procedure yields composite uncertainties that are moderately larger than those estimated by MBH98, though none of the primary conclusions therein are altered.

Temperature Reconstruction

The reconstructed NH series and estimated uncertainties are shown in Figure 3, along with its associated power spectrum. The substantial secular spectral peak is highly sig-

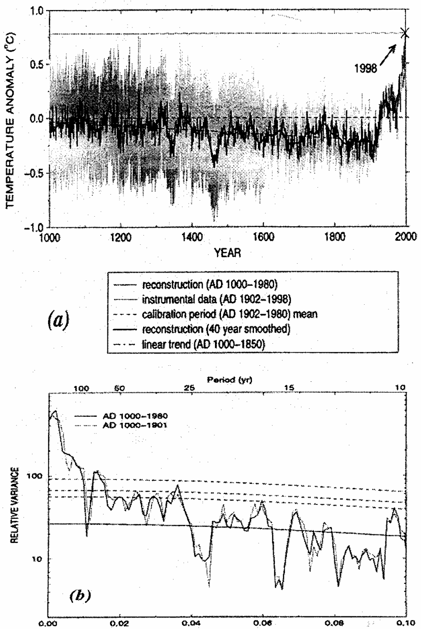

Figure 3. Millennial temperature reconstruction. (a) NH reconstruction (solid) and raw data (dotted) from AD 1000-1998. Smoothed version of NH series(thick solid), linear trend from AD 1000-1850 (dot-dashed) and two standard error limits (shaded) are also shown. (b) Power spectrum of the NH series based on full (AD 1000-1980) and pre-calibration (AD 1000-1901) intervals. Robustly estimated median and 90%, 95%, and 99% significance levels relative to red noise are shown [see Mann and Lees, 1996].

nificant relative to red noise, associated with a long-term cooling trend in the NH series prior to industrialization ($\delta T = -0.02^o$C/century). This cooling is possibly related to astronomical forcing, which is thought to have driven long-term temperatures downward since the mid-Holocene at a rate within the range of -0.01 to -0.04^{o}C/century [see Berger, 1988]. In addition, significant century-scale variability may be associated with solar irradiance variations [see Lean et al, 1995; MBH98], and a robust spectral peak centered at 50-70 year period seems to correspond to a multidecadal climate signal discussed by Mann et al [1995].

The 20th century (1900-1998) (anomaly of $\overline{T} = 0.07^o$C relative to the 1902-1980 calibration period mean) is nominally the warmest of the millennium (11-12th: -0.04; 13th: -0.09, 14th: -0.07; 15th: -0.19; 16th: -0.14; 17th: -0.18; 18th : -0.14; 19th:-0.21). Expanded uncertainties in centennial means prior to AD 1600, and warmer conditions during the earlier centuries of the millennium, however, preclude a definitive statement prior to AD 1400–the 11th and 12th centuries are within a (centennial) standard error of the 20th century. The late 11th, late 12th, and late 14th centuries rival *mean* 20th century temperature levels (see Figure 3a). Our reconstruction thus supports the notion of relatively warm hemispheric conditions earlier in the millennium, while cooling following the 14th century could be viewed as the initial onset of the Little Ice Age *sensu lato*. Considerable spatial variability is evident however [see Hughes and Diaz, 1994] and, as in in Lamb's [1965] original concept of a Medieval Warm Epoch, there are episodes of cooler as well as warmer conditions punctuating this period. Even the warmer intervals in our reconstruction pale, however, in comparison with *modern* (mid-to-late 20th century) temperatures. For the NH series, both the past year (1998) and past decade (1989-1998) are well documented as the warmest in the 20th century instrumental record. Furthermore, the past decade ($\overline{T} = 0.45^o$C) is nearly two (decadal) standard errors warmer than the next warmest decade prior to the 20th century (1166-1175: $\overline{T}$=0.11), and 1998 ($T = 0.78^o$C) more than two standard errors warmer than the next warmest year (1249 with an anomaly $T = 0.27^o$C; 1253 and 1366 with $T \approx 0.25^o$C are the only other two years approaching typical modern warmth), supporting the conclusion that both the past decade and past year are likely the warmest for the Northern Hemisphere *this millennium*. The recent warming is especially striking if viewed as defying a long-term cooling trend associated with astronomical forcing.

Conclusions

Although NH reconstructions prior to about AD 1400 exhibit expanded uncertainties, several important conclusions are still possible. While warmth early in the millennium approaches *mean* 20th century levels, the late 20th century still appears anomalous: the 1990s are likely the warmest decade, and 1998 the warmest year, in at least a millennium. More widespread high-resolution data which can resolve millennial-scale variability are needed before more confident conclusions can be reached with regard to the spatial and temporal details of climate change in the past millennium and beyond.

Acknowledgments. We thank P.D. Jones and two anonymous reviewers for their comments. We gratefully acknowledge the numerous researchers who have contributed to the ITRDB. This research was supported by grants from the NSF (ATM-9626833) and DOE. M.E.M. acknowledges support through the Alexander Hollaender Distinguished Postdoctoral Fellowship Program (DOE).

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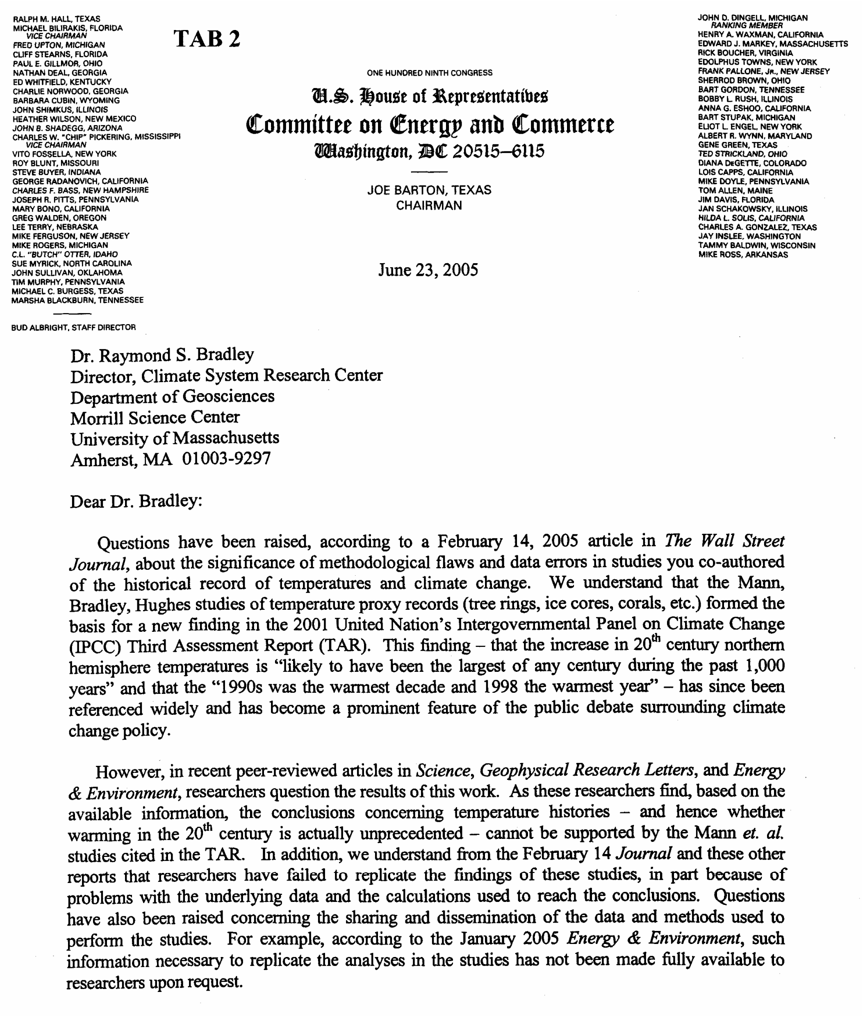
M.E. Mann, Department of Geosciences, Morrill Science Building, University of Massachusetts, Amherst MA 01003-5820.

(Received October 14, 1998; revised January 21, 1999; accepted January 27, 1999.)



TAB 2

RALPH M. HALL, TEXAS
MICHAEL BILIRAKIS, FLORIDA
VICE CHAIRMAN
FRED UPTON, MICHIGAN
CLIFF STEARNS, FLORIDA
PAUL E. GILLMOR, OHIO
NATHAN DEAL, GEORGIA
ED WHITFIELD, KENTUCKY
CHARLIE NORWOOD, GEORGIA
BARBARA CUBIN, WYOMING
JOHN SHIMKUS, ILLINOIS
HEATHER WILSON, NEW MEXICO
JOHN B. SHADEGG, ARIZONA
CHARLES W. "CHIP" PICKERING, MISSISSIPPI
VICE CHAIRMAN
VITO FOSSELLA, NEW YORK
ROY BLUNT, MISSOURI
STEVE BUYER, INDIANA
GEORGE RADANOVICH, CALIFORNIA
CHARLES F. BASS, NEW HAMPSHIRE
JOSEPH R. PITTS, PENNSYLVANIA
MARY BONO, CALIFORNIA
GREG WALDEN, OREGON
LEE TERRY, NEBRASKA
MIKE FERGUSON, NEW JERSEY
MIKE ROGERS, MICHIGAN
C.L. "BUTCH" OTTER, IDAHO
SUE MYRICK, NORTH CAROLINA
JOHN SULLIVAN, OKLAHOMA
TIM MURPHY, PENNSYLVANIA
MICHAEL C. BURGESS, TEXAS
MARSHA BLACKBURN, TENNESSEE

BUD ALBRIGHT, STAFF DIRECTOR

ONE HUNDRED NINTH CONGRESS

U.S. House of Representatives
Committee on Energy and Commerce
Washington, DC 20515–6115

JOE BARTON, TEXAS
CHAIRMAN

JOHN D. DINGELL, MICHIGAN
RANKING MEMBER
HENRY A. WAXMAN, CALIFORNIA
EDWARD J. MARKEY, MASSACHUSETTS
RICK BOUCHER, VIRGINIA
EDOLPHUS TOWNS, NEW YORK
FRANK PALLONE, JR., NEW JERSEY
SHERROD BROWN, OHIO
BART GORDON, TENNESSEE
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ELIOT L. ENGEL, NEW YORK
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LOIS CAPPS, CALIFORNIA
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TOM ALLEN, MAINE
JIM DAVIS, FLORIDA
JAN SCHAKOWSKY, ILLINOIS
HILDA L. SOLIS, CALIFORNIA
CHARLES A. GONZALEZ, TEXAS
JAY INSLEE, WASHINGTON
TAMMY BALDWIN, WISCONSIN
MIKE ROSS, ARKANSAS

June 23, 2005

Dr. Raymond S. Bradley
Director, Climate System Research Center
Department of Geosciences
Morrill Science Center
University of Massachusetts
Amherst, MA 01003-9297

Dear Dr. Bradley:

Questions have been raised, according to a February 14, 2005 article in *The Wall Street Journal*, about the significance of methodological flaws and data errors in studies you co-authored of the historical record of temperatures and climate change. We understand that the Mann, Bradley, Hughes studies of temperature proxy records (tree rings, ice cores, corals, etc.) formed the basis for a new finding in the 2001 United Nation's Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report (TAR). This finding – that the increase in 20th century northern hemisphere temperatures is "likely to have been the largest of any century during the past 1,000 years" and that the "1990s was the warmest decade and 1998 the warmest year" – has since been referenced widely and has become a prominent feature of the public debate surrounding climate change policy.

However, in recent peer-reviewed articles in *Science*, *Geophysical Research Letters*, and *Energy & Environment*, researchers question the results of this work. As these researchers find, based on the available information, the conclusions concerning temperature histories – and hence whether warming in the 20th century is actually unprecedented – cannot be supported by the Mann *et. al.* studies cited in the TAR. In addition, we understand from the February 14 *Journal* and these other reports that researchers have failed to replicate the findings of these studies, in part because of problems with the underlying data and the calculations used to reach the conclusions. Questions have also been raised concerning the sharing and dissemination of the data and methods used to perform the studies. For example, according to the January 2005 *Energy & Environment*, such information necessary to replicate the analyses in the studies has not been made fully available to researchers upon request.

The concerns surrounding these studies reflect upon the quality and transparency of federally funded research and of the IPCC review process – two matters of particular interest to the Committee. For example, one concern relates to whether IPCC review has been sufficiently independent. We understand that you were a contributing author of the IPCC chapter that assessed and reported your own studies, and that two study co-authors were also contributors to this very same chapter. Given the prominence these studies were accorded in the IPCC TAR and your position and role in that process, we seek to learn more about the facts and circumstances that led to acceptance and prominent use of this work in the IPCC TAR and to understand what this controversy indicates about the data quality of key IPCC studies.

As you know, sharing data and research results is a basic tenet of open scientific inquiry, providing a means to judge the reliability of scientific claims. The ability to replicate a study, as the National Research Council has noted, is typically the gold standard by which the reliability of claims is judged. Given the questions reported about data access surrounding these studies, we also seek to learn whether obligations concerning the sharing of information developed or disseminated with federal support have been appropriately met.

In light of the Committee's jurisdiction over energy policy and certain environmental issues, the Committee must have full and accurate information when considering matters relating to climate change policy. We open this review because this dispute surrounding your studies bears directly on important questions about the federally funded work upon which climate studies rely and the quality and transparency of analyses used to support the IPCC assessment process. With the IPCC currently working to produce a fourth assessment report, addressing questions of quality and transparency in the process and underlying analyses supporting that assessment, both scientific and economic, are of utmost importance if Congress is eventually going to make policy decisions drawing from this work.

To assist us as we begin this review, and pursuant to Rules X and XI of the U.S. House of Representatives, please provide the following information requested below on or before July 11, 2005:

1. Your *curriculum vitae*, including, but not limited to, a list of all studies relating to climate change research for which you were an author or co-author and the source of funding for those studies.

2. List all financial support you have received related to your research, including, but not limited to, all private, state, and federal assistance, grants, contracts (including subgrants or subcontracts), or other financial awards or honoraria.

3. Regarding all such work involving federal grants or funding support under which you were a recipient of funding or principal investigator, provide all agreements relating to those underlying grants or funding, including, but not limited to, any provisions, adjustments, or exceptions made in the agreements relating to the dissemination and sharing of research results.

4. Provide the location of all data archives relating to each published study for which you were an author or co-author and indicate: (a) whether this information contains all the specific data you used and calculations your performed, including such supporting documentation as computer source code, validation information, and other ancillary information, necessary for full evaluation and application of the data, particularly for another party to replicate your research results; (b) when this information was available to researchers; (c) where and when you first identified the location of this information; (d) what modifications, if any, you have made to this information since publication of the respective study; and (e) if necessary information is not fully available, provide a detailed narrative description of the steps somebody must take to acquire the necessary information to replicate your study results or assess the quality of the proxy data you used.

5. Regarding study data and related information that is not publicly archived, what requests have you or your co-authors received for data relating to the climate change studies, what was your response, and why?

6. The authors McIntyre and McKitrick (*Energy & Environment*, Vol. 16, No. 1, 2005) report a number of errors and omissions in Mann *et. al.*, 1998. Provide a detailed narrative explanation of these alleged errors and how these may affect the underlying conclusions of the work, including, but not limited to answers to the following questions:
    a. Did you run calculations without the bristlecone pine series referenced in the article and, if so, what was the result?
    b. Did you or your co-authors calculate temperature reconstructions using the referenced "archived Gaspe tree ring data," and what were the results?
    c. Did you calculate the R2 statistic for the temperature reconstruction, particularly for the 15th Century proxy record calculations and what were the results?
    d. What validation statistics did you calculate for the reconstruction prior to 1820, and what were the results?
    e. How did you choose particular proxies and proxy series?

7. Explain in detail your work for and on behalf of the Intergovernmental Panel on Climate Change, including, but not limited to: (a) your role in the Third Assessment Report; (b) the process for review of studies and other information, including the dates of key meetings, upon which you worked during the TAR writing and review process; (c) the steps taken by you, reviewers, and lead authors to ensure the data underlying the studies forming the basis for key findings of the report were sound and accurate; (d) requests you received for revisions to your written contribution; and (e) the identity of the people who wrote and reviewed the historical temperature-record portions of the report, particularly Section 2.3, "Is the Recent Warming Unusual?"

Thank you for your assistance. If you have any questions, please contact Peter Spencer of the Majority Committee staff at (202) 226-2424.

Sincerely,

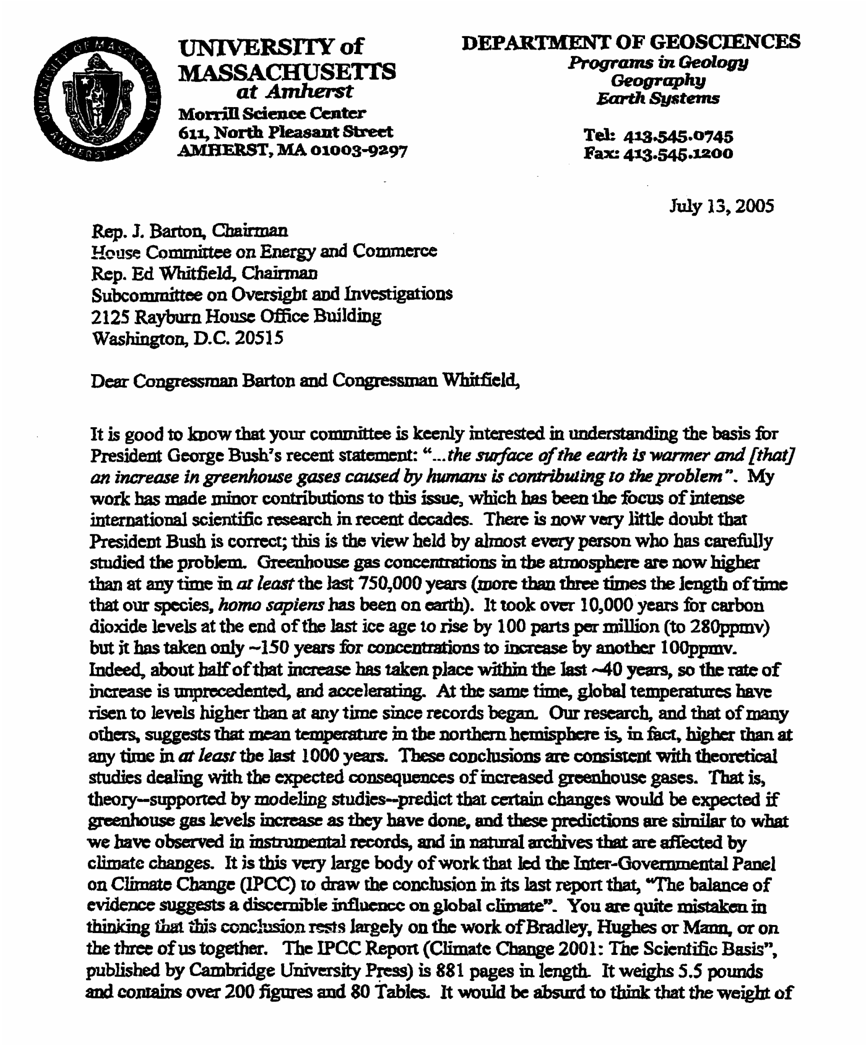
Joe Barton

Joe Barton
Chairman

Ed Whitfield

Ed Whitfield
Chairman
Subcommittee on Oversight
and Investigations

cc: The Honorable John Dingell, Ranking Member
The Honorable Bart Stupak, Ranking Member,
Subcommittee on Oversight and Investigations



UNIVERSITY of MASSACHUSETTS
at Amherst
Morrill Science Center
611, North Pleasant Street
AMHERST, MA 01003-9297

DEPARTMENT OF GEOSCIENCES
Programs in Geology
Geography
Earth Systems

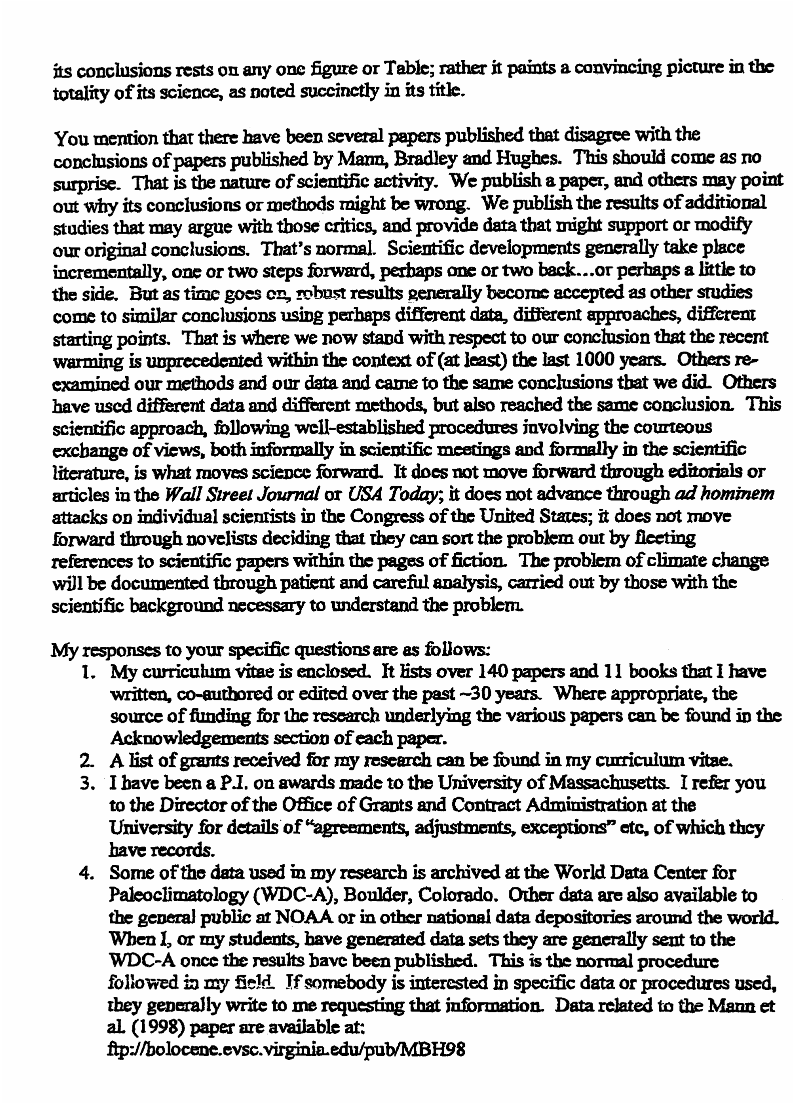
Tel: 413.545.0745
Fax: 413.545.1200

July 13, 2005

Rep. J. Barton, Chairman
House Committee on Energy and Commerce
Rep. Ed Whitfield, Chairman
Subcommittee on Oversight and Investigations
2125 Rayburn House Office Building
Washington, D.C. 20515

Dear Congressman Barton and Congressman Whitfield,

It is good to know that your committee is keenly interested in understanding the basis for President George Bush's recent statement: "*...the surface of the earth is warmer and [that] an increase in greenhouse gases caused by humans is contributing to the problem*". My work has made minor contributions to this issue, which has been the focus of intense international scientific research in recent decades. There is now very little doubt that President Bush is correct; this is the view held by almost every person who has carefully studied the problem. Greenhouse gas concentrations in the atmosphere are now higher than at any time in *at least* the last 750,000 years (more than three times the length of time that our species, *homo sapiens* has been on earth). It took over 10,000 years for carbon dioxide levels at the end of the last ice age to rise by 100 parts per million (to 280ppmv) but it has taken only ~150 years for concentrations to increase by another 100ppmv. Indeed, about half of that increase has taken place within the last ~40 years, so the rate of increase is unprecedented, and accelerating. At the same time, global temperatures have risen to levels higher than at any time since records began. Our research, and that of many others, suggests that mean temperature in the northern hemisphere is, in fact, higher than at any time in *at least* the last 1000 years. These conclusions are consistent with theoretical studies dealing with the expected consequences of increased greenhouse gases. That is, theory--supported by modeling studies--predict that certain changes would be expected if greenhouse gas levels increase as they have done, and these predictions are similar to what we have observed in instrumental records, and in natural archives that are affected by climate changes. It is this very large body of work that led the Inter-Governmental Panel on Climate Change (IPCC) to draw the conclusion in its last report that, "The balance of evidence suggests a discernible influence on global climate". You are quite mistaken in thinking that this conclusion rests largely on the work of Bradley, Hughes or Mann, or on the three of us together. The IPCC Report (Climate Change 2001: The Scientific Basis", published by Cambridge University Press) is 881 pages in length. It weighs 5.5 pounds and contains over 200 figures and 80 Tables. It would be absurd to think that the weight of



its conclusions rests on any one figure or Table; rather it paints a convincing picture in the totality of its science, as noted succinctly in its title.

You mention that there have been several papers published that disagree with the conclusions of papers published by Mann, Bradley and Hughes. This should come as no surprise. That is the nature of scientific activity. We publish a paper, and others may point out why its conclusions or methods might be wrong. We publish the results of additional studies that may argue with those critics, and provide data that might support or modify our original conclusions. That's normal. Scientific developments generally take place incrementally, one or two steps forward, perhaps one or two back...or perhaps a little to the side. But as time goes on, robust results generally become accepted as other studies come to similar conclusions using perhaps different data, different approaches, different starting points. That is where we now stand with respect to our conclusion that the recent warming is unprecedented within the context of (at least) the last 1000 years. Others re-examined our methods and our data and came to the same conclusions that we did. Others have used different data and different methods, but also reached the same conclusion. This scientific approach, following well-established procedures involving the courteous exchange of views, both informally in scientific meetings and formally in the scientific literature, is what moves science forward. It does not move forward through editorials or articles in the *Wall Street Journal* or *USA Today*; it does not advance through *ad hominem* attacks on individual scientists in the Congress of the United States; it does not move forward through novelists deciding that they can sort the problem out by fleeting references to scientific papers within the pages of fiction. The problem of climate change will be documented through patient and careful analysis, carried out by those with the scientific background necessary to understand the problem.

My responses to your specific questions are as follows:

1. My curriculum vitae is enclosed. It lists over 140 papers and 11 books that I have written, co-authored or edited over the past ~30 years. Where appropriate, the source of funding for the research underlying the various papers can be found in the Acknowledgements section of each paper.
2. A list of grants received for my research can be found in my curriculum vitae.
3. I have been a P.I. on awards made to the University of Massachusetts. I refer you to the Director of the Office of Grants and Contract Administration at the University for details of "agreements, adjustments, exceptions" etc, of which they have records.
4. Some of the data used in my research is archived at the World Data Center for Paleoclimatology (WDC-A), Boulder, Colorado. Other data are also available to the general public at NOAA or in other national data depositories around the world. When I, or my students, have generated data sets they are generally sent to the WDC-A once the results have been published. This is the normal procedure followed in my field. If somebody is interested in specific data or procedures used, they generally write to me requesting that information. Data related to the Mann et al. (1998) paper are available at:
   ftp://holocene.evsc.virginia.edu/pub/MBH98

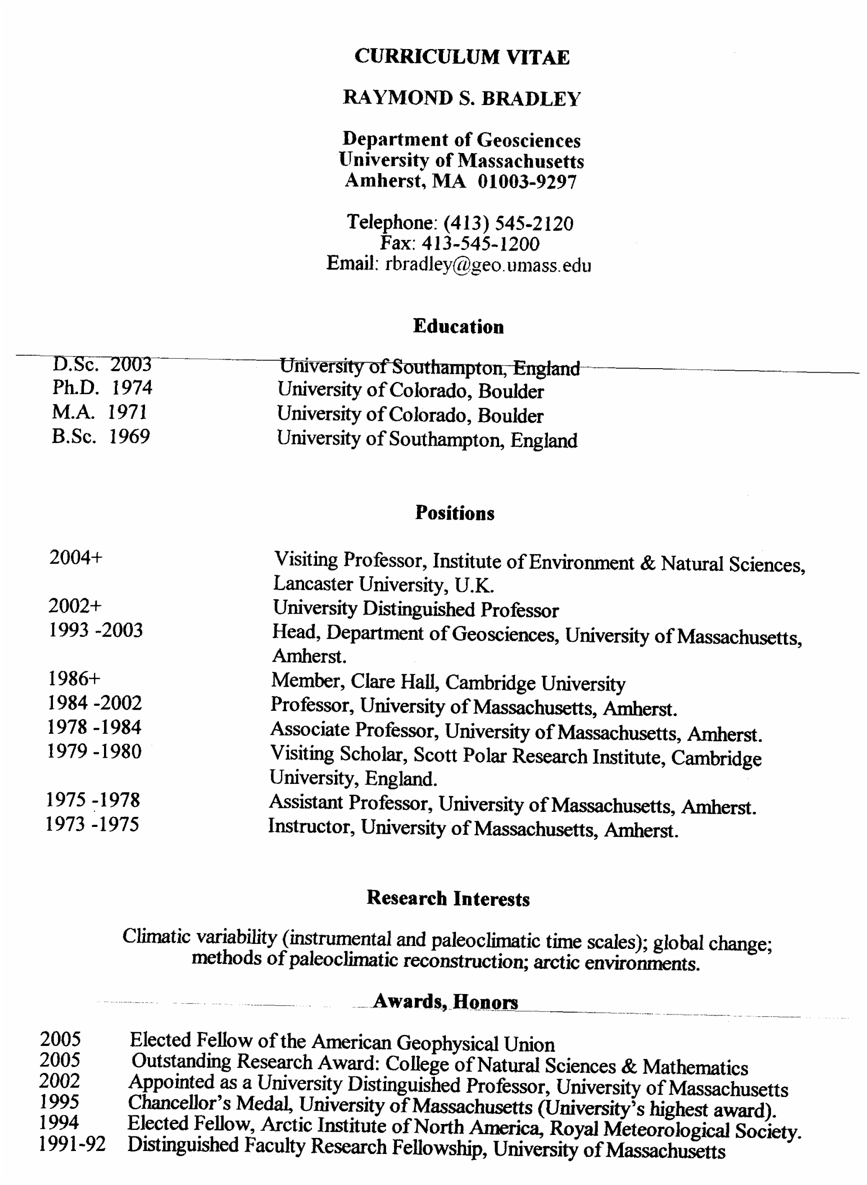
5. I get somewhere between 30,000 and 35,000 emails a year. These include many inquiries about my research ranging from schoolchildren doing projects, to college students and scientists carrying out their own research, to religious fundamentalists who wish to convince me that the end is nigh. I do not have time to respond to all requests but try to respond to as many inquiries as I can.
6. McIntyre and McKitrick have criticized our work, claiming to have "audited" and "corrected" what we did. In fact, they did neither. Had their article been subjected to an appropriate scientific review, it is unlikely that it would have been published. They then submitted their criticisms to *Nature*; we responded to these through extensive correspondence with *Nature*. After reviewing their claims, *Nature* chose not to publish them. They then submitted essentially the same criticisms to *Geophysical Research Letters*. The editors of this journal made an error by not requesting comments on the article from us (as *Nature* did). This is the normal procedure when the work of any author is directly criticized; then (if recommended by reviewers) both the criticism and the response are jointly published for all to judge who is right; this procedure was not followed. If it had been, once again I believe the criticisms of McIntyre and McKitrick would probably not have been published. There is a very good guide to the issues involved at this web site: http://www.realclimate.org/index.php?p=121
This shows why their criticisms are erroneous and irrelevant to our basic conclusion that the recent warming is unprecedented in the context of the last 1000 years.
7. I had a very minor role in the last IPCC assessment, limited to reading draft sections of Chapter 2 and providing comments.

Let me conclude by pointing out that the paper which seems to be the focus of so much of your attention (Mann et al, 1999) was entitled, *Northern Hemisphere temperatures during the past millennium: inferences, uncertainties, and limitations*. In fact, a major point of the paper—which both you and others seem to have overlooked—is that we were at pains in this paper to point out the difficulties of drawing conclusions about the climate of the past millennium. We recognize and estimate the uncertainties involved in such paleoclimatic reconstructions. If others choose to ignore those caveats, there's not much we can do about it. Nevertheless, the estimates that we provided have proven to be quite robust and the "working hypothesis" that we presented is now quite well supported by numerous other studies.

Sincerely,

Raymond S. Bradley

Raymond S. Bradley
University Distinguished Professor



CURRICULUM VITAE

RAYMOND S. BRADLEY

Department of Geosciences
University of Massachusetts
Amherst, MA 01003-9297

Telephone: (413) 545-2120
Fax: 413-545-1200
Email: rbradley@geo.umass.edu

Education

~~D.Sc. 2003~~	~~University of Southampton, England~~
Ph.D. 1974	University of Colorado, Boulder
M.A. 1971	University of Colorado, Boulder
B.Sc. 1969	University of Southampton, England

Positions

2004+	Visiting Professor, Institute of Environment & Natural Sciences, Lancaster University, U.K.
2002+	University Distinguished Professor
1993 -2003	Head, Department of Geosciences, University of Massachusetts, Amherst.
1986+	Member, Clare Hall, Cambridge University
1984 -2002	Professor, University of Massachusetts, Amherst.
1978 -1984	Associate Professor, University of Massachusetts, Amherst.
1979 -1980	Visiting Scholar, Scott Polar Research Institute, Cambridge University, England.
1975 -1978	Assistant Professor, University of Massachusetts, Amherst.
1973 -1975	Instructor, University of Massachusetts, Amherst.

Research Interests

Climatic variability (instrumental and paleoclimatic time scales); global change; methods of paleoclimatic reconstruction; arctic environments.

Awards, Honors

2005	Elected Fellow of the American Geophysical Union
2005	Outstanding Research Award: College of Natural Sciences & Mathematics
2002	Appointed as a University Distinguished Professor, University of Massachusetts
1995	Chancellor's Medal, University of Massachusetts (University's highest award).
1994	Elected Fellow, Arctic Institute of North America, Royal Meteorological Society.
1991-92	Distinguished Faculty Research Fellowship, University of Massachusetts

Appointments: 1984-2005

2003+	Chairman, NOAA Paleoclimate Advisory Group
2003+	Mountain Research Initiative, Bern, Switzerland: Scientific Advisory Board
2002+	Max-Planck Institute for Meteorology (Hamburg) Advisory Council
2000+	NOAA Climate Change Data & Detection (C^2D^2) Advisory Panel
2000+	Editorial Advisory Boards:, *Boreas, Polar Geography, J. Quaternary Science, J. Paleolimnology, The Holocene, Key Issues in Environmental Change (Arnold), Advances in Global Change Research (Kluwer), Developments in Paleoenvironmental Research (Kluwer);* Series Editor*: Blackwell Quaternary Geoscience Series*
2002-2004	Associate Editor, *J. Geophysical Research (Atmospheres)*
2000-2004	Arctic Research Consortium of the United States (ARCUS) Board of Directors
2000-2004	Swiss "National Centers for Competence in Research-Climate" Advisory Board
1990-1998	IGBP PAGES Scientific Steering Committee and Executive Committee Member (SSC Chairman, 1996+); Member of IGBP SSC, 1996+.
1994-1998	National Research Council: Panel on Decade-to-Century-Scale Climate Variability.
1994-1998	NSF Earth System History Advisory Panel.
1994-1996	U.S. Project Leader, Paleoclimatology, Working Group 8: U.S./Russia Convention on Protection of the Environment.
1990-1993	NOAA Climate and Global Change Advisory Panel.
1991	Evaluation committee: Swedish National Research Council: National research in physical geography
1988-1994	Secretary, INQUA Paleoclimate Commission
1989-90, 1994-95	Member of the Inter-Governmental Panel on Climate Change (I.P.C.C.)
1992-3	PALE (Paleoclimate of Arctic Lakes and Estuaries) Steering Committee
1988-1990	Chairman, National Geophysical Data Center (NOAA) Paleoclimate Advisory Panel
1989	U.S. delegate, Working Group 8: U.S./U.S.S.R. meeting on development of data sets for detecting climatic change, Obninsk, U.S.S.R.
1986 - 1989, 1994-96	American Quaternary Association: Council Member.
1986	U.S. delegate to Working Group 8, Project 11 of the U.S./U.S.S.R. Bilateral Agreement on Protection of the Environment "*Meeting of Experts on Causes of Recent Climatic Change*", Leningrad, U.S.S.R.
1983 - 1986	National Research Council, National Academy of Sciences: Committee on Monitoring and Assessment of Trends in Acid Deposition.
1984 - 1987	American Meteorological Society: Committee on Climatic Variations.

Field Experience

Summer 2005	Northern Ellesmere Island, Nunavut, Canada: paleoclimatic significance of laminated lake sediments (Murray Lake).
Summer 2002	Kilimanjaro, Tanzania: meteorology on the Northern Icefield.
Summer 2001	Lofoten Islands, northwestern Norway: lake sediment collection late Holocene paleoclimatic reconstruction.
Summer 2000	Northeastern Ellesmere Island: energy balance and topoclimatic studies of the Hazen Plateau (Murray and Simmonds Ice Caps).
Summers 1990 to 1999	Northern Ellesmere Island and Cornwallis Island, N.W.T. Canada: paleoclimatic significance of laminated lake sediments.
Fall 1987, 1988	Central China: chronology and paleoclimatic aspects of loess/paleosol sequences.
Summer 1988	Cornwallis Island, N.W.T., Canada: lake sediment collection from Sophia Lake for late Holocene paleoclimatic reconstruction.
Fall 1987	Czechoslovakia and Austria: chronology and paleoclimatology of loess/paleosol sequences.
Summer 1986	Northern Ellesmere Island: lake sediment collection for Holocene paleoclimatic reconstruction.
Winters, 1982, 1984, 1985	Venezuelan Andes: collection of lake sediments from high elevation basins.
Summers 1982, 1983	Northeastern Ellesmere Island and northwestern Greenland: energy balance and topoclimatic studies of Hazen Ice Cap
Summer 1978, 1981	Northeastern Ellesmere Island, N.W.T., Canada: paleoclimatology and glacial history of Beaufort Lakes area.
Summers 1975, 1976, 1978	Northeastern Ellesmere Island, N.W.T., Canada: glacial and climatic history of Archer Fiord/Judge Daly Promontory.
Summer 1970, 1972	Eastern Baffin Island, N.W.T., Canada: sea-ice energy budget and glacier mass balance study, east coast of Cumberland Peninsula.
Summer 1971	Northeastern Ellesmere Island, N.W.T., Canada: field assistant, glacial history of Hazen Plateau.

RESEARCH GRANTS

Period	**Agency**	**Title**	**Total Funds**	**Co-P.I.**
2005-2009	NSF	Collaborative research: a synthesis of the last 2000 years of climatic variability from Arctic lakes	$193,098	Francis
2004-2007	DOE	Regional-scale climate variability on decadal to multi-century timescales	$286,489	
2004-2007	NSF	High-Resolution Studies of High Arctic Paleoclimate from Varved Lake Sediments.	$425,147	
2003-2005	NOAA	A 19th century data catalog for New England and adjacent states	$194,001	
2003-2005	NOAA	Climatic conditions in East Africa and Arabia: implications for paleoclimatic interpretations	$236,055	Vuille
~~2002-2005~~	~~NOAA~~	~~Reconstruction and analysis of patterns of~~ climate variability over the last 1-2 millennnia	~~$113,434~~	~~Mann, Hughes~~
2001-2004	DOE	Regional-scale climate variability on decadal to century timescales	$272,229	
2001-2003	NSF	Lake sediment paleoclimate research in the Lofoten Islands, Arctic Norway	$60,019	Francus
2000-2002	NSF	Stable isotope variability in precipitation at low-latitude sites	$254,653	Vuille
2000-2001	U. Va	Multiproxy climate reconstruction	$33,889	
1999-2001	NSF	The Late Quaternary water balance of the Venezuelan Andes	$335,477	Abbott
1999-2002	NSF	Land-atmosphere-ice and snow interactions in the High Arctic	$538,727	Hardy
1998-2001	DOE	Validation of decadal to multi-century climate predictions	$450,000	Diaz
1997-2002	NSF	High resolution lake sediment studies for paleoclimatic reconstruction	$637,096	
1999-2000	NSF	Solar influences on cloud formation and global temperature	$55,335	
1996-1999	NSF	Patterns of interannual climate variability	$137,000	
1997-99	NSF	Precipitation variability in the Inter-Tropical Andes	$142,848	
1996-97	NASA	Meteorological studies, Windy Dome Ice Cap, Franz-Josef Land	$31,000	
1996-99	NOAA	Meteorological studies, Sajama Ice Cap, Bolivia	$170,007	
1996-97	N.S.F.	Workshop on climate variability in the Americas from high elevation ice cores	$49,937	
1995-98	D.O.E.	Climate system studies	$587,000	Diaz
1994-97	N.S.F.	Paleoclimate from Arctic lake sediments	$300,000	
1993-94	D.O.E.	Records of recent climatic change in northern Canada	$ 85,000	
1993-94	National Geog. Soc.	Climate of the last millennium from Arctic lake sediments	$ 18,500	
1991-96	N.S.F.	Analysis of rapid and recent climatic change (ARRCC)	$286,000	
1991-94	D.O.E.	Diagnostic studies of climate variability	$640,681	Diaz
1990-92	NOAA	Climatic fluctuations of Monsoon Asia since A.D. 1600	$ 71,600	Hughes, Thompson

		Monsoon Asia since A.D. 1600		Thompson
1990-93	N.S.F.	Paleoclimatic significance of laminated lake sediments from the Canadian High Arctic	$299,997	
1990-91	NOAA	Precipitation history of Massachusetts	$ 15,000	
1990-91	D.O.E.	Scientific workstation	$ 27,040	
1988-91	D.O.E.	Climate system research	$526,541	Diaz, Kelly
1987-91	N.S.F.	Chronology and paleoclimatology loess deposits in central China	$246,500	McCoy
1985-87	N.S.F.	Evolution of Arctic lacustrine environments	$162,000	Yuretich
1985-88	D.O.E.	Studies of climatic variability	$484,000	Diaz, Kelly, Jones
1983-86	N.S.F.	Late Quaternary climatic variations in the Venezuelan Andes	$101,000	Yuretich
1984-85	N.S.F.	Equipment for lake sediment studies	$ 50,000	
1981-84	D.O.E.	Climate of the Northern Hemisphere 1851 -1900 compared to contemporary climate	$408,550	Diaz, Jones ,Kelly
1981-83	N.S.F.	Climatic fluctuations of northernmost North America	$128,000	England
1977-79	N.S.F.	Secular climatic history of the arid and semi-arid western U.S.	$ 65,000	

INVITED/KEYNOTE TALKS (LAST 2 YEARS)

2005	Walter Orr Roberts Memorial Lecture, Aspen Global Change Institute, July 18 (invited)
2005	Taafe lecture; Department of Geography, Ohio State University (invited): Feb.17
2004	Institute of Geographical Sciences, Chinese Academy of Sciences, (invited) Beijing, China, Nov. 18
2004	International Conference on Environmental Processes of East Eurasia: Past, Present and Future (keynote), Xi'an, China, Nov. 15-17.
2004	European Science Foundation HOLIVAR (Holocene Climate Variability) Workshop (invited), Kastanienbaum, Switzerland, Sept. 23-25.
2004	Bjerknes Centenary Symposium on "Climate Change in High Latitudes" (keynote): Bergen, Norway; Sept. 1-3.
2004	Symposium on Mountain Climate Sciences (invited): Lake Tahoe, California, May 25-27.
2004	Conference on Tree Rings and Climate: Sharpening the Focus (keynote): Tucson, Arizona, April 6-9.
2003	GCOS Regional Workshop for South America, Santiago, Chile (invited) October 14-16.
2003	Conference on Global Change Research in Mountain Biosphere Reserves (invited), Entlebuch Biosphere Reserve, Switzerland, 10-13 November.
2003	IMAGES Holocene Workshop (invited); Hafslo, Norway, Aug 27-30.
2003	International Quaternary Association Congress (keynote): Reno, Nevada,
2003	American Meteorological Society conference on Polar Meteorology (keynote): Hyannisport, Massachusetts

PUBLICATIONS

A. BOOKS AND EDITED VOLUMES

Diaz, H.F. and R.S. Bradley (eds.), 2004. **The Hadley Circulation, Present, Past and Future.** Kluwer Academic, Dordrecht, 511pp.

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B. JOURNAL ARTICLES AND BOOK CHAPTERS

Bradley, R.S. and Miller, G.H., 1972: Recent climatic change and increased glacierization in the eastern Canadian Arctic. **Nature**, 237, 385-387.

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BUD ALBRIGHT, STAFF DIRECTOR

TAB 3

ONE HUNDRED NINTH CONGRESS
U.S. House of Representatives
Committee on Energy and Commerce
Washington, DC 20515–6115

JOE BARTON, TEXAS
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HILDA L. SOLIS, CALIFORNIA
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JAY INSLEE, WASHINGTON
TAMMY BALDWIN, WISCONSIN
MIKE ROSS, ARKANSAS

June 23, 2005

Dr. Malcolm K. Hughes
Professor
Laboratory of Tree-Ring Research
The University of Arizona
PO Box 210058
Tucson, AZ 85721

Dear Dr. Hughes:

Questions have been raised, according to a February 14, 2005 article in *The Wall Street Journal*, about the significance of methodological flaws and data errors in studies you co-authored of the historical record of temperatures and climate change. We understand that the Mann, Bradley, Hughes studies of temperature proxy records (tree rings, ice cores, corals, etc.) formed the basis for a new finding in the 2001 United Nation's Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report (TAR). This finding – that the increase in 20th century northern hemisphere temperatures is "likely to have been the largest of any century during the past 1,000 years" and that the "1990s was the warmest decade and 1998 the warmest year" – has since been referenced widely and has become a prominent feature of the public debate surrounding climate change policy.

However, in recent peer-reviewed articles in *Science*, *Geophysical Research Letters*, and *Energy & Environment*, researchers question the results of this work. As these researchers find, based on the available information, the conclusions concerning temperature histories – and hence whether warming in the 20th century is actually unprecedented – cannot be supported by the Mann *et. al.* studies cited in the TAR. In addition, we understand from the February 14 *Journal* and these other reports that researchers have failed to replicate the findings of these studies, in part because of problems with the underlying data and the calculations used to reach the conclusions. Questions have also been raised concerning the sharing and dissemination of the data and methods used to perform the studies. For example, according to the January 2005 *Energy & Environment*, such information necessary to replicate the analyses in the studies has not been made fully available to researchers upon request.

The concerns surrounding these studies reflect upon the quality and transparency of federally funded research and of the IPCC review process – two matters of particular interest to the Committee. For example, one concern relates to whether IPCC review has been sufficiently independent. We understand that you were a contributing author of the IPCC chapter that assessed and reported your own studies, and that two study co-authors were also contributors to this very same chapter. Given the prominence these studies were accorded in the IPCC TAR and your position and role in that process, we seek to learn more about the facts and circumstances that led to acceptance and prominent use of this work in the IPCC TAR and to understand what this controversy indicates about the data quality of key IPCC studies.

As you know, sharing data and research results is a basic tenet of open scientific inquiry, providing a means to judge the reliability of scientific claims. The ability to replicate a study, as the National Research Council has noted, is typically the gold standard by which the reliability of claims is judged. Given the questions reported about data access surrounding these studies, we also seek to learn whether obligations concerning the sharing of information developed or disseminated with federal support have been appropriately met.

In light of the Committee's jurisdiction over energy policy and certain environmental issues, the Committee must have full and accurate information when considering matters relating to climate change policy. We open this review because this dispute surrounding your studies bears directly on important questions about the federally funded work upon which climate studies rely and the quality and transparency of analyses used to support the IPCC assessment process. With the IPCC currently working to produce a fourth assessment report, addressing questions of quality and transparency in the process and underlying analyses supporting that assessment, both scientific and economic, are of utmost importance if Congress is eventually going to make policy decisions drawing from this work.

To assist us as we begin this review, and pursuant to Rules X and XI of the U.S. House of Representatives, please provide the following information requested below on or before July 11, 2005:

1. Your *curriculum vitae*, including, but not limited to, a list of all studies relating to climate change research for which you were an author or co-author and the source of funding for those studies.

2. List all financial support you have received related to your research, including, but not limited to, all private, state, and federal assistance, grants, contracts (including subgrants or subcontracts), or other financial awards or honoraria.

3. Regarding all such work involving federal grants or funding support under which you were a recipient of funding or principal investigator, provide all agreements relating to those underlying grants or funding, including, but not limited to, any provisions, adjustments, or exceptions made in the agreements relating to the dissemination and sharing of research results.

4. Provide the location of all data archives relating to each published study for which you were an author or co-author and indicate: (a) whether this information contains all the specific data you used and calculations your performed, including such supporting documentation as computer source code, validation information, and other ancillary information, necessary for full evaluation and application of the data, particularly for another party to replicate your research results; (b) when this information was available to researchers; (c) where and when you first identified the location of this information; (d) what modifications, if any, you have made to this information since publication of the respective study; and (e) if necessary information is not fully available, provide a detailed narrative description of the steps somebody must take to acquire the necessary information to replicate your study results or assess the quality of the proxy data you used.

5. Regarding study data and related information that is not publicly archived, what requests have you or your co-authors received for data relating to the climate change studies, what was your response, and why?

6. The authors McIntyre and McKitrick (*Energy & Environment*, Vol. 16, No. 1, 2005) report a number of errors and omissions in Mann *et. al.*, 1998. Provide a detailed narrative explanation of these alleged errors and how these may affect the underlying conclusions of the work, including, but not limited to answers to the following questions:
    a. Did you run calculations without the bristlecone pine series referenced in the article and, if so, what was the result?
    b. Did you or your co-authors calculate temperature reconstructions using the referenced "archived Gaspe tree ring data," and what were the results?
    c. Did you calculate the R2 statistic for the temperature reconstruction, particularly for the 15th Century proxy record calculations and what were the results?
    d. What validation statistics did you calculate for the reconstruction prior to 1820, and what were the results?
    e. How did you choose particular proxies and proxy series?

7. Explain in detail your work for and on behalf of the Intergovernmental Panel on Climate Change, including, but not limited to: (a) your role in the Third Assessment Report; (b) the process for review of studies and other information, including the dates of key meetings, upon which you worked during the TAR writing and review process; (c) the steps taken by you, reviewers, and lead authors to ensure the data underlying the studies forming the basis for key findings of the report were sound and accurate; (d) requests you received for revisions to your written contribution; and (e) the identity of the people who wrote and reviewed the historical temperature-record portions of the report, particularly Section 2.3, "Is the Recent Warming Unusual?"

Thank you for your assistance. If you have any questions, please contact Peter Spencer of the Majority Committee staff at (202) 226-2424.

Sincerely,

Joe Barton
Joe Barton
Chairman

Ed Whitfield
Ed Whitfield
Chairman
Subcommittee on Oversight
and Investigations

cc: The Honorable John Dingell, Ranking Member
The Honorable Bart Stupak, Ranking Member,
Subcommittee on Oversight and Investigations

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July 15, 2005

The Honorable Joe Barton, Chairman
House Committee on Energy and Commerce
The Honorable Ed Whitfield, Chairman
Subcommittee on Oversight and Investigations
2125 Rayburn House Office Building
Washington, D.C. 20515

Dear Congressman Barton and Congressman Whitfield,

I am writing in response to your letter of June 23 2005. As I only returned from Europe July 4 weekend, I am only able to provide information that is readily available. You ask a number of questions about work[1],[2] published by Professors Michael Mann, Ray Bradley and myself, and request a great deal of detailed information on the whole of my research career. Much of my work has had little if any bearing on the topic of global warming.

I would like to provide some accurate information about the papers published with Professors Mann and Bradley and their implications, the availability of the data we used, and the current status of this area of scientific inquiry.

I think you might find it helpful if I dealt briefly with three points first:

- You write that you "*understand that the Mann, Bradley, Hughes studies of temperature proxy records (tree rings, ice cores, corals, etc.) formed* ***the*** (my emphasis, MKH) *basis for a new finding in the 2001 United Nations Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report (TAR). This finding* [was] – *that the increase in 20th century northern hemisphere temperatures is "likely to have been the largest of any century during the past 1000 years" and that the "1990s was the warmest decade and 1998 the warmest year"*. This finding did not rest solely on our work, as is made clear on page 133 in section 2.3.2.2 of the IPCC TAR volume – "*Climate Change 2001: The Scientific Basis*" which lists the several lines of evidence they used to reach their finding.
- Your letter states that "*information necessary to replicate the analyses in the studies has not been made fully available to researchers upon request*". This too is mistaken. We have actively made the data and methods we used freely available. The proxy data (tree rings, coral bands,

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[1] Mann, M.E., Bradley, R.S., Hughes, M.K. Global-Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries, *Nature*, 392, 779-787 (1998)

[2] Mann, M.E., Bradley, R.S. and Hughes, M.K., Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations. *Geophysical Research Letters*, 26, 759-762 (1999)

ice cores, etc) we used in the Mann, Bradley, Hughes 1998 and 1999 papers have been at a publicly-accessible ftp site since 2000, and these data, plus the instrumental data we used, and an amplification of certain details of our methods have been available at the website of the journal *Nature* since July 2004. In fact, the original papers and supplementary material published in 1998 and 1999 were sufficient for working scientists to replicate both our methods and results, as they have done.

- Of course there has been debate in the scientific literature about the climate history of recent millennia, and about the many papers published on this topic recently. Our work is no exception to this, and rightly so. The small selection of articles referred to in your letter is not representative of this growing literature. A series of recent papers in the peer-reviewed scientific literature, based on quite different data and methods, confirm our main findings. This is the best and strongest form of scientific confirmation.

Expanding on the question of data availability and repeatability, my peer-reviewed published papers have given sufficiently detailed information to satisfy the scientific editors of the journals concerned and their peer-review processes. The U.S. National Science Foundation has stated that I have "*complied with the policy guidelines set out by the US government, and the NSF in particular, regarding access to data from publicly funded research*"[3]. For specifically dendrochronological calculations I use the publicly available programs of the Dendrochronology Program Library http://www.ltrr.arizona.edu/software.html. In the case of the Mann, Bradley, Hughes work mentioned in your letter, the proxy dataset we assembled has been publicly available for several years, and since July 2004 in an even more accessible form, with the addition of the instrumental data we used, as *Nature* Supplementary Material at http://www.nature.com/nature/journal/v430/n6995/suppinfo/nature02478.html. They may also be found at ftp://holocene.evsc.virginia.edu/pub/MBH98.

As a result, scientists at the National Center for Atmospheric Research are among those who have been able to reproduce our procedures and conclusions[4]. Further details of our work are given in our published papers[5], and in a paper that has been accepted for publication[6]. Any scientific issues arising from our work will be resolved in the scholarly literature over a period of years.

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[3] Email from Program Manager David Verardo 10 August 2004

[4] http://www.ucar.edu/news/releases/2005/ammann.shtml

[5] Mann, M.E., Bradley, R.S., Hughes, M.K. Global-Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries, *Nature*, 392, 779-787 (1998)
Mann, M.E., Bradley, R.S. and Hughes, M.K., Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations. *Geophysical Research Letters*, 26, 759-762 (1999)
Mann, M.E., Gille, E., Bradley, R.S., Hughes, M.K., Overpeck, J.T., Keimig, F.T., Gross, W. Global Temperature Patterns in Past Centuries: An interactive presentation, *Earth Interactions*, 4-4, 1-29, (2000), accessible online at: http://www.ncdc.noaa.gov/paleo/ei/ei_cover.html, and in particular note: http://www.ncdc.noaa.gov/paleo/ei/ei_nodendro.html

My role in the IPCC TAR was very small. I was one of 140 "Contributing authors" of Chapter 2 of the IPCC TAR Working Group 1 volume titled "*Climate Change 2001: The Scientific Basis*". In practice this meant that I was sent a *.pdf of the whole draft report in late 1999 and was asked for my comments and suggestions. My comments related to Chapters 1 and 2. I returned these comments, which were limited to matters of which I have professional knowledge, to the IPCC Secretariat. That was the sum total of my involvement in that volume. In April 2001 I was one of a very large number of people asked to be expert reviewers on the first draft of the Synthesis Report of the IPCC TAR. Again, I returned my comments to the IPCC secretariat, that being the end of my involvement with the IPCC TAR. I attended no meetings, was not asked to revise anything I submitted, and have no knowledge of the writing assignments, duties or practices of the authors of the various sections.

Please find my *curriculum vitae* enclosed with this letter. It lists over 120 publications, most in the peer-reviewed scientific literature, that I have written, co-authored or edited on the basis of work done since 1965. It also contains a list of grants received for my research since coming to the US in 1986.

In summary, our work was definitely not the only line of evidence used by the IPCC to reach the conclusions you mention, but one of several. Our work has been independently replicated, proving that we made ample information available for this. Furthermore, a series of completely independent studies have arrived at similar conclusions using entirely different data and methods. The normal, time-tested, process of science is at work, and should be permitted to continue.

I hope you will find my comments to be of help. Our university is a major center of research on past environments and, should you ever have the opportunity to visit Tucson, we would be pleased to give you an introduction the wide and fascinating range of work being done here.

Sincerely,

Malcolm K. Hughes
Professor of Dendrochronology

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[6] Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, *Journal of Climate*, in press, 2005.

CURRICULUM VITAE

Malcolm K. Hughes

PERSONAL DETAILS

Born in Matlock, Derbyshire, England, 24th July 1943. US citizen.

EDUCATION

1965	B.Sc. (Honours) in Botany and Zoology, University of Durham, U.K..
1970	Ph.D., title of thesis: 'Investigations of the ecosystem energetics of an English woodland'. Supervisor; Dr. J. Phillipson, University of Durham.

EXPERIENCE

1968-69	Amanuensis, Research Fellow, Soil Biology Institute, University of Aarhus, Denmark
1969-71	University Research Fellow, Botany Department University of Durham.
1971-73	Lecturer II in Ecology, Biology Department, Liverpool Polytechnic (now Liverpool John Moores University).
1973-80	Senior Lecturer in Ecology, Liverpool Polytechnic.
1980-82	Principal Lecturer in Ecology, Liverpool Polytechnic.
1982-86	Reader in Ecology, Liverpool Polytechnic.
1986-1999	Director of the Laboratory of Tree-Ring Research, University of Arizona
1986-	Professor of Dendrochronology, University of Arizona
1992-	Professor of Watershed Management, School of Renewable Natural Resources, University of Arizona
1992-3	Visiting Fellow, Cooperative Institute for Research in Environmental Sciences, University of Colorado-Boulder

OTHER ACADEMIC ACTIVITIES

1978-85	External Examiner (CNAA) for degree studies in ecology, New College, Durham.
~~1980-86~~	~~External Examiner in ecology for Membership of the Institute of Biology,~~ U.K.
1974-	Internal or External Examiner of various higher degree candidates of the Council for National Academic Awards and the Universities of Oxford,

	Durham, East Anglia, Ulster, Aix-Marseille, Amsterdam, Auckland, New Zealand, Queen's University Belfast, and of the Chinese Academy of Science.
1983-86	Member, Combined Studies (Science) Board, Council for National Academic Awards, U.K.
1990	Member, University of Arizona review panel of Arid Lands Sciences academic program
1991	Chair, University review panel of Remote Sensing academic program
1991	Member, University review panel of Statistics Department
1996	Member, University review panel of Hydrology and Water Resources Department
1995	Member, Academic review panel for Quaternary Research Center, University of Washington
1994	Member, Search Committee, Dean of College of Science, University of Arizona
1995-	Member, Executive Committee, Institute for the Study of Planet Earth
1996-	Member, Executive Committee, Interdisciplinary Program in Global Change

HONORS

1998	Fellow, American Geophysical Union
1999-2000	Bullard Fellow, Harvard University

LEARNED SOCIETIES AND RELATED ACTIVITIES

	Member, British Ecological Society. Fellow, American Geophysical Union Member AAAS Member, International Tree-Ring Society
1972-78	Secretary, Energy and Production Biology Group, member of the meetings committee: British Ecological Society.
1978-82	Organizer, Global Dendroclimatology Workshop.
1982-85	Member, Terrestrial Life Sciences Grants Committee, Natural Environment Research Council, U.K.
1984-85	Member, Advanced Courses Review Panel, Natural Environment Research Council, U.K.
1984-6	Member, Council of the British Ecological Society.
1986-	Chair, Committee on Global Change, Faculty of Science, University of Arizona.
~~1988-1992~~	~~Chair, Coordinating Committee on Global Change, University of Arizona.~~
1988-	Member, U.S. National Committee for the International Union for Quaternary Research, National Research Council
1988	Member, working group meeting on 'Techniques for extracting

	environmental data from the past' set up by ICSU's Special Committee for the International Geosphere-Biosphere Programme
1988-1989	Member, Organizing Committee, 1989 Global Change Institute, Universities' Corporation for Atmospheric Research
1989-1992	Member, National Oceanographic and Atmospheric Administration (NOAA) Paleoclimatology Advisory Panel
1990	Member, Advisory Panel for Meeting on Earth System History, National Science Foundation.
1990-	Member, Technical Advisory Panel, Western Region of the National Institute on Global Environmental Change, US Department of Energy
1991	Organizer (with H.Diaz) Medieval Warm Period Workshop
1991-1993	Guest editor (with H.Diaz) special issue of *Climatic Change*.
1992-1995	Member, Board of Trustees, National Institute of Global Environmental Change (NIGEC)
1992-	Member, Scientific Council, Siberian International Centre for Ecological Research of Boreal Forests
1992-1994	Chair, program committee, International Conference on Tree Rings, Environment and Humanity, Tucson, Arizona, May 1994.
1993	Chair, search committee for new national Director, NIGEC
1993-	Member, executive committee, Institute for the Study of Planet Earth, University of Arizona
1993-1997	Member, Committee on Geophysical and Environmental Data, National Research Council
1995-1998	Member, Biometeorology Committee, American Meteorological Society (AMS)
1995-1998	Member, AMS Glossary revision committee
1996-1999	Member, joint working group between the PAGES core project of the International Geosphere-Biosphere Program and the CLIVAR project of the World Climate Research Program
1999-2005	Member, steering committee, National Science Foundation PARCS.
2000-2004	Vice-President, International Tree-Ring Society
2001-	Member, Advisory Board, *Dendrochronologia*
2003-4	Chair, Organizing Committee, international conference "Tree Rings and Climate: Sharpening the Focus", Tucson, Arizona, April 2004.

LECTURES/SEMINARS/PAPERS READ

Presented papers or given invited lectures in Argentina, France, the Netherlands, Denmark, ~~Germany, Sweden, Finland, Switzerland, Italy, Canada, Chile, Argentina, Panama, Mexico, U.K,~~ Poland, Peoples Republic of China, Korea, Japan, Taiwan, Russia, Jordan and India as well as in the U.S.A.

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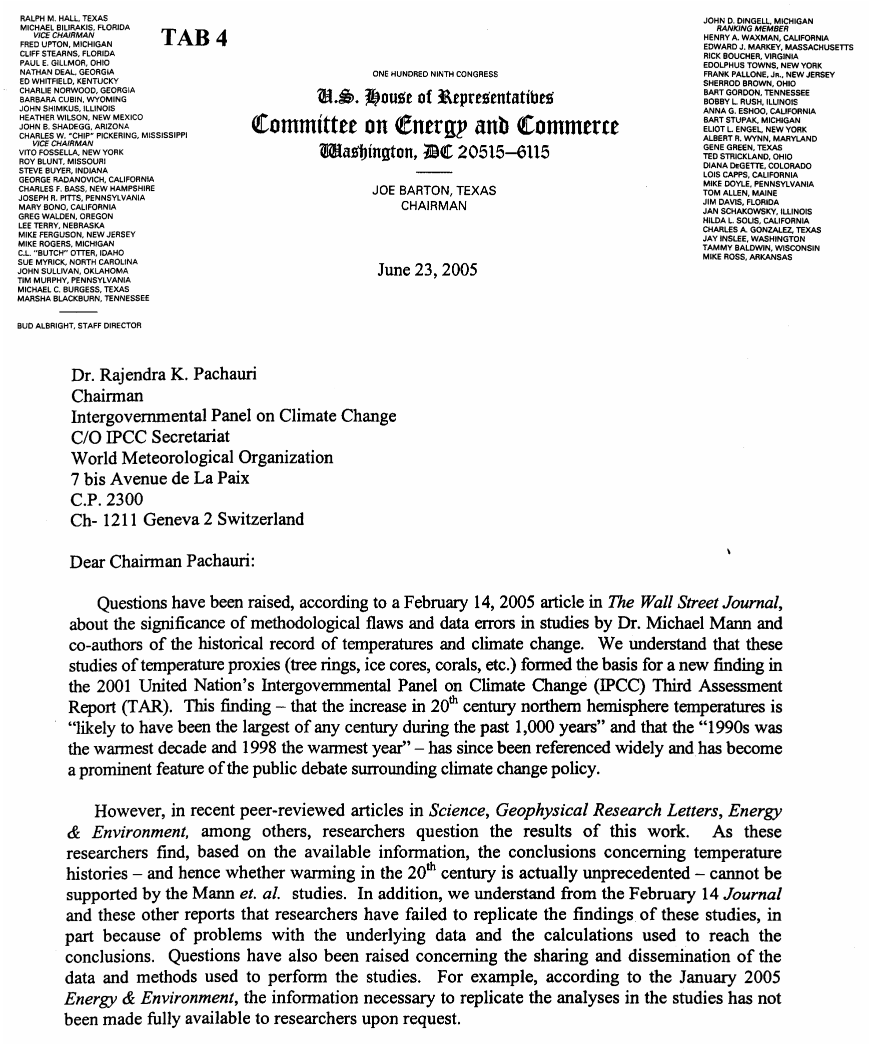
Table 1: M K Hughes Funded projects

	Funding Agency	Title	Start Date	Lead PI	Co-PIs
1	NSF	Arizona Center on Global Change: Planning Grant	7/1/1988	Malcolm Hughes	Charles Hutchinson, William Sellers, Victor Baker
2	NSF	Long-term Spatial and Temporal Variability of Precipitation,Evaporation, Soil Moisture and Runoff as Related to Drought Occurrence over Continental United States	1/1/1989	Charles Stockton	Malcolm Hughes
3	NSF	Solar Modulation of Atmospheric 14C production during the Little Ice Age and Medieval Warm Epoch (Solar Wind, Flares and Irradiance)	4/1/1990	Paul Damon	Malcolm Hughes and others?
4	NOAA	Climatic Variations in the Asian Monsoon Region since AD 1600	10/1/1990	Malcolm Hughes	Raymond Bradley, Lonnie Thompson
5	National Park Service: Global Change Program	Tree-ring evidence of climatic change and vegetation responses	6/1/1991	Lisa Graumlich	Malcolm Hughes
6	NSF	Project ARRCC: Analysis of Recent and Rapid Climate Change	9/15/1991	Malcolm Hughes	
7	NSF	Global Change: Cosmic Rays, Solar Activity, Geomagnetism, Cosmogenic Isotopes and Climate	6/15/1993	Paul Damon	Charles Sonett,Austin Long, Douglas Donahue,Malcolm Hughes
8	NOAA	Climate in North-Central China since AD 1600 from Tree-Ring Variables	6/1/1993	Malcolm Hughes	Steve Leavitt
9	NSF	A multimillennial temperature reconstruction from far northeastern Eurasia	4/1/1994	Malcolm Hughes	
10	NOAA	Climate Variability in Western North America on decadal time scales	5/1/1995	Malcolm Hughes	David Meko
11	NSF	Preparation for collaborative work on past climate in Jordan	10/1/1995	Malcolm Hughes	Ramzi Touchan
12	NOAA	Late Holocene climate variability from long tree-ring chronologies	6/1/1996	Malcolm Hughes	
13	NSF	Development of Computer Assisted Tree Ring Dating System	9/15/1996	Malcolm Hughes	Robert Schowengerdt

14	CRDF	Natural variability of summer temperature in the Siberian subarctic recorded in tree rings	?/?/1997	Malcolm Hughes	Eugene Vaganov
15	NSF	A Multimillennial Temperature Reconstruction from Far Northeastern Eurasia	8/1/1997	Malcolm Hughes	
16	NOAA	Climate variability, social vulnerability and public policy in the southwestern US: a proposal for regional assessment activities	?/?/1997	Roger Bales	T. Finan, M.Hughes, D. Liverman, S.Sorooshian
17	USDA Forest Service	A tree-ring reconstruction of Pandora Moth population history in central and southern Oregon: expansion of exploratory study	1/1/1997	Tom Swetnam	Malcolm Hughes
18	NOAA	Decade-to-century hydroclimatic variability in Western North America	8/1/1998	David Meko	Malcolm Hughes
19	NSF	Temperature variability since AD 1000 in the Western US from tree-ring	8/1/1998	Malcolm Hughes	Connie Woodhouse, Peter Brown
20	NSF	Climate and society on the Colorado Plateau, AD 500-1600	5/1/1999	Malcolm Hughes	Jeff Dean
21	University of Massachusetts (sub-NSF)	Multiproxy climate reconstruction: Extension in space and time, model/data intercomparison	9/1/1999	Malcolm Hughes	
22	NSF	International Conference on Dendrochronology 2-7 April 2000, Mendoza, Argentina	12/1/1999	Malcolm Hughes	
23	University of Nevada in Reno (sub-NOAA)	Tree-ring construction of Climate Variability at the Tropical boundary of the American West	6/1/2000	Malcolm Hughes	Wolfgang Berger, Franco Biondi, Daniel Cayan
24	CRDF	Tree-ring measurements and NDVI as indicators of forest growth in Siberia as influenced by varying climate (Russia)	7/1/2000	Malcolm Hughes	Eugene Vaganov
25	NSF	Near East climate variability from tree-ring, Syria, Turkey, Jordan	7/1/2000	Ramzi Touchan	Malcolm Hughes

26	NSF	The application of tree-ring analysis to reconstruct the drouhgt/hydro-climatic history of the West African Sahel	5/7/1999	Malcolm Hughes	
27	NOAA	Global multidecadal to century-scale climate oscillations during the last 1000 years	5/1/2001	Malcolm Hughes	
28	NOAA	Variability, social vulnerability, and public policy in the Southwestern United States: a propsal for regional assessment activities	6/1/2001	Roger Bales	Barbara Morehouse, Soroosh Sorooshian, Andrew Comrie, Diana Liverman, Malcolm Hughes, Timothy Finan, Maria Carmen Lemos
29	NSF	Development of an enhanced computer-assisted analysis system for eath science investigation of laminated sediments and tree rings	1/1/2002	Jonathan Overpeck	Malcolm Hughes, Robert Schowengerdt
30	NSF	Natural spatiotemporal variability of climate over the Western US in the late Holocene	7/1/2002	Malcolm Hughes	
31	NOAA	Collaborative: Reconstruction and analysis of patterns of climate variability over one to two millenia	9/1/2002	Malcolm Hughes	
32	NSF	Acquisition of an Analytical Facility for High-Resolution Paleoclimatology	8/1/2003	Michael Evans	J. Warren Beck, J.T.Overpeck, J.E.Cole, Malcolm Hughes
33	NSF	New and improved tree-ring records of climate from Siberia	6/1/2003	Malcolm Hughes	
34	NOAA	Time-dependent bias in tree-ring based reconstruction	9/1/2003	Malcolm Hughes	Dave Meko
35	NSF	Meeting on tree-ring and climate: sharpening the focus	10/8/2003	Malcolm Hughes	
36	NOAA	Meeting on tree-ring and climate: sharpening the focus	1/1/2004	Malcolm Hughes	
37	Institute for Aegean Prehistory	Volcanic eruptions 3400-3700 years ago recorded in *Pinus longaeva* tree-rings: expanded collections and analyses	6/1/2005	Malcolm Hughes	



TAB 4

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ONE HUNDRED NINTH CONGRESS

U.S. House of Representatives
Committee on Energy and Commerce
Washington, DC 20515–6115

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MIKE ROSS, ARKANSAS

June 23, 2005

Dr. Rajendra K. Pachauri
Chairman
Intergovernmental Panel on Climate Change
C/O IPCC Secretariat
World Meteorological Organization
7 bis Avenue de La Paix
C.P. 2300
Ch- 1211 Geneva 2 Switzerland

Dear Chairman Pachauri:

Questions have been raised, according to a February 14, 2005 article in *The Wall Street Journal*, about the significance of methodological flaws and data errors in studies by Dr. Michael Mann and co-authors of the historical record of temperatures and climate change. We understand that these studies of temperature proxies (tree rings, ice cores, corals, etc.) formed the basis for a new finding in the 2001 United Nation's Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report (TAR). This finding – that the increase in 20th century northern hemisphere temperatures is "likely to have been the largest of any century during the past 1,000 years" and that the "1990s was the warmest decade and 1998 the warmest year" – has since been referenced widely and has become a prominent feature of the public debate surrounding climate change policy.

However, in recent peer-reviewed articles in *Science*, *Geophysical Research Letters*, *Energy & Environment,* among others, researchers question the results of this work. As these researchers find, based on the available information, the conclusions concerning temperature histories – and hence whether warming in the 20th century is actually unprecedented – cannot be supported by the Mann *et. al.* studies. In addition, we understand from the February 14 *Journal* and these other reports that researchers have failed to replicate the findings of these studies, in part because of problems with the underlying data and the calculations used to reach the conclusions. Questions have also been raised concerning the sharing and dissemination of the data and methods used to perform the studies. For example, according to the January 2005 *Energy & Environment*, the information necessary to replicate the analyses in the studies has not been made fully available to researchers upon request.

The concerns surrounding these studies reflect upon the quality and transparency of federally funded research and of the IPCC review process – two matters of particular interest to the Committee. For example, one concern relates to whether IPCC review has been sufficiently robust and independent. We understand that Dr. Michael Mann, the lead author of the studies in question, was also a lead author of the IPCC chapter that assessed and reported this very same work, and that two co-authors of the studies were also contributing authors to the same chapter. Given the prominence these studies were accorded in the IPCC TAR, we seek to learn more about the facts and circumstances that led to acceptance and prominent use of this work in the IPCC TAR and to understand what this controversy indicates about the data quality of key IPCC studies.

In light of the Committee's jurisdiction over energy policy and certain environmental issues in the U.S. House of Representatives, the Committee must have full and accurate information when considering matters relating to climate change policy. We open this review because the dispute surrounding these studies bears directly on important questions about the federally funded work upon which climate studies rely and the quality and transparency of analyses used to support the IPCC assessment process. With the IPCC currently working to produce a fourth assessment report, addressing questions of quality and transparency in the underlying analyses supporting that assessment, both scientific and economic, are of utmost importance if Congress is eventually going to make policy decisions drawing from this work.

To assist us as we begin this review, and pursuant to Rules X and XI of the U.S. House of Representatives, please provide the following information requested below on or before July 11, 2005:

1. Explain the IPCC process for preparing and writing its assessment reports, including, but not limited to: (a) how referenced studies are reviewed and assessed by the relevant Working Group; (b) the steps taken by lead authors, reviewers, and others to ensure the data underlying the studies forming the basis for key findings – particularly proxy and temperature data – are accurate and up to date; and (c) the IPCC requirements governing the quality of data used in reports.

2. What specifically did IPCC do to check the quality of the Mann et. al. studies and underlying data, cited in the TAR? Did IPCC seek to ensure the studies could be replicated?

3. What is your position with regard to: (a) the recent challenges to the quality of the Mann et. al. data, (b) related questions surrounding the sharing of methods and research for others to test the validity of these studies, and (c) what this controversy indicates about the data quality of key IPCC studies?

4. What did IPCC do to ensure the quality of data for other prominent historical temperature or proxy studies cited in the IPCC, including the Folland et. al. and Jones et. al. studies that were sources for the graphic accompanying the Mann et. al. graphic in the Summary for Policy Makers? Are the data and methodologies for such works complete and available for other researchers to test and replicate?

5. Explain (a) the facts and circumstances by which Dr. Michael Mann served as a lead author of the very chapter that prominently featured his work and (b) by which his work became a finding and graphical feature of the TAR Summary for Policymakers.

6. Explain (a) how IPCC ensures objectivity and independence among section contributors and reviewers, (b) how they are chosen, and (c) how the chapters, summaries, and the full report are approved and what any such approval signifies about the quality and acceptance of particular research therein.

7. Identify the people who wrote and reviewed the historical temperature-record portions of the TAR, particularly Section 2.3, "Is the Recent Warming Unusual?" and explain all their roles in the preparation of the TAR, including, but not limited to, the specific roles in the writing and review process.

8. Given the questions about Mann et. al. data, has the Working Group I or the IPCC made any changes to specific procedures or policies, including policies for checking the quality of data, for the forthcoming Fourth Assessment Report? If so, explain in detail any such changes, and why they were made.

9. Does the IPCC or Working Group I have policies or procedures regarding the disclosure and dissemination of scientific data referenced in the reports? If so, explain in detail any such policies and what happens when they are violated.

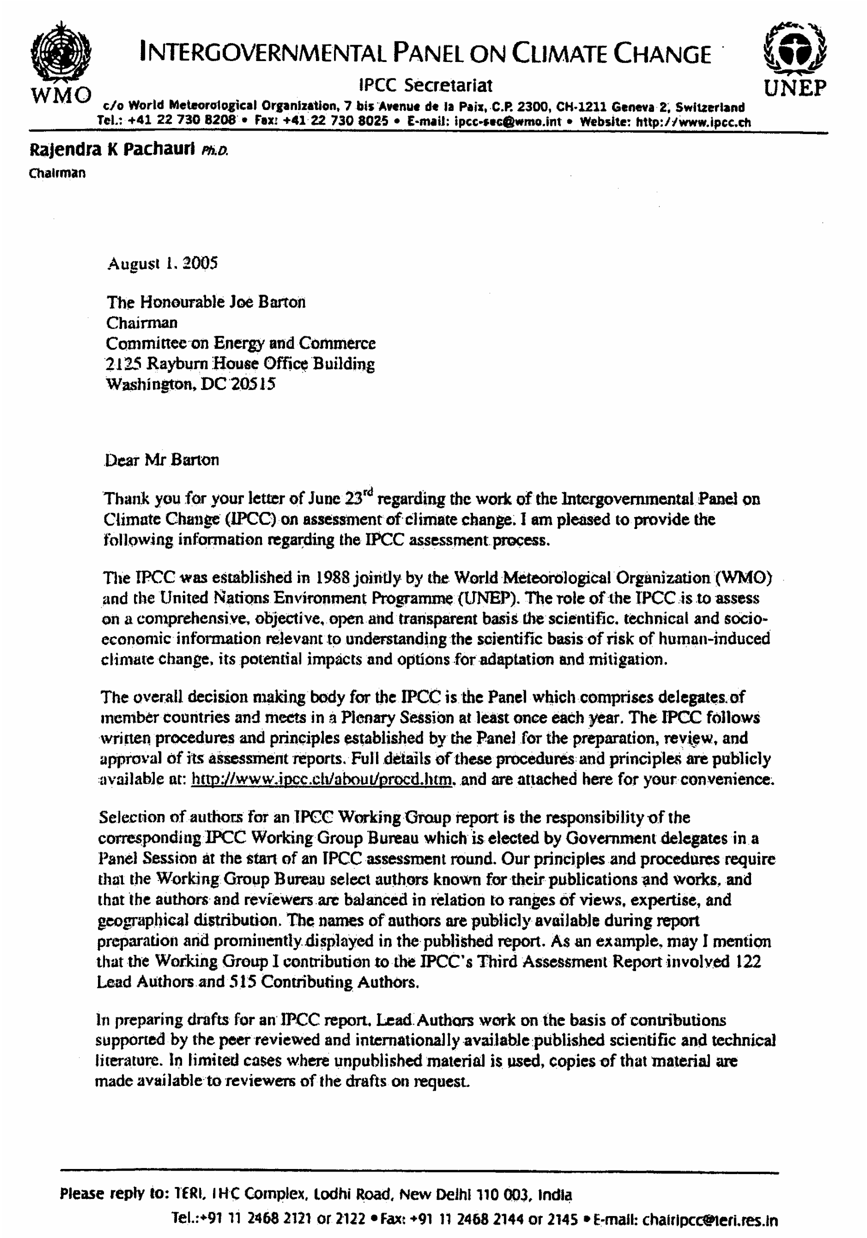
Thank you for your assistance. If you have any questions, please contact Peter Spencer of the Majority Committee staff at (202) 226-2424.

Sincerely,

Joe Barton
Joe Barton
Chairman

Ed Whitfield
Ed Whitfield
Chairman
Subcommittee on Oversight
and Investigations

cc: The Honorable John Dingell, Ranking Member
The Honorable Bart Stupak, Ranking Member,
Subcommittee on Oversight and Investigations



WMO
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Rajendra K Pachauri *Ph.D.*
Chairman

August 1, 2005

The Honourable Joe Barton
Chairman
Committee on Energy and Commerce
2125 Rayburn House Office Building
Washington, DC 20515

Dear Mr Barton

Thank you for your letter of June 23rd regarding the work of the Intergovernmental Panel on Climate Change (IPCC) on assessment of climate change. I am pleased to provide the following information regarding the IPCC assessment process.

The IPCC was established in 1988 jointly by the World Meteorological Organization (WMO) and the United Nations Environment Programme (UNEP). The role of the IPCC is to assess on a comprehensive, objective, open and transparent basis the scientific, technical and socio-economic information relevant to understanding the scientific basis of risk of human-induced climate change, its potential impacts and options for adaptation and mitigation.

The overall decision making body for the IPCC is the Panel which comprises delegates of member countries and meets in a Plenary Session at least once each year. The IPCC follows written procedures and principles established by the Panel for the preparation, review, and approval of its assessment reports. Full details of these procedures and principles are publicly available at: http://www.ipcc.ch/about/procd.htm, and are attached here for your convenience.

Selection of authors for an IPCC Working Group report is the responsibility of the corresponding IPCC Working Group Bureau which is elected by Government delegates in a Panel Session at the start of an IPCC assessment round. Our principles and procedures require that the Working Group Bureau select authors known for their publications and works, and that the authors and reviewers are balanced in relation to ranges of views, expertise, and geographical distribution. The names of authors are publicly available during report preparation and prominently displayed in the published report. As an example, may I mention that the Working Group I contribution to the IPCC's Third Assessment Report involved 122 Lead Authors and 515 Contributing Authors.

In preparing drafts for an IPCC report, Lead Authors work on the basis of contributions supported by the peer reviewed and internationally available published scientific and technical literature. In limited cases where unpublished material is used, copies of that material are made available to reviewers of the drafts on request.

Please reply to: TERI, IHC Complex, Lodhi Road, New Delhi 110 003, India
Tel.:+91 11 2468 2121 or 2122 • Fax: +91 11 2468 2144 or 2145 • E-mail: chairipcc@teri.res.in

A central feature of IPCC Assessment reports is that they are subject to two rounds of open and extensive review. The Working Group I contribution to the IPCC Third Assessment Report was reviewed by over 670 expert reviewers, whose names are given in Apppendix III of that report, in addition to reviews coordinated by Governments. All review comments are considered by the authors and a group of independent review editors ensure that all substantive expert and Government review comments are afforded appropriate consideration. Author responses to all comments are recorded and archived.

I hope this clarifies the breadth of the author teams as well as the breadth and depth of the review process followed. The findings in a final IPCC report are not the views of any individual scientist but reflect a much broader process.

Let me stress that the IPCC is an intergovernmental assessment body that is designed to meet the requirements of Governments for information relating to all aspects of climate change that is policy relevant but not policy prescriptive. The IPCC does not undertake research, including that related to scientific data.

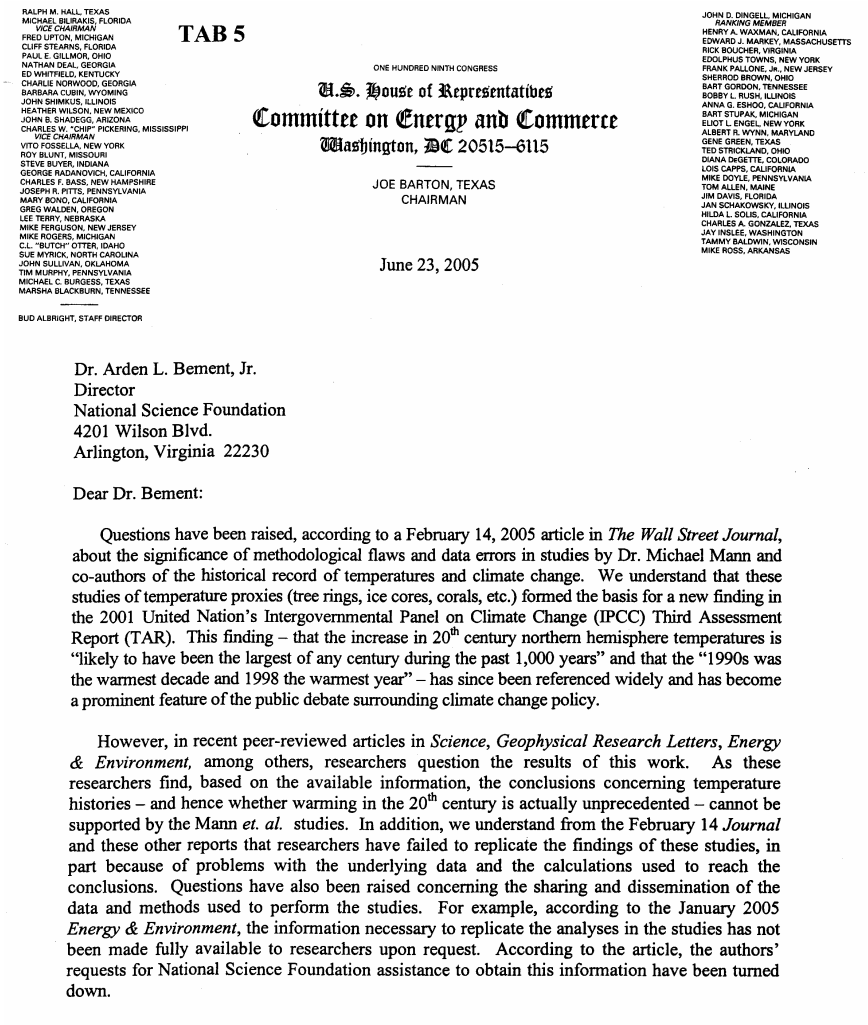
The final stage in the preparation of an IPCC assessment report is a line by line approval of a Summary for Policymakers and acceptance of the underlying report in a Plenary Session involving both authors of the report and government delegates. The Plenary Session for approval of the Working Group I contribution to the Third Assessment Report was held in Shanghai in January 2001 and attended by delegates from 99 member countries.

As you will be aware, the Panel has decided to undertake a Fourth Assessment Report that will be completed in 2007. This decision recognizes the continual advances being made in climate change science and the need to periodically re-assess the peer-reviewed literature as new studies become available. The authors and reviewers involved in the assessment process that is now underway will thus consider new work in an objective fashion on the reconstruction of past temperatures among other topics pertinent to understanding climate change. This would include not only works that critique the literature considered in the Third Assessment Report such as those recent studies you have cited, but also those that support or extend it. As in the Third Assessment Report, careful attention will be paid to issues of balance and the scientific evaluation of levels of confidence including the broad range of published studies.

I trust that you will find this information useful.

Sincerely

R. K. Pachauri
Chair
Intergovernmental Panel on Climate Change



TAB 5

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JAY INSLEE, WASHINGTON
TAMMY BALDWIN, WISCONSIN
MIKE ROSS, ARKANSAS

June 23, 2005

Dr. Arden L. Bement, Jr.
Director
National Science Foundation
4201 Wilson Blvd.
Arlington, Virginia 22230

Dear Dr. Bement:

Questions have been raised, according to a February 14, 2005 article in *The Wall Street Journal*, about the significance of methodological flaws and data errors in studies by Dr. Michael Mann and co-authors of the historical record of temperatures and climate change. We understand that these studies of temperature proxies (tree rings, ice cores, corals, etc.) formed the basis for a new finding in the 2001 United Nation's Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report (TAR). This finding – that the increase in 20th century northern hemisphere temperatures is "likely to have been the largest of any century during the past 1,000 years" and that the "1990s was the warmest decade and 1998 the warmest year" – has since been referenced widely and has become a prominent feature of the public debate surrounding climate change policy.

However, in recent peer-reviewed articles in *Science*, *Geophysical Research Letters*, *Energy & Environment,* among others, researchers question the results of this work. As these researchers find, based on the available information, the conclusions concerning temperature histories – and hence whether warming in the 20th century is actually unprecedented – cannot be supported by the Mann *et. al.* studies. In addition, we understand from the February 14 *Journal* and these other reports that researchers have failed to replicate the findings of these studies, in part because of problems with the underlying data and the calculations used to reach the conclusions. Questions have also been raised concerning the sharing and dissemination of the data and methods used to perform the studies. For example, according to the January 2005 *Energy & Environment*, the information necessary to replicate the analyses in the studies has not been made fully available to researchers upon request. According to the article, the authors' requests for National Science Foundation assistance to obtain this information have been turned down.

The concerns surrounding these studies reflect upon the quality and transparency of federally funded research and of the IPCC review process – two matters of particular interest to the Committee. For example, one concern relates to whether IPCC review has been sufficiently independent. We understand that Dr. Michael Mann, the lead author of the studies in question, was also a lead author of the IPCC chapter that assessed and reported this very same work, and that two co-authors of the studies were also contributing authors to the same chapter. Given the prominence these studies were accorded in the IPCC TAR, we seek to learn more about the facts and circumstances that led to acceptance and prominent use of this work in the IPCC TAR and to understand what this controversy indicates about the data quality of key IPCC studies.

As you know, sharing data and research results is a basic tenet of open scientific inquiry, providing a means to judge the reliability of scientific claims. The ability to replicate a study, as the National Research Council has noted, is typically the gold standard by which the reliability of claims is judged. Given the questions reported about data access surrounding these studies, we also seek to learn whether obligations concerning the sharing of information developed or disseminated with the support of your agency have been appropriately met.

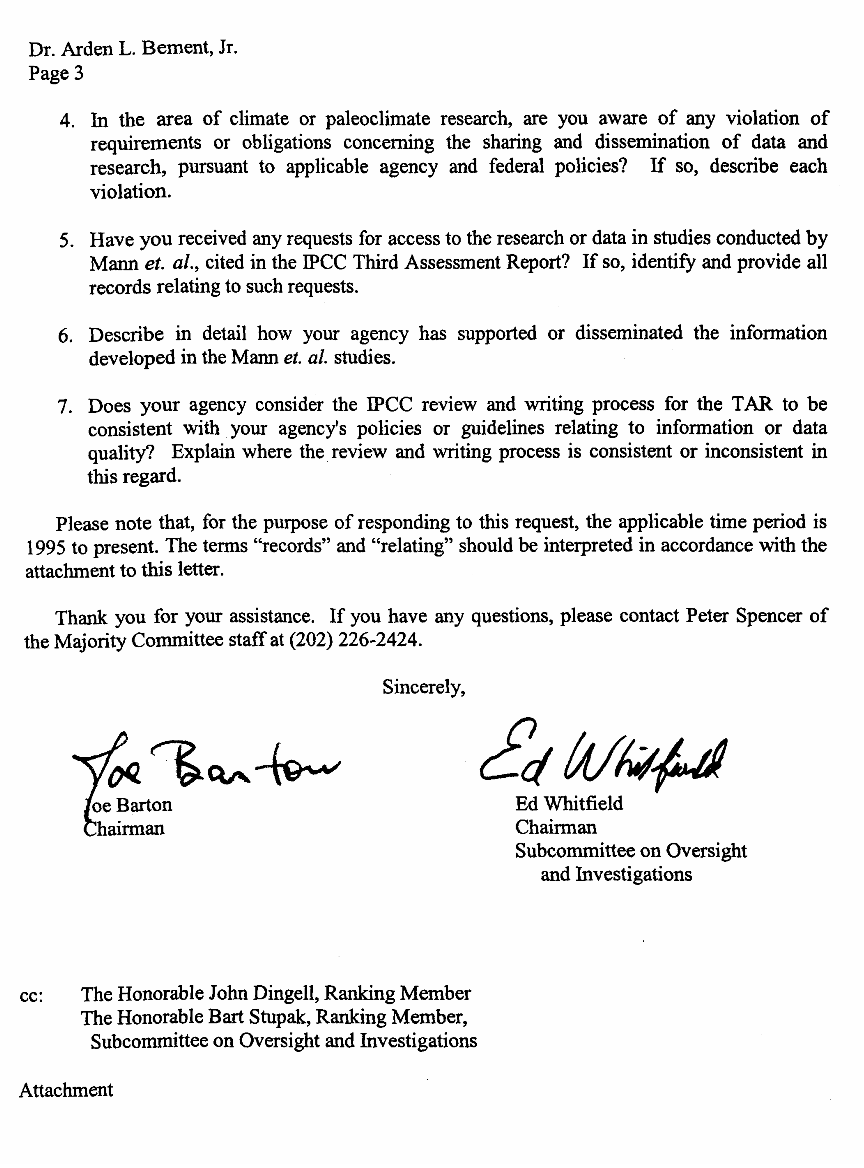
In light of the Committee's jurisdiction over energy policy and certain environmental issues, the Committee must have full and accurate information when considering matters relating to climate change policy. We open this review because the dispute surrounding these studies bears directly on important questions about the federally funded work upon which climate studies rely and the quality and transparency of analyses used to support the IPCC assessment process. With the IPCC currently working to produce a fourth assessment report, addressing questions of quality and transparency in the underlying analyses supporting that assessment, both scientific and economic, are of utmost importance if Congress is eventually going to make policy decisions drawing from this work.

To assist us as we begin this review, and pursuant to Rules X and XI of the U.S. House of Representatives, please provide the following information requested below on or before July 11, 2005:

1. Explain in detail your policies and procedures regarding the disclosure and dissemination of scientific data obtained with federal funds and grants, including, but not limited to: (a) a copy of all such applicable policies and internal guidance relating to implementation of such policies, (b) the obligations of universities, individuals, and other funding recipients regarding these policies, (c) the agency departments and individuals charged with ensuring such policies are implemented, and (d) the steps your agency takes to ensure these policies are implemented.

2. List all grants and all other funding awards given for research in the area of climate or paleoclimate research, including, but not limited to, the dates of the awards, the identity of the recipients, principal investigators, and whoever is contractually obligated to ensure provisions of the awards are met.

3. Explain how NSF ensures compliance with award provisions.



Dr. Arden L. Bement, Jr.
Page 3

4. In the area of climate or paleoclimate research, are you aware of any violation of requirements or obligations concerning the sharing and dissemination of data and research, pursuant to applicable agency and federal policies? If so, describe each violation.

5. Have you received any requests for access to the research or data in studies conducted by Mann *et. al.*, cited in the IPCC Third Assessment Report? If so, identify and provide all records relating to such requests.

6. Describe in detail how your agency has supported or disseminated the information developed in the Mann *et. al.* studies.

7. Does your agency consider the IPCC review and writing process for the TAR to be consistent with your agency's policies or guidelines relating to information or data quality? Explain where the review and writing process is consistent or inconsistent in this regard.

Please note that, for the purpose of responding to this request, the applicable time period is 1995 to present. The terms "records" and "relating" should be interpreted in accordance with the attachment to this letter.

Thank you for your assistance. If you have any questions, please contact Peter Spencer of the Majority Committee staff at (202) 226-2424.

Sincerely,

Joe Barton
Joe Barton
Chairman

Ed Whitfield
Ed Whitfield
Chairman
Subcommittee on Oversight
and Investigations

cc: The Honorable John Dingell, Ranking Member
The Honorable Bart Stupak, Ranking Member,
Subcommittee on Oversight and Investigations

Attachment

ATTACHMENT

1. The term "records" is to be construed in the broadest sense and shall mean any written or graphic material, however produced or reproduced, of any kind or description, consisting of the original and any non-identical copy (whether different from the original because of notes made on or attached to such copy or otherwise) and drafts and both sides thereof, whether printed or recorded electronically or magnetically or stored in any type of data bank, including, but not limited to, the following: correspondence, memoranda, records, summaries of personal conversations or interviews, minutes or records of meetings or conferences, opinions or reports of consultants, projections, statistical statements, drafts, contracts, agreements, purchase orders, invoices, confirmations, telegraphs, telexes, agendas, books, notes, pamphlets, periodicals, reports, studies, evaluations, opinions, logs, diaries, desk calendars, appointment books, tape recordings, video recordings, e-mails, voice mails, computer tapes, or other computer stored matter, magnetic tapes, microfilm, microfiche, punch cards, all other records kept by electronic, photographic, or mechanical means, charts, photographs, notebooks, drawings, plans, inter-office communications, intra-office and intra-departmental communications, transcripts, checks and canceled checks, bank statements, ledgers, books, records or statements of accounts, and papers and things similar to any of the foregoing, however denominated.

2. The terms "relating," "relate," or "regarding" as to any given subject means anything that constitutes, contains, embodies, identifies, deals with, or is in any manner whatsoever pertinent to that subject, including but not limited to records concerning the preparation of other records.

NATIONAL SCIENCE FOUNDATION
4201 WILSON BOULEVARD
ARLINGTON, VIRGINIA 22230

nsf

OFFICE OF THE
DIRECTOR

July 18, 2005

The Honorable Joe Barton, Chairman
Committee on Energy and Commerce
The Honorable Ed Whitfield, Chairman
Subcommittee on Oversight and Investigations
House of Representatives
2125 Rayburn House Office Building
Washington, D.C. 20515

Dear Representatives Barton and Whitfield:

I am responding to your letter of June 23, 2005. You asked several questions about NSF policy and practices regarding the dissemination of data obtained with NSF funded scientific research grants. You also requested specific information about NSF funding of paleoclimate research, as well as information about any inquiries related to the research specifically conducted by Dr. Mann, et al., that was cited in the IPCC Third Assessment Report. We have provided below and in the attachments to this letter materials and information responsive to this request that are either publicly available or are routinely made available.[1]

Question 1

> Explain in detail your policies and procedures regarding the disclosure and dissemination of scientific data obtained with federal funds and grants, including, but not limited to: a) a copy of all such applicable policies and internal guidance relating to the implementation of such policies, (b) the obligations of universities, individuals and other funding recipients regarding these policies, (c) the agency departments and individual charged with ensuring such policies are implemented, and (d) the steps your agency takes to ensure such policies are implemented.

Response

NSF's primary external policy document regarding the disclosure and dissemination of scientific data is the *Grant Proposal Guide* (GPG). The sharing of findings, data and other research products is addressed in the GPG Chapter VI.I (http://www.nsf.gov/pubs/gpg/nsf04_23/6.jsp#VII). The text is as follows:

---

[1] Inasmuch as the information and documents provided are limited to those publicly available, this response does not require NSF to take a position with respect to committee jurisdiction.

> **"I. SHARING OF FINDINGS, DATA AND OTHER RESEARCH PRODUCTS**
>
> NSF advocates and encourages open scientific communication. NSF expects significant findings from supported research and educational activities to be promptly submitted for publication with authorship that accurately reflects the contributions of those involved. It expects Principal Investigators (PIs) to share with other researchers, at no more than incremental cost and within a reasonable time, the data, samples, physical collections and other supporting materials created or gathered in the course of the work. It also encourages grantees to share software and inventions, once appropriate protection for them has been secured, and otherwise act to make the innovations they embody widely useful and usable.
>
> NSF program management will implement these policies, in ways appropriate to field and circumstances, through the proposal review process; through award negotiations and conditions; and through appropriate support and incentives for data cleanup, documentation, dissemination, storage and the like. Adjustments and, where essential, exceptions may be allowed to safeguard the rights of individuals and subjects, the validity of results and the integrity of collections, or to accommodate legitimate interests of investigators."

Post-award guidance can be found in NSF's *Grant Policy Manual* (GPM). Dissemination of research results is addressed in the GPM Section 734 (http://www.nsf.gov/pubs/manuals/gpm05_131/gpm05_131.pdf) and states:

> **"734 Dissemination and Sharing of Research Results**
>
> a. Investigators are expected to promptly prepare and submit for publication, with authorship that accurately reflects the contributions of those involved, all significant findings from work conducted under NSF grants. Grantees are expected to permit and encourage such publication by those actually performing that work, unless a grantee intends to publish or disseminate such findings itself.
>
> b. Investigators are expected to share with other researchers, at no more than incremental cost and within a reasonable time, the primary data, samples, physical collections and other supporting materials created or gathered in the course of work under NSF grants. Grantees are expected to encourage and facilitate such sharing. Privileged or confidential information should be released only in a form that protects the privacy of individuals and subjects involved. General adjustments and, where essential, exceptions to this sharing expectation may be specified by the funding NSF Program or Division for a particular field or discipline to safeguard the rights of individuals and subjects, the validity of results, or the integrity of collections or to accommodate the legitimate interest of

> investigators. A grantee or investigator also may request a particular adjustment or exception from the cognizant NSF Program Officer.
>
> c. Investigators and grantees are encouraged to share software and inventions created under the grant or otherwise make them or their products widely available and usable.
>
> d. NSF normally allows grantees to retain principal legal rights to intellectual property developed under NSF grants to provide incentives for development and dissemination of inventions, software and publications that can enhance their usefulness, accessibility and upkeep. Such incentives do not, however, reduce the responsibility that investigators and organizations have as members of the scientific and engineering community, to make results, data and collections available to other researchers."

As mentioned above, NSF's policies on dissemination and sharing of research results are also made a condition of the award by the NSF *Grant General Conditions* (GC-1) Article 38 (http://www.nsf.gov/publications/pub_summ.jsp?ods_key=gc1605). The text of Article 38 is as follows:

> **"38. Sharing of Findings, Data, and Other Research Products**
>
> a. NSF expects significant findings from research and education activities it supports to be promptly submitted for publication, with authorship that accurately reflects the contributions of those involved. It expects investigators to share with other researchers, at no more than incremental cost and within a reasonable time, the data, samples, physical collections and other supporting materials created or gathered in the course of the work. It also encourages grantees to share software and inventions or otherwise act to make the innovations they embody widely useful and usable.
>
> b. Adjustments and, where essential, exceptions may be allowed to safeguard the rights of individuals and subjects, the validity of results, or the integrity of collections or to accommodate legitimate interests of investigators."

Internal guidance for Foundation staff on data sharing issues is contained in the NSF *Proposal and Award Manual* (PAM). The policy is located in Chapter VII.G.2.b and the text of that section reads:

> **"Sharing of Findings, Data, and Other Research Products**
>
> The National Science Foundation advocates and encourages open scientific communication. The NSF expects significant findings from research and education activities it supports to be promptly submitted for publication, with authorship that accurately reflects the contributions of those involved. It expects investigators to share with other researchers, at no more than incremental cost and

> within a reasonable time, the data, samples, physical collections, and other supporting materials created or gathered in the course of the work. It also encourages awardees to share software and inventions or otherwise act to make the innovations they embody widely useful and usable.
>
> NSF Program management will implement these policies, in ways appropriate to the field and circumstances, through the proposal review process; through award negotiations and conditions; and through appropriate support and incentives for data cleanup, documentation, dissemination, storage, and the like. Adjustments and, where essential, exceptions may be allowed to safeguard the rights of individuals and subjects, the validity of results, or the integrity of collections or to accommodate legitimate interests of investigators."

It should also be noted that proposers must specifically address plans for the sharing of data in the Project Description section of their proposal. The *Grant Proposal Guide* Chapter II.C.2.d.(i) (http://www.nsf.gov/pubs/gpg/nsf04_23/2.jsp#IIC2d) outlines the information that must be included in each proposal submitted to NSF:

> "The Project Description should outline the general plan of work, including the broad design of activities to be undertaken, and, where appropriate, provide a clear description of experimental methods and procedures and plans for preservation, documentation, and sharing of data, samples, physical collections, curriculum materials and other related research and education products. It must describe as an integral part of the narrative, the broader impacts resulting from the proposed activities, addressing one or more of the following as appropriate for the project: how the project will integrate research and education by advancing discovery and understanding while at the same time promoting teaching, training, and learning; ways in which the proposed activity will broaden the participation of underrepresented groups (e.g., gender, ethnicity, disability, geographic, etc.); how the project will enhance the infrastructure for research and/or education, such as facilities, instrumentation, networks, and partnerships; how the results of the project will be disseminated broadly to enhance scientific and technological understanding; and potential benefits of the proposed activity to society at large. Examples illustrating activities likely to demonstrate broader impacts are available electronically on the NSF Website."

The Foundation's policies make clear to our funding recipients what their obligations are with regard to dissemination and sharing of data from NSF funded projects and NSF program management ensures these policies are implemented via a myriad number of avenues outlined above.

Question 2

List all grants and other funding awards given for research in the area of climate or paleoclimate research, including, but not limited to, the dates of the awards, the identity of the recipients, principal investigators, and whoever is contractually obligated to ensure provisions of the awards are met.

Response

Attachment 1 provides a list of all such awards from 1995 to June 15, 2005. This attachment includes the principal investigator, grantee institution, title of the grant, start date and award amount, all of which is information publicly available on NSF's Website. Under NSF's General Grant Terms and Conditions it is the responsibility of the grantee institution to insure that award provisions are met.

Question 3

Explain how NSF ensures compliance with award provisions?

Response

NSF program management implements the data sharing policies in ways that are appropriate to the field and circumstances, through the proposal review process; through award negotiations and conditions; and through appropriate support and incentives for data cleanup, documentation, dissemination, storage, and the like. Adjustments and, where essential, exceptions have been permitted to safeguard the rights of individuals and subjects, the validity of results, the integrity of collections or to accommodate legitimate interests of investigators.

In addition, if a proposer has received funding from NSF in the past, they must report in their proposal on the results of that prior NSF support. Instructions on what is required are listed in the *Grant Proposal Guide* Chapter II.C.2.d.(iii) and state:

> "If any Principal Investigator (PI) or co-PI identified on the project has received NSF funding in the past five years, information on the award(s) is required. Each PI and co-PI who has received *more than one award* (excluding amendments) must report on the award most closely related to the proposal. The following information must be provided:
>
> (a) the NSF award number, amount and period of support;
>
> (b) the title of the project;

> (c) a summary of the results of the completed work, including, for a research project, any contribution to the development of human resources in science and engineering;
>
> (d) publications resulting from the NSF award;
>
> (e) a brief description of available data, samples, physical collections and other related research products not described elsewhere; and
>
> (f) if the proposal is for renewed support, a description of the relation of the completed work to the proposed work."

As part of the review process, reviewers are asked to comment on the quality of the prior work described in this section of the proposal and this is one of the factors taken into consideration when funding recommendations are made.

Question 4

> In the area of climate or paleoclimate research, are you aware of any violation of requirements or obligations concerning the sharing and dissemination of data and research, pursuant to applicable agency and federal policies? If so, describe each violation.

Response

We are not aware of any violations of NSF policies on sharing and dissemination of paleoclimate data and research results.

Question 5

Have you received any requests for access to the research or data in studies conducted by Dr. Mann, et al., cited in the IPCC Third Assessment Report? If so, identify and provide all records relating to such requests.

Response

The only request for such research results and data of which we are aware came from Mr. Stephen McIntyre of Toronto, Canada. The information and documents responsive to this request are supplied in Attachment 2. Because this same information would have been provided under a Freedom of Information Act request, we are not asking the Committee to accord it any special treatment or confidentiality. It is also our understanding that "source code" information or data sought by Mr. McIntyre is not, and never has been, within the custody of NSF. I have been advised that even if it were, such proprietary information would normally be protected from disclosure by Exemption b(4) of the FOIA and by the Trade Secrets Act.

Question 6

Describe in detail how your agency has supported or disseminated the information developed in the Mann et al studies.

Response

NSF supported the collection and analyses of some of the paleoclimate proxy data records used by Dr. Mann and other researchers in various climate reconstructions. The NSF does not disseminate the research results from the projects it funds; rather, the investigators disseminate their results through the peer-reviewed literature, at scientific conferences, and through formal or informal lectures. Data may be made available through scientific publications, the investigator's institution, and/or through publicly accessible data repositories.

Question 7

Does your agency consider the IPCC review and writing process for the TAR to be consistent with your agency's policy or guidelines relating to information or data quality? Explain where the review and writing process is consistent or inconsistent in this regard.

Response

> NSF policies and guidelines do not address how other individuals or organizations *use* data disseminated by our grantees.

I hope you find this response meets your needs. If you have questions or need any additional information, please contact David Stonner in our Office of Legislative and Public Affairs at (703) 292-8070.

Sincerely,

Arden L. Bement, Jr.
Director

cc: The Honorable John Dingell, Ranking Member
The Honorable Bart Stupak, Ranking Member,
Subcommittee on Oversight and Investigations

The Honorable Sherwood L. Boehlert, Chairman
The Honorable Bart Gordon, Ranking Member,
Committee on Science

TAB 6

THE M&M CRITIQUE OF THE MBH98 NORTHERN HEMISPHERE CLIMATE INDEX: UPDATE AND IMPLICATIONS

Stephen McIntyre
512–120 Adelaide St. West, Toronto, Ontario Canada M5H 1T1;
Ross McKitrick
Department of Economics, University of Guelph, Guelph Ontario Canada N1G2W1.

ABSTRACT

The differences between the results of *McIntyre and McKitrick* [2003] and *Mann et al.* [1998] can be reconciled by only two series: the Gaspé cedar ring width series and the first principal component (PC1) from the North American tree ring network. We show that in each case MBH98 methodology differed from what was stated in print and the differences resulted in lower early 15th century index values. In the case of the North American PC1, MBH98 modified the PC algorithm so that the calculation was no longer centered, but claimed that the calculation was "conventional". The modification caused the PC1 to be dominated by a subset of bristlecone pine ring width series which are widely doubted to be reliable temperature proxies. In the case of the Gaspé cedars, MBH98 did not use archived data, but made an extrapolation, unique within the corpus of over 350 series, and misrepresented the start date of the series. The recent Corrigendum by Mann *et al.* denied that these differences between the stated methods and actual methods have any effect, a claim we show is false. We also refute the various arguments by Mann et al. purporting to salvage their reconstruction, including their claims of robustness and statistical skill. Finally, we comment on several policy issues arising from this controversy: the lack of consistent requirements for disclosure of data and methods in paleoclimate journals, and the need to recognize the limitations of journal peer review as a quality control standard when scientific studies are used for public policy.

1. INTRODUCTION

The Northern Hemisphere temperature index of *Mann et al. [1998, "MBH98"]*, together with its extension in *Mann et al. [1999]*, was adopted by the Intergovernmental Panel on Climate Change *[IPCC, 2001]* as the canonical temperature history of the Northern Hemisphere. It is the authority for claims that the 1990s were the warmest decade of the millennium and its influence on the public's attitude towards climate change and climate change policy has been enormous, and was recently reinforced by its usage in the Arctic Climate Impact Assessment *[ACIA, 2004*].

In *McIntyre and McKitrick [2003*, "MM03"], we attempted to replicate the results of MBH98 and encountered many data and methodological problems, some of which had a significant effect on the central MBH98 conclusions concerning the uniqueness of the late-20th century climate. In response to MM03 and subsequent submissions and correspondence to *Nature*, Mann et al. have provided new information about MBH98, including an extensive archive of data and methods at the Supplementary Information (the "Corrigendum SI") to *Mann et. al., [2003, 2004a, 2004c*, the "Corrigendum"], an extensive archive of data and methods at a University of Virginia FTP site, *[Mann, 2002-2004]* and various written responses to our work *[Mann et al. 2003, 2004a, 200b, 2004d]*. Unfortunately Mann et al. have refused to provide the source code used to generate their results, other than the limited (but essential) programs used for tree ring principal components (PCs). They have also refused to provide supporting calculations for the individual calculation steps in MBH98, especially the controversial step from 1400-1450 (the "AD1400 step"). We made unsuccessful appeals to both *Nature* and the U.S. National Science Foundation, which funded MBH98, to compel release of this material.

Because of this obstruction, not all the problems in MBH98 can be resolved. However, we believe that we have sufficient information in hand to:

(1) completely reconcile the differing results of MM03 and MBH98;
(2) establish the non-robustness of MBH98;
(3) reject the temperature reconstruction in MBH98.

The results presented here do not contradict the results of MM03, but are a logical development of the issues first raised therein.

In order to establish our results, we have attempted to emulate all aspects of MBH98. Much of the controversy in the response to our first article on MBH98 pertained to the accuracy of our emulation. We have followed all published information on the MBH98 procedures, and any remaining differences likely cannot be addressed without disclosure of the actual MBH98 code. However, none of the points established herein are affected by the remaining secrecy surrounding MBH98 computational details (for full details of our emulation, including R code, see the Supplementary Information). We anticipate that there will be critical interest in the emulation itself and we will address these matters in a separate paper.

One of the points of view advocated in this article is that individual data series matter in the MBH98 results. We disagree with the view that problems with individual series simply get washed out in a multiproxy study. In the context of the MBH98 methodology this optimistic assumption is untenable.

Section 2 explains the sources of difference between MM03 and MBH98. Section 3 considers the issue of the robustness of MBH98 results. Section 4 discusses particular issues concerning bristlecone pine and cedar proxies, which are central to the matters in this paper. Section 5 deals with some remaining counter-arguments from Mann *et al.* and Section 6 offers concluding comments.

2. RECONCILING MM03 AND MBH98

Differences between MM03 results and MBH98-type results can be reconciled through variations in the handling of only two series, the Gaspé "northern treeline" series and the first principal component (PC1) from the North American proxy roster (NOAMER). The changes are illustrated in Figure 1 below.

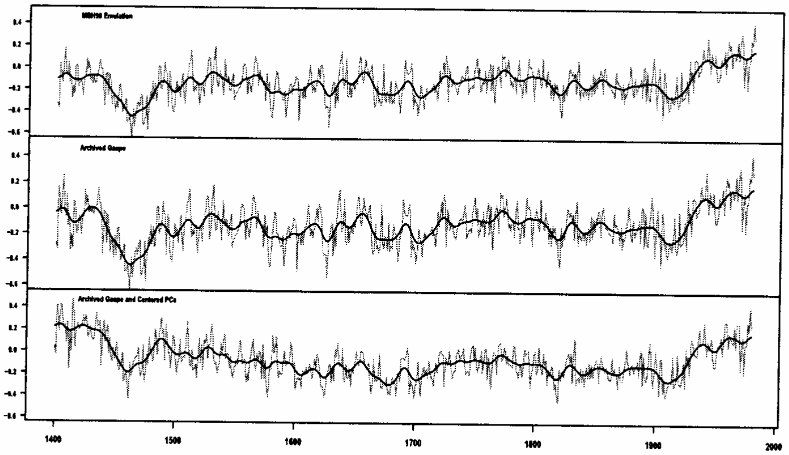


Figure 1. NH Temperature Index. Top panel: MBH98 emulation; middle panel: using archived Gaspé version; bottom panel: using centered PC algorithm.

The top panel shows our emulation (version 3) of MBH98, which implements new information from the Corrigendum SI. Relative to the MBH98 reconstruction, it has a Reduction of Error ("RE") statistic of 0.83 in the 1400–1901 period (R^2 – 0.68) – both values lower than our version 2 emultion without the Corrigendum SI information.

The middle panel ("Archived Gaspé") shows the effect of merely using the version of the Gaspé series archived at WDCP, rather than the version as modified by MBH98, accounting for a material change in the early 15th century. The only difference between the two series is the extrapolation of the first four years in MBH98. Under MBH98 methods, a series had to be present at the start of a calculation step in order to be included in the interval roster. In only one case in the entire MBH98 corpus was this rule broken – where the Gaspé series was extrapolated in its early portion, with the convenient result of depressing early 15th century results. This extrapolation was not disclosed in MBH98, although it is now acknowledged in the Corrigendum *[Mann et al., 2004c]*. In MBH98, the start date of this series was misrepresented; we discovered the unique extrapolation only by comparing data as used to archived data. There are other considerations making this unique extrapolation singularly questionable. The Gaspé series is already included in the NOAMER principal components network (as cana036) and thus appears twice in the MBH98 data set, and the extrapolation, curiously, is only applied to one of the columns. The underlying dataset is based on only one tree up to 1421 and only 2 trees up to 1447. *Jones and Mann [2004]* point to the need for "circumspect use" of tree ring sites with few early examples. The early portion of the series fails standard minimum signal criteria [e.g. *Wigley et al. 1984]* and indeed fails the data quality standards Mann et. al. themselves listed elsewhere. The early portion of the series was not used by the originating authors *[Jacoby and d'Arrigo, 1989; D'Arrigo and Jacoby, 1992]*, whose analysis only begins effective 1601. In fact, *Jones and Mann [2004]* do not use the Gaspé series as an individual proxy and only use the Jacoby-d'Arrigo northern treeline composite when it is adequately replicated after 1601.

The bottom panel ("Archived Gaspé and Centered PCs") shows the additional effect of using conventional (centered) PC methods and is virtually identical to MM03. MBH98 had stated that they used "conventional" principal components (PC) calculations. A conventional PC calculation applies standardization in which the columns are centered by subtracting their mean, which is done for the calculations in the bottom panel. Stepwise PC series are used (extending the NOAMER network back to AD1400), thereby avoiding the main criticism leveled against MM03.

Once again, MBH98 contained a misrepresentation, this time about their PC method. After the University of Virginia FTP site was made publicly available following MM03, by examining PC series archived there and, by examining source code for PC calculations,[1] we were able to determine that MBH98 had not carried out a "conventional" PC calculation, but had modified the PC algorithm, by, among other things, subtracting the 1902-1980 mean, rather than the 1400-1980 column mean, prior to PC calculations, so that the columns were no longer centered on a zero mean in the 1400–1980 step. By this procedure, series are more decentered, and their variance more inflated, the larger is the difference between the series mean and the mean of the 20th century subset. The effect of this transformation would have been mitigated if they had carried out a singular value decomposition on the covariance matrix, but they carried it out on the de-centered data matrix. We have shown elsewhere that this method re-allocates variance so that the PC algorithm then strongly over-weights hockey stick-shaped proxies and that it is so efficient in mining a hockey stick shape that it nearly always produces a hockey-stick shaped PC1 even from persistent red noise [*McIntyre and McKitrick, 2005;* discussed in *Muller, 2004*].

This observation has received a considerable amount of publicity and some observers have misunderstood the point. While we have made scripts available at our FTP site, for greater certainty, we show here the relevant R functions, which were used

---

[1] See weights in ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/pca-noamer.f. This program has 588 lines of Fortran code, of which the first 323 lines are particular to the PC calculations and the final 265 lines is an SVD routine. The 1902–1980 mean is removed in lines 168–173 as follows:

```
168 c  remove 1902–19xx mean from training data
169 c
170    do i=nlow,nhigh
171      iyear = i-nlow+1
172     aprox(iyear,j)=aprox(iyear,j)-roxave(j)
173     end do
```

for these results and which show algorithmically exactly what Mann and we are doing. We emphasize that we are able to exactly replicate Mann's PC calculations with these scripts and that, in unpublished material at *Nature*, Mann has replicated our PC calculations. Although many readers may not be familiar with R *[R Development Core Team, 2004]*, we strongly believe that the provision of source code in the actual language is an essential part of ensuring replicable results and that there is sufficient commonality in source codes that the following code will illuminate the issues even for persons who are unfamiliar with the language. We have also chosen to highlight source code in the running text rather than a footnote, because source code issues turn out to be an essential finding. The detrended standard deviation was calculated with the following function:

```
sd.detrend<-function(x) { t<-c(1:length(x)) ; fm<-lm(x~t); sd.detrend<-
    sd(fm$residuals); sd.detrend }
```

The MBH98 transformation was calculated with the following function:

```
mannomatic<-function(x, M=78) {N<-length(x);  xstd<- (x- mean( x[(N-
    M):N]))/sd(x[(N-M):N]);
sdprox<-sd.detrend(xstd[(N-M):N]); mannomatic<- xstd/sdprox; mannomatic }
```

The main effect of this transformation is through the de-centering, rather than the difference between detrended and undetrended standard deviations, which, in this case, is empirically rather slight. A default value of 78 for M is used to simplify use for the 1902–1980 calibration period, which is M+1 years long.

The North American tree ring network for the AD1400 step was collected into one matrix *Tree*, in this case of dimension 581x70. A matrix *Tree.mannomatic* transformed according to MBH98 was obtained through applying the above function to the matrix as follows:

```
Tree.mannomatic<-apply(Tree,2,mannomatic)
```

By applying the svd function in R, a singular value decomposition corresponding exactly to the archived results (eigenvalues, eigenvectors and PC series) at the University of Virginia FTP site[2] was obtained:

```
PCA.mannomatic.svd<-svd(Tree.mannomatic)
```

We have reported that this algorithm nearly always yields hockey-stick shaped series from persistent red noise networks [*McIntyre and McKitrick, 2005;* also see below]. In response, some readers have expressed incredulity about whether our methods accurately reflect actual MBH98 methods. For this specific point – the replication of tree ring PC calculations, we re-iterate that we have *exact* replication of MBH98 PCs and that the above method, however implausible it may seem on first principles, is the method that was used in MBH98 tree ring PC calculations.

---

[2] Directory <ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/BACKTO_1400>, PCs at pc01.out, …; eigenvectors at eof01.out,…eigen.out contains percentage variance for each eigenvalue of total variance rather than actual eigenvalues.

While PC algorithms are related to SVD algorithms, they are not exactly the same. We were able to replicate the above results with a PC algorithm, only by specifying an uncentered option as follows:

```
PCA.mannomatic.prcomp<-prcomp(Tree.mannomatic, center=FALSE)
```

A different protocol is used for reporting eigenvalue information in the *svd* and *prcomp* algorithms, but the results are identical, allowing for the protocol.[3]

The method which we used in MM03, MM04a and MM04b can be shown by the corresponding command, displaying two differences clearly – not using the transformed data; and the use of a *centered* method.

```
PCA.centered<-prcomp(Tree, center=TRUE)
```

The default value is center=TRUE and the result would also have been realized by:

```
PCA.centered<-prcomp(Tree)
```

A centered calculation is clearly what one would expect in a "conventional" calculation. A centered calculation on the de-centered matrix is a possible variation, which can be implemented through:

```
PCA.mannomatic.centered<-prcomp(Tree.mannomatic, center=TRUE)
```

In this case, the calculation is done on the covariance matrix of the transformed data and produces an intermediate result (in terms of the hockey stick shape of the PC1).

We see no advantage to the MBH98 approach of using hundreds of lines of Fortran text to carry out the above functions, thereby opening up the possibility of error, since it can be easily done in a few lines of high-level programming languages, as shown above. While the MBH98 procedure may have originated as a programming error, the Corrigendum did not admit any error and seemed to take the position that the above method was intentional (even though it was undisclosed and tends to produce hockey sticks). Be that as it may, the key difference turns out to be not the stepwise principal components method, as claimed in *Mann et al. [2003],* but the use of an uncentered algorithm on de-centered data. Together with the MBH98 use of a non-archived version of the Gaspé series (containing a unique extrapolation), this fully reconciles MM03 and MBH98 results.

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[3] PCA.mannomatic.svd$d = PCA.mannomatic.prcomp$sdev *sqrt (N-1), where N is the number of years in the calculation. PCA.mannomatic.svd$u[,k]=PCA.mannomatic.prcomp$x[,k]/(sqrt(N-1) *PCA.mannomatic.prcomp$sdev[k]).

3. EFFECT OF SLIGHT VARIATIONS ON 15TH CENTURY TEMPERATURE RESULTS

We presented these results in a slightly different form in *McIntyre and McKitrick [2004a, 2004b]*. In response to these results (and to MM03), *Mann et al. [2004a, 2004b, 2004d]* argued that they can still obtain MBH98-type results under other conditions. While we differ with Mann et al. on the issue of which methodological assumptions are "correct", if the assumptions are specified sufficiently precisely, there is surprising consensus on the actual effects. Slight variations of methods and data lead on the one hand to MM-type results (with a 15th century higher than the late 20th century) or on the other hand to MBH-type results (with a 15th century lower than the late 20th). These can be summarized as follows.

Variants on the NOAMER PC1 (After Removing the Gaspé Series Extrapolation)

- In the MBH98 de-centered PC calculation, a small group of 20 primarily bristlecone pine sites, all but one of which were collected by Donald Graybill and which exhibit an unexplained 20th century growth spurt (see Section 5 below), dominate the PC1. Only 14 such chronologies account for over 93% of the variance in the PC1,[4] effectively omitting the influence of the other 56 proxies in the network. The PC1 in turn accounts for 38% of the total variance. In a centered calculation on the same data, the influence of the bristlecone pines drops to the PC4 (pointed out in *Mann et al., 2004b, 2004d)*. The PC4 in a centered calculation only accounts for only about 8% of the total variance, which can be seen in calculations by Mann et al. in Figure 1 of *Mann et al. [2004d]*.
- If a centered PC calculation on the North American network is carried out (as we advocate), then MM-type results occur if the first 2 NOAMER PCs are used in the AD1400 network (the number as used in MBH98), while MBH-type results occur if the NOAMER network is expanded to 5 PCs in the AD1400 segment (as proposed in *Mann et al., 2004b, 2004d)*. Specifically, MBH-type results occur as long as the PC4 is retained, while MM-type results occur in any combination which excludes the PC4. Hence their conclusion about the uniqueness of the late 20th century climate hinges on the inclusion of a low-order PC series that only accounts for 8 percent of the variance of one proxy roster.
- If de-centered PC calculation is carried out (as in MBH98), then MM-type results still occur regardless of the presence or absence of the PC4 if the bristlecone pine sites are excluded, while MBH-type results occur if bristlecone pine sites (and PC4) are included. Mann's FTP site *[Mann, 2002–2004]* actually contains a sensitivity study on the effect of excluding 20 bristlecone pine sites[5] in which this adverse finding was discovered, but the results were not reported or stated publicly and could be discerned within the FTP site only with persistent detective work.
- If the data are transformed as in MBH98, but the principal components are calculated on the covariance matrix, rather than directly on the de-centered data, the

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[4] See <ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/BACKTO_1400/eof01.out>

[5] See <ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB.NOAMER/BACKTO_1400-CENSORED>

results move about halfway from MBH to MM. If the data are not transformed (MM), but the principal components are calculated on the correlation matrix rather than the covariance matrix, the results move part way from MM to MBH, with bristlecone pine data moving up from the PC4 to influence the PC2. In no case other than MBH98 do the bristlecone series influence PC1, ruling out their interpretation as the "dominant component of variance" *[Mann et al, 2004b]*
- If no North American PC1 is used at all in the AD1400 calculations (which occurs if PC calculations are done over the maximum period in which all sites are available, as done in MM03), then MM-type results occur under both centered and decentered PC calculations, with and without bristlecone pines.

Variants on the Gaspé Series (After Applying Centered PC Method on NOAMER)

- If the archived version of the Gaspé series is used, MM-type results occur. If the early (pre-1447) portion of the site chronology with less than 3 trees is not used [see discussion in *Jones and Mann, 2004*], MM-type results occur. If the duplicate version of the Gaspé series used as an individual proxy is not used (while continuing the use of the Gaspé series in the NOAMER network with or without the extrapolation), MM-type results occur. MBH-type results occur only if a duplicate version of the Gaspé series is used as an individual proxy and the portion of the site chronology with 1–2 trees is used and if the first four years of the chronology are extrapolated under an ad hoc procedure not otherwise used in MBH98. *Mann et al. [2004a, 2004b]* justified the extrapolation as a means of maintaining representation of northern treeline series in this interval. If representation is achieved by use of the updated version of the Sheenjek River series (which meets replication standards in the 15th century), then MM-type results occur.

Variants on the Entire Procedure

- If, as is suggested in *Mann et al. [2004a, 2004b]*, no PC calculations are applied to the North American and Stahle/SWM networks and the sites are instead used as individual proxies (while otherwise carrying on with MBH98 methods), then MBH-type results are obtained regardless of whether the Gaspé series is duplicated or extrapolated. In this case, the MBH temperature reconstruction becomes little more than an index of bristlecone pine growth. However, if the bristlecone pine sites are excluded from this network, then MM-type results are obtained.

We emphasize the consensus between ourselves and Mann et al. on the results of sufficiently well-defined calculations. The PC calculations themselves are replicated between parties to complete accuracy. Differences remain in the emulations of NH temperature (given the PC series), but *Mann et al. [2003]* showed a calculation with high early 15th century results if the North American PC1 were unavailable; the comments in *Mann et al. [2004b]* about the effect of the PC4 confirm this overall agreement if assumptions are sufficiently well defined.

These results also show that the effects of individual series are not necessarily washed out in a multiproxy method of MBH98 type, contrary to suggestions in *Zorita et al. [2003]*[6] and *von Storch et al. [2004]*[7].

In response to a reader's suggestion, we performed a sensitivity test in which we arbitrarily increased the ring widths of all non-Graybill (50 of 70) sites by +0.5 (index units) in the first half of the 15th century, and then re-calculated the PC1 under MBH98 methodology. The purpose is to evaluate how well the added variance is retained in the final temperature index. We provide the exact script here both to describe the calculation exactly and because the results are initially very counter-intuitive and have provoked some disbelief.

```
Tree.adj<- Tree                                     #creates mirror object for testing
Tree.adj[1:50,!graybill]<- Tree.adj[1:50,!graybill]+0.5  # adds 0.5 to all non-Graybill sites (mean is 1)
Tree.adj.mannomatic<- apply(Tree.adj,2, mannomatic) # applies MBH98 transformation to columns
PCA.adj.mannomatic<-svd(Tree.adj.mannomatic)        #svd on data matrix
PC1.adj.mannomatic<- PCA.adj.mannomatic$u [,1]      #selects PC1 from svd model
```

The results of this calculation are shown in Figure 2 together with the results from a centered calculation (all results smoothed). For a centered calculation, the increased ring widths for the first 50 years lead to an increase in the PC1 as expected. However, under the MBH98 de-centered method the *increased* ring widths for 50 non-Graybill sites in the early 15th century causes a significant *decrease* (!) in the early 15th century PC1. *Carried forward through to Northern Hemisphere temperature calculations, these increased ring widths would be construed by the MBH98 method as evidence of* ***colder*** *temperatures in the early 15th century.*

This rather perverse result nicely illustrates a problem of mechanically applying a numerical algorithm like PC analysis without regard to whether it makes sense for the underlying physical process. PC methods are indifferent to the orientation (up or down) of a series – the difference is merely the presence or absence of a negative sign. A vivid example in this context is the archived PC1 for *Mann et al. [1999]*,[8] which is upside-down as archived, but which was flipped for presentation purposes in *Mann et al. [1999]*. Under the MBH98 algorithm, the addition of the extra values in the first half of the 15th century causes the algorithm to flip the series upside-down so that they match as well as possible to the bristlecone pines, whose hockey stick pattern is

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[6] "MBH98's method yields an estimation of the value of the temperature PCs that is optimal for the set of climate indicators as a whole, so that the estimations of individual PCs cannot be traced back to a particular subset of indicators or to an individual climate indicator. This reconstruction method offers the advantage that possible errors in particular indicators are not critical, since the signal is extracted from all the indicators simultaneously."

[7] "The optimized temperature fields target the whole available proxy network at a given time, so that the inclusion of a few instrumental data sets in the network should have little influence on the estimated fields, unless the instrumental records are explicitly overweighted. The advantage is that the method is robust against very noisy local records. This contrasts with direct regression methods, where the estimated temperature fields are the predictands of a regression equation. In this case a few instrumental records, highly correlated to the temperature fields, may overwhelm the influence of proxy records with lower correlations in the calibration period."

[8] ftp://ftp.ngdc.noaa.gov/paleo/contributions_by_author/mann1999/proxies/itrdb-namer-pc1.dat>.

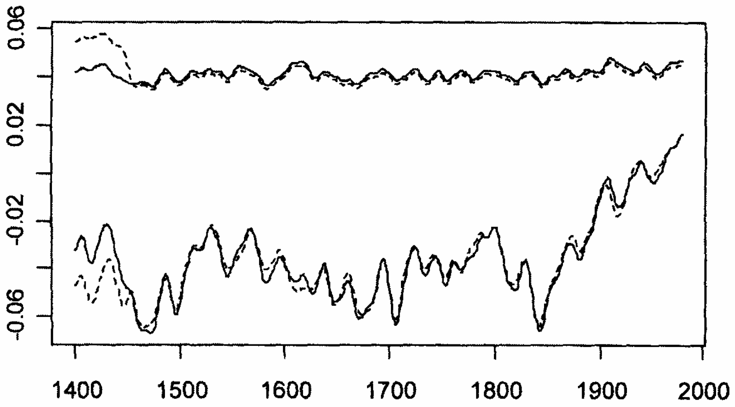

Figure 2. North American AD1400 PC1. Above: PC1 using centered calculations; below: MBH98 PC1 using decentered method. Solid-base case; dashed – with arbitrary addition of 0.5 to non-Graybill sites from 1400–1450. 25-year smoothing is applied.

imprinted on the PC1. This does not occur using a centered algorithm.

4. ROBUSTNESS

The sensitivity of 15th century results to such slight variations of method and data show a fundamental instability in MBH98 results, related especially to the presence or absence of bristlecone pines and Gaspé cedars. This flatly contradicts some claims by Mann et al. about the robustness of MBH98 results. MBH98 stated the following:

> **the long-term trend in NH is relatively robust to the inclusion of dendroclimatic indicators in the network,** suggesting that potential tree growth trend biases are not influential in the multiproxy climate reconstructions. (p. 783, emphasis added)

This was stated in even stronger, and equally misleading, terms in *Mann et al. [2000]* as follows:

> We have also verified that possible low-frequency bias due to non-climatic influences on dendroclimatic (tree-ring) indicators is not problematic in our temperature reconstructions....**Whether we use all data, exclude tree rings, or base a reconstruction only on tree rings, has no significant effect on the form of the reconstruction for the period in question.** ... These comparisons show no evidence that the possible biases inherent to tree-ring (alone) based studies

> impair in any significant way the multiproxy-based temperature pattern reconstructions discussed here.
> (http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html, emphasis added)

The synopsis of results in Section 3 effectively disproves these claims, regardless of the point of view that one may take on questions such as whether 2 PCs or 5 PCs is "correct" for the AD1400 North American network. Each of the permutations discussed above is a sensitivity test much less draconian than excluding all tree rings. Both the Gaspé cedar series and the bristlecone pine series are obviously subsets of the dendroclimatic indicators and each has a significant effect on the 15th century results, as indeed do the specific methodological decisions (extrapolation, decentered PC calculations), which enhance the effect of these series.

Figure 3 may be helpful in illustrating exactly why these two series have such a dramatic impact on early 15th century results. The left panel is a scatterplot as follows. For each of the 22 proxies in the AD1400 roster we computed the correlation between each proxy and the temperature PC1 over the 1902–1980 interval (x-axis), and the difference between the 1902–1980 mean and the 1400–1450 mean, divided by the 1400–1980 standard deviation (y-axis). The 1902–1980 interval is the MBH98 calibration period and is the interval over which the mean is computed in the PC decentering. It can be shown that the PC weights for each proxy in the AD1400 network are closely related to the correlation with the temperature PC1. The difference of means is a measure of "MBH-ness" – series with a zero value are flat, while those with an absolute value in excess of 1 have a a hockey stick shape (sometimes upside-up and sometimes upside-down). The two points in the top right hand corner represent

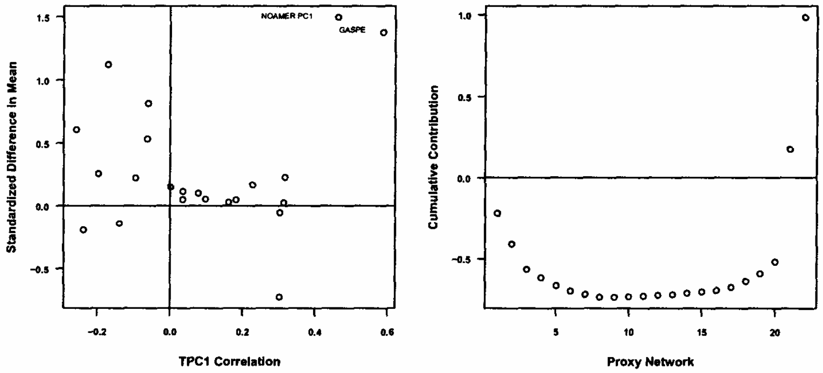

Figure 3. Left: Scatterplot with x-axis: correlation between AD1400 step proxies and the temperature PC1 in the calibration period 1902–1980; y-axis: difference between 1902–1980 mean and 1400–1450 mean divided by 1400–1980 standard deviation. right: cumulative contribution to standardized difference between 1902–1980 mean and 1400–1450 mean.

the MBH98 NOAMER PC1 and Gaspé tree ring series. Except for these two points there is an overall *negative* relationship between the difference of means and the correlation with temperature: i.e. hockey stick series fit the temperature data relatively poorly in the calibration interval. But the NOAMER PC1 and Gaspé series are such influential outliers that they reverse this pattern for the model as a whole.

In the right panel the 22 series in the AD1400 step are introduced sequentially into the multiproxy calibration model, with the Gaspé and NOAMER PC1 series introduced last. The standardized difference between the 1902–1980 mean NH temperature and the 1400–1450 mean NH temperature is computed at each step. The relatively high 1902–1980 temperature in MBH98 (i.e. the hockey stick shape) results entirely from the contributions of the two final, outlier values.

If the same calculations are carried out using centered principal components calculations and the Sheenjek River series is used as a northern treeline proxy instead of the Gaspé series, as represented in Figure 4, there are no longer two outlier series, resulting in the 1400–1450 mean temperature being higher than the 1902–1980 mean temperature.

Some consternation has been expressed by critics of MM03 that its high early 15th century values are inconsistent with other supposedly independent temperature reconstructions. However, the MM results are obtained from the same underlying proxy set as MBH98. The influence of the two outlier series can be seen in a different way in Figure 5, which shows a simple comparison of the mean of Gaspé and NOAMER PC1 against a weighted average of 6 series used in the AD1400 network

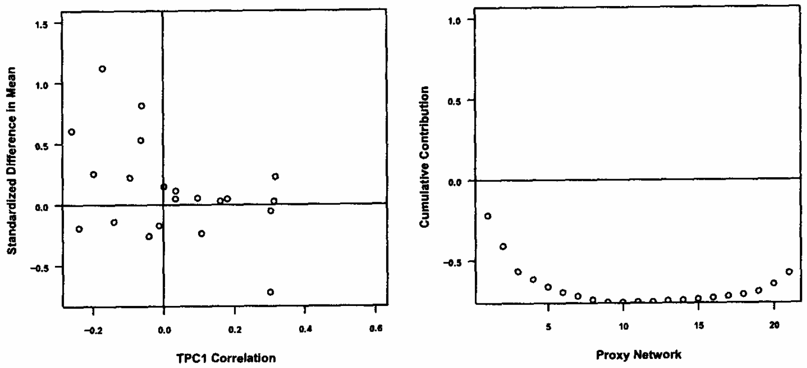

Figure 4. As with Figure 3, but with centered PC calculations and Sheenjek River. Left: Scatterplot with x-axis: correlation between AD1400 step proxies and the temperature PC1 in the calibration period 1902–1980; y-axis: difference between 1902–1980 mean and 1400–1450 mean divided by 1400–1980 standard deviation. right: cumulative contribution to standardized difference between 1902–1980 mean and 1400–1450 mean. The 1400–1450 mean temperature is now higher than the 1902–1980 mean temperature.

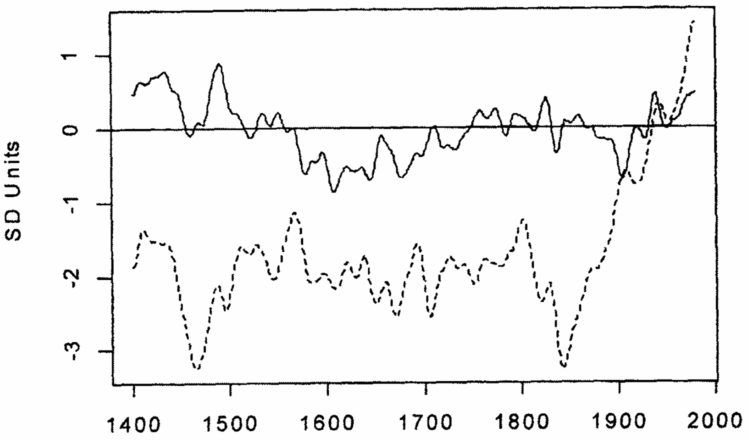

Figure 5. Solid: Weighted average of 6 MBH98 AD1400 step proxies: 4 Quelccaya series (averaged to one series), Tornetrask temperature reconstruction and Tasmania temperature reconstruction; Dashed – average of MBH98 NOAMER PC1 and Gaspé. All series smoothed with 25-year Gaussian filter.

(and often used in other multiproxy studies). The weights are calculated as follows: equal weight is given to the Tornetrask and Tasmania series, while each of the 4 constituent Quelccaya series are given a quarter-weight, reversing the sign for the accumulation series. All series are standardized. The strong negative bias of the two outlier series is evident, as is the closer relationship of the 6 series average to the MM-type reconstruction.

5. BRISTLECONE PINES AND GASPÉ CEDARS

Although considerable publicity has attached to our demonstration that the PC methods used in MBH98 nearly always produce hockey sticks, we are equally concerned about the validity of series so selected for over-weighting as temperature proxies. While our attention was drawn to bristlecone pines (and to Gaspé cedars) by methodological artifices in MBH98, ultimately, the more important issue is the validity of the proxies themselves. This applies particularly for the 1000–1399 extension of MBH98 contained in *Mann et al. [1999]*. In this case, because of the reduction in the number of sites, the majority of sites in the AD1000 network end up being bristlecone pine sites, which dominate the PC1 in *Mann et al. [1999]* simply because of their longevity, not through a mathematical artifice (as in MBH98).

Given the pivotal dependence of MBH98 results on bristlecone pines and Gaspé cedars, one would have thought that there would be copious literature proving the validity of these indicators as temperature proxies. Instead the specialist literature only raises questions about each indicator which need to be resolved prior to using them as temperature proxies at all, let alone considering them as uniquely accurate stenographs of the world's temperature history.

5.1 Bristlecone Pines

There has been an undoubted increase in bristlecone pine ring widths in the 20th century. *Graybill and Idso [1993]* explicitly stated it is greater than could be explained by temperature. Ironically, *Mann et al. [1999]* (referring to the bristlecone pine sites) admits the same thing:

> A number of the highest elevation chronologies in the western U.S. do appear, however, to have exhibited long-term growth increases that are more dramatic than can be explained by instrumental temperature trends in these regions. (p. 760)

The anomalous 20th century growth rate for bristlecone pines is illustrated in Figure 6, which compares the standardized MBH98 PC1 (dominated by bristlecone pines) to the *Briffa et al. [1992a]* North American temperature reconstruction (using tree rings from many species), which is used in MBH98 itself, as well as *Jones and Bradley [1993]*, *Jones et al. [1998]* and *Jones and Mann [2004]*. There is little visual relationship. The correlation between the two series in the MBH98 calibration period of 1902–1980 is 0; the RE statistic for the MBH98 PC1 as a predictor for the Briffa temperature reconstruction in a verification period of 1600–1901 is –7.7, showing no skill whatsoever. Thus, whatever "dominant component of variance" *[Mann et al., 2004a]* is supposedly captured in the MBH98 PC1 has apparently escaped detection in the Briffa reconstruction. The strong negative bias of the MBH98 PC1 is evident in comparison to the Briffa reconstruction. The strong negative bias of the MBH98 PC1 is also evident in periods where we have instrumental records in North America. There is no reason to believe that average temperatures in the 18th century were negative 3 standard deviation units.

Despite the reliance of MBH98 on the North American PC1, the validity of this series as a temperature proxy was not independently established in peer-reviewed literature. Co-author Hughes stated later *[Hughes and Funkhouser, 2003]* that the anomalous growth rate of bristlecone pines was a "mystery", which should have raised questions about the PC1. The strong difference between the Briffa re-construction, comprised of many species, and the MBH98 PC1 (representing only bristlecone pines) should also have raised questions about whether there may be species-particular effects related to any of the numerous unusual features of bristlecone pines.

We surveyed the literature on bristlecone pines and report here on many peculiarities pertaining to this species, which should be clearly addressed prior to relying on the MBH98 PC1 for policy purposes.

Bristlecones are famously long-lived, but despite this, do not appear to senesce *[Lanner and Connor, 2001; Connor and Lanner, 1991]*. They occur in an unusual strip bark form, where the bark in most long-lived trees dies around the circumference except for a small strip on one side. Unlike most pines, they continue to respire during the winter thereby consuming photosynthate *[Schulze et al., 1967]*.

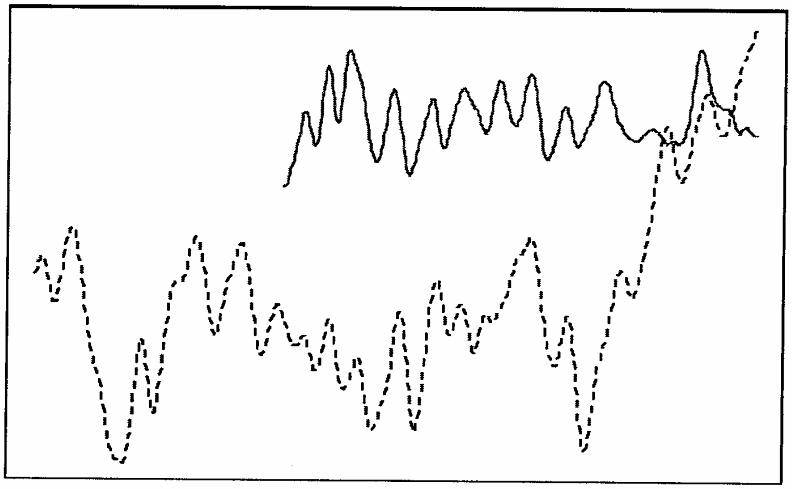

Figure 6. Solid: North American temperature reconstruction of Briffa et al. [1992a]. Dashed: the MBH98 North American PC1. All series smoothed with a 25-year Gaussian filter. Both series are standardized by subtracting the 1902–1980 mean and dividing by the 1902–1980 standard deviation (re-scaling is not an issue here, since the PC calculations have already been done.)

Tree lines at bristlecone pine sites were significantly higher in the past, especially during the Holocene Optimum about 6000 years ago, but also during the medieval period *[LaMarche and Mooney, 1967; LaMarche, 1973]*. In 1957, bristlecone pines in the Campito Mountain area were not reproducing *[Billings and Thompson, 1957]*, indicating that more favourable conditions than the 1950s were required for bristlecones pine to become seedlings.

The Sheep Mountain series, ca534, is the top-weighted series in the MBH98 NOAMER PC1[1] receiving 390 times the weight of the least-weighted series. *LaMarche, Fritts, Graybill and Rose [1984]* (all authors listed for emphasis) argued that the anomalous 20th century growth in bristlecone pines was attributable to CO_2 fertilization, using Sheep Mountain as an example. In the 1980s, Donald Graybill followed up at the other bristlecone pine sites, collecting the samples which later comprised the NOAMER PC1. Graybill specifically sought out strip bark samples and reported that strip bark forms had much stronger 20th century growth than entire bark forms at the same site *[Graybill and Idso, 1993]*. More recently, *Bunn et al. [2003]* confirmed higher growth in strip bark forms than entire bark forms at sites in Montana.

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[1] ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/BACK TO_1400/ECOF01.OUT and series identifications in ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRD /NOAMER/ BACTO_1400/noamer-itrdb-ad1400

Brooks et al. [1996] also pointed out the impact of anthropogenic nitrogen on fertilization of high-altitude bristlecone pines, stating that:

> At these high-elevation catchments there has been a shift in ecosystem dynamics from an N-limited system to an N-saturated system as a result of anthropogenically-fixed N in wetfall and dryfall. Results from the Western Lakes Survey component of the National Surface Water Survey show that N saturation is a regional problem in the Colorado Front Range, with many lakes having (NO3-) concentrations greater than 10 meq/L. Foliar N to P ratios in Bristlecone Pine increase with elevation in the Colorado Front Range, indicating that at higher elevations P is translocated from foliar tissue more efficiently than N and that increasing atmospheric deposition of N with elevation is causing a change from N limitation to P limitation in the highest-elevation Bristlecone Pines.

Graybill and Idso [1993] attributed the anomalous 20th century growth of strip-bark forms to CO_2 fertilization There are some possible reasons why CO_2 fertilization may affect high-altitude strip bark forms more strongly than lower-altitude entire-bark forms and there is specific evidence for CO_2 fertilization for vegetation from the White Mountains, California, where important bristlecone pine stands are located *[Mooney et al., 1964]*. The response to changes of CO_2 concentration in controlled experiments is strongly non-linear and attenuates as CO_2 levels increase. CO_2 levels at the high altitudes of bristlecone pines (3000–3500 m) are significantly lower than at sea level and, at the lower CO_2 levels at high-altitude, the response to increased CO_2 levels is in a range with stronger response.

The general hypothesis of CO2 fertilization of tree growth has been hotly contested. Opponents [e.g. *Jacoby and D'Arrigo, 1997 and Graumlich, 1991]* have pointed to other sites which do not exhibit anomalous 20th century growth and argued that the anomalous growth effect is limited to high-altitude strip bark forms. More recently, *Schauer et al. [2001]* reported that trees with strip-bark have significantly lower pre-dawn water potentials during the driest part of the growing season relative to non-strip-barked trees. So the anomalous growth of strip bark trees may be related to effects differing from CO_2.

Whatever side one takes on the CO_2 fertilization debate is really immaterial. Even if one adopts the position of Jacoby and Graumlich – that the effect is limited to strip bark forms – since the NOAMER PC1 is comprised almost entirely of strip-bark forms it could easily be affected as a proxy, even under the limited position taken by Jacoby and Graumlich.

Mann et al. [1999] purported to adjust the NOAMER PC1 for CO_2 fertilization, by coercing the shape of the NOAMER PC1 to the Jacoby northern treeline reconstruction in the 1750–1980 period, arguing that the northern treeline series would not be affected by CO_2 levels. Once one gets into such ad hoc adjustments, many new questions need to be answered about the validity of the adjustment procedure. In the actual *Mann et al. [1999]* adjustment, the main adjustment for "CO_2 fertilization" takes place in the 19th century rather than the 20th century, with *Mann et al. [1999]* being forced into the counterintuitive position that the effect of CO_2 fertilization was somehow stronger in the 19th century but became attenuated in the 20th century, the exact opposite of the hypothesis of *LaMarche et al. [1984]* and later *Graybill and Idso [1993]*. If the differences between the northern treeline series and the bristlecone pines arise from some other factor (a couple of possibilities are discussed below), then the *Mann et al. [1999]* "adjustment" would have made the proxy record even more distorted. In MBH98, no such adjustment was made in the AD1400 period in any event. (It also appears that no such adjustment was made in *Mann and Jones [2003]* or *Jones and Mann [2004]*, but we are presently unable to confirm this.)

An important influence on these trees is precipitation. *Mann and Jones [2003]* point out the need to distinguish between temperature and precipitation effects, which may have a different expression. Within bristlecone pine literature, *LaMarche and Stockton [1974]* pointed out that high-altitude bristlecone pine stands have both a lower limit and upper limit and argued that bristlecone pine growth at the lower limit was controlled by precipitation and at the upper limit by temperature. *Hughes and Funkhouser [2003]* found regional correlations among high-altitude bristlecone pine growth, which they attributed to regional climate, but still concluded that the anomalous 20th century growth was a "mystery". Even in upper limit stands, the bristlecone pine stands in the PC1 are located in semi-arid regions and the bifurcation in *LaMarche and Stockton [1974]* may be overly simplistic. Studies of actual bristlecone pine growth have shown that it is limited by soil moisture *[Fritts, 1969; Beasley and Klemmedson, 1973]*. Even in higher stands, their principal botanical competition in many locations is with big sagebrush *[Wright and Mooney, 1965; Mooney et al., 1964]* with bristlecones outcompeting big sagebrush on moister dolomite substrate. This effect is vividly illustrated by Figure 2 of *Wright and Mooney [1965]*, where the sharp geological contact between the dolomite and sandstone is clearly shown by the change from bristlecone pines to sagebrush at the same elevation. The same effect is also perhaps shown in the charming 19th century painting (Figure 7), where a sharp change in vegetation at the same elevation is easily observed. There is evidence that higher moisture levels in the 20th century in the American Southwest accounted for high growth rates in New Mexico *[Grissino-Mayer, 1996; D'Arrigo and Jacoby, 1991]*, where two of the *LaMarche and Stockton [1974]* sites are located. The effect may extend to other locations. In the classical bristlecone pine sites of the White Mountains, where a weather station operated close to Sheep Mountain and Campito Mountain from 1954 to 1980, records show low ring widths correlate to drought, even in upper limit stands. *Mann and Jones [2003]* pointed out that precipitation proxies need to be carefully distinguished from temperature proxies and a complete demonstration that these effects have been separated in bristlecone pines is obviously required. *Williams [1996]* reported that a continuous climate record since 1951 at Niwot Ridge in the Colorado Front Range, near a bristlecone pine site, showed a decline in mean annual temperature and an increase in annual precipitation amount.

There is one other issue that needs to be canvassed and eliminated prior to reliance on bristlecone pines. The pulse in bristlecone pine growth is contemporaneous with a

Figure 7. 19th century painting showing bristlecone pines. Source: NOAA website. In the background, the pines reaching up the hill are almost certainly bristlecone pines on a dolomite substrate, with a sandstone substrate where there are no pines. Retrieved from <http://www.ngdc.noaa.gov/paleo/drought/drght_graumlich.html>

pulse in woody plant growth throughout the American Southwest, attributed to overgrazing by sheep in the late 19th Century (see Figure 8), which in turn followed the extension of the railroads *[Allen, 1998; Allen et al., 1998]*. Sheep differ from other species in that they will completely destroy grasslands by eating down to the roots, leaving barrens *[Allen, 1998]*. Although *Allen [1998]* only documented the expansion of pinyon pines and junipers into terrain formerly occupied by 19th century grasslands, Allen (2004, pers. comm.) did not exclude the possibility of a similar effect involved in anomalous 20th century growth for bristlecone pines, but was unaware of any studies on the topic. There is a published reference to the introduction of large commercial sheep flocks in the late 19th century in the White Mountains CA *[St. Andre et al. 1967]*, where the key sites of Sheep Mountain and Campito Mountain are located. The founder of the Sierra Club, John Muir, complained of the depredations of sheep in the Sierra Nevadas (adjacent to the White Mountains) as "hoofed locusts" *[Muir, 1911]*. Carl Purpus, a late 19th century botanical collector in the Sierra Nevadas, stated in 1896 that commercial flocks had cleaned out all grass to the top of Old Mt Whitney [present-day Mount Langley, which reaches 4,280 m] *[Ertter, 1988]*. Allen (pers. comm., 2004) said that there was a large commercial sheep trail at Jicarita Peak NM, another bristlecone pine site studied by *LaMarche and Stockton [1974]*. In severe high-altitude terrain, even after the departure of commercial flocks, a small population of bighorn sheep could prevent the re-establishment of grass (Leslie Thomas, Colorado Springs, landscape architect, pers. comm.) Since grass (and other herbs) compete with pines for scarce moisture, one can hardly exclude, on a priori basis, the possibility of a connection between anomalous 20th century growth rates of bristlecone pines and a growth release following 19th century overgrazing, as experienced elsewhere in the American Southwest.

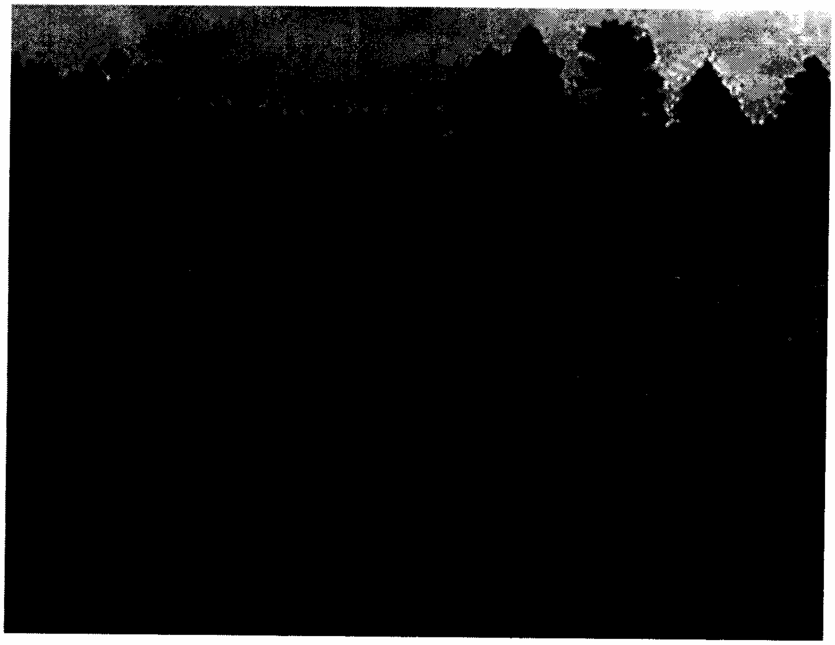

Figure 8. Sheep grazing in ponderosa pine forests and grasslands near Flagstaff, AZ, ca. 1899. Image 21a by F.H. Maude, Cline Library Special Collections, Northern Arizona University. Retrieved from http://www.cpluhna.nau.edu/Change/grazing.htm

Finally, there may even be problems with the site chronologies as indexes of actual growth. *Cook and Peters [1997]* pointed out that conventional dendrochronology techniques resulted in a bias in 20th century results at Campito Mountain, one of the Graybill sites in the NOAMER PC1. Presumably the same effect applies to other bristlecone pine sites.

If the reader takes the (reasonable, we think) view that these unusual trees are not mystical antennae for an elusive "climate signal" missed by all other proxy indicators, then each of the above problems and issues must be dealt with systematically, prior to any reliance being placed on bristlecone pine ring widths as the dominant arbiter of world climate history.

4.2 Gaspé Cedar Series

The other critical series in MBH98 is the Gaspé cedar chronology. There are many interesting similarities between bristlecone pines and cedars. *Kelly, Cook and Larson [1992]* likened Eastern white cedars to bristlecone pines, pointing out the occurrence of strip bark forms and the apparent lack of senescence. Larson (pers. comm., 2004) stated that there is little aging effect in Ontario cliff cedars: they grow slowly when they are young and slowly when they are old.

There is virtually no site information on the Gaspé cedar series. *Sheppard and Cook [1988]* mentions the site, but is not a comprehensive publication. They commented on its "peculiar" growth spurt in the 20th century, but cautioned that there were no other northern white cedar chronologies available so it was not possible to say whether it was indicative of climatic conditions or some other influence. *Sheppard and Cook [1988]* referred to two other pending cedar studies, one in Maine and one in Michigan. The Maine series (Sag Pond) has been archived at WDCP and does not show any 20th century trend, while the Michigan series seems to have remained unpublished. *Archambault and Bergeron [1992]* published a cedar series from Lac Duparquet, Quebec, but it does not show any 20th century trend and the authors reported a positive correlation to precipitation and a negative relationship to June temperature. For Ontario cedars, *Kelly et al. [1994]* reported a strong negative relationship between temperature and observed growth rates in the 20th century.

Cedar growth is optimal under cool and moist conditions *[Kelly et al., 1994; Matthes-Sears and Larson, 1990]* and declines both in very hot and very cold weather (an upside-down U). A similar upside-down U pattern has been reported for bristlecone pines and two other conifer species *[Schoettle, 2004]*. In fact, the possibility of a quadratic ring width response to temperature has been recently posited by *D'Arrigo et al. [2004*] in connection with Twisted Tree Heartrot Hill. This possibility has far-reaching implications on the entire enterprise of estimating past temperatures from tree ring widths: with a quadratic (upside-down U) response, it is impossible to determine whether a past narrow ring width resulted from cold or hot weather.

We carried out our own comparison between gridcell temperature in the Gaspé area and Gaspé temperatures and did not find any correlation.

Cook and Peters [1997] discussed above, explored spurious end-of-sample growth bias as an artifact of tree-ring chronology de-trending. Amazingly, in addition to the Campito Mountain bristlecone pine site, their other main example was the Gaspé series (cana036). In order to eliminate this bias, the underlying tree ring chronologies would have to be re-calculated, a calculation which would have the effect of reducing its hockey-stick shape, with implications that stand alone from any of the other issues raised in this paper.

The Gaspé site was re-sampled in the early 1990s; we have seen a site chronology showing that the re-sampling did not replicate the previously reported 20th century growth spurt. However, the new data has not been published or archived, and the originating authors have refused to disclose the new data on the grounds that the older data "better" shows temperature and because their research is "mission-oriented". We have sought coordinates of the actual site in order to commission a re-sampling of the site, but we have not received this information despite repeated requests.

5. REFUTATION OF COUNTER-ARGUMENTS OF MANN ET AL.

We now turn to a discussion of recent arguments of *Mann et al. [2003, 2004a, 2004b, 2004d]*, purporting to counter our various criticisms.

5.1 "Effective Omission" of Indicators

Mann et al. [2004a, 2004b] argued that our use of centered principal components calculations amounted to an "effective omission" of the 70 sites of the North American network. They showed that a calculation excluding the North American PC1 also resulted in MM-type results with high early 15th century values. Although the calculations to which they were referring (using centered PC methods) did *not* actually omit this network, since the results were similar to results without the PC1, Mann et al. argued that our calculations *effectively omitted* these indicators. This critique fails on several counts. First, the North American PC1 is only one of 22 series in the AD1400 step. A robust statistical method should be relatively insensitive to the presence or absence of one of 22 series. If centered principal components calculations are used, the temperature index is relatively insensitive to the presence or absence of the North American PC1. On the other hand, if de-centered principal components calculations are used, the results are very unstable to the presence or absence of the North American PC1. Robustness considerations therefore tend to support the use of a conventional centered PC method. Second, using the MBH98 decentered method, 14 bristlecone sites account for over 99% of the explained variance in the PC1. Using the terminology of Mann et al., under the decentered methodology, the other 56 sites are "effectively omitted" from the PC1, which is merely a carrier for the bristlecone pines. Using a centered methodology, the PC1 is relatively similar to the mean of all the series. Thus, we believe that it is more accurate to characterize MBH98 de-centering methods as "effectively omitting" the majority of tree ring sites.

Mann et al. [2004a, 2004b] also argued that use of the archived version of the Gaspé series amounted to an "effective omission" of the northern treeline series. *Jones and Mann [2004]* do not use the Gaspé series at all and only use the Jacoby northern treeline series in the relatively well-replicated portion after 1601. Simply applying the quality control criteria of *Jones and Mann [2004]* should not provoke complaints about "effective omission". Moreover, we have specifically maintained the number of northern treeline series in the AD1400 step, by using the updated version of the Sheenjek River series (which could have been used in 1997). Replication in the Sheenjek River series is much superior to that of the Gaspé series, which does not meet standard quality control criteria in its early portion.

5.2 Tendency of decentered PC methods to yield PC1 hockeysticks

In *McIntyre and McKitrick [2004a, 2004b]*, we pointed out that the de-centered PC method used in MBH98 tends to produce hockey-stick shaped series. We have sharpened this result considerably in *McIntyre and McKitrick [2005]*. There we define a "hockey stick" as a series in which the 1902–1980 mean differs from the long-term mean by more than 1 standard deviation (σ). Applying the MBH98 decentered PC method to trendless red noise with persistence properties of the North American tree ring network (modeled as fractional processes), in 10,000 simulations we found that the 1902–1980 mean differed from the 1400–1980 mean by more than 1 σ over 99% of the time, (1.5 σ –72%; 1.75 σ –19% and 2 σ –0.2%). The hockey stick blades sloped up about half the time and down half the time, but the 1902-1980 mean is almost never within one σ of the 1400–1980 mean. PC series are unoriented so that no significance is attributed to the sign.

In their comment to the earlier version of this argument, Mann et al. [2004a, 2004b] argued that their PC series were simply linear combinations of the underlying proxies and that no pattern could be produced in the PC1 which was not in the underlying data. It is of course true that the PC series are linear combinations of the proxies, but it is evident that the de-centering process preferentially selects series with hockey-stick shapes and this over-weighting is what yields a pattern that is not representative of the underlying data. The exclusive selection of bristlecones into the PC1 should give rise to serious examination of why all other proxies are so efficiently discarded – a discussion which does not occur in MBH98.

5.3 Lack of a linear response to temperature in "key" proxies

In *McIntyre and McKitrick [2004b],* in our criticism of bristlecone pines as an arbiter of world climate, we pointed out (as above) that a linear response to temperature had not been established for these sites (as seemingly required by MBH98). *Mann et al. [2004b]* replied that:

> MM04 demonstrate their failure to understand our methods by claiming that we required that "proxies follow a linear temperature response". In fact we specified (MBH98) that indicators should be "linearly related to one or more of the instrumental training patterns[2]", not local temperatures.

We doubt the authors really believe the idea of a temperature proxy exhibiting no relationship to local temperature makes much sense. It is instructive to compare this response to the policy articulated in *Jones and Mann [2004],* which states:

> A number of other temperature reconstructions used in earlier multiproxy composites or in review papers [e.g., Jones et al., 1998; Mann et al., 1998a, 1999; Mann and Jones, 2003] are not included. This is because they are either less resolved than decadal resolution [e.g., Dahl-Jensen et al., 1998] or correlations with local grid box temperatures are not significant …

Jones and Mann [2004] do consider "climate field reconstructions" (CFRs), which appear to be similar to "instrumental training patterns" of MBH98. In this case, *Jones and Mann [2004]* argue that the CFRs should be shown to be similar to some aspect of local climate during some part of the year. This would seem to invite opportunistic use of either precipitation or temperature as a climate indicator, something for which they reproached *Soon et al. [2003].* But perhaps most telling is the comment of MBH98 co-author Hughes in *Hughes and Funkhouser [2003],* who did not attribute the bristlecone pine growth to an "instrumental training pattern", but stated that their anomalous 20th century growth rate is a "mystery".

5.4 Insignificant Values of Verification Statistics

Mann et al. [2004a, 2004b] have argued that, regardless of how they got their results, their reconstruction with decentered PC methods and extrapolation of the duplicate Gaspé series has greater "skill" than a reconstruction with centered PC methods and use of the archived version of the Gaspé series. Most dendroclimatic reconstructions provide a suite of verification statistics, including RE, R^2, CE, sign test and product mean test *[e.g. Cook et al, 1994]*. In MBH98, only the RE statistic is reported for steps prior to the AD1820 step, including the controversial AD1400 step. Mann et al. have not provided their own results for the other verification statistics or supporting calculations from which these statistics could be calculated, and have refused requests for this information. *McIntyre and McKitrick, 2005,* using Monte Carlo simulations, shows that the MBH98 benchmark for 99% significance for the RE statistic is substantially under-stated (0.0 in MBH98 versus a Monte Carlo estimate of 0.59) and that the R^2 and other verification statistics, which were not reported in MBH98, are statistically insignificant in the AD1400 step.

Mann et al. [2004b] contained a diatribe against the R^2 statistic. However, in other papers [e.g. *Mann and Jones, 2003]*, when they were in his favour, Mann has reported R^2 statistics. In this case, we estimate the R^2 statistic as being only 0.02 – obviously well short of statistical significance and strongly indicating that even the lower level of RE significance discussed above is spurious.

5.5. "Confirmation" by other studies

Mann et al. [2003, 2004a, 2004b] argued that their results are similar to those of "independent" studies, such as *Jones, Briffa et al. [1998], Crowley and Lowery [2000], Briffa, Jones et al [2001], Mann and Jones [2003]* and *Jones and Mann [2004],* calculated with different proxies and different methods. This "similarity" is typically shown by "spaghetti" diagrams supposedly illustrating the similarity, rather than through detailed analysis.

These studies are hardly "independent". If all the authors in the multiproxy articles are listed, one sees much overlapping. Mann himself was a co-author of two supposedly "independent" studies; his sometime co-author (as well as Bradley's sometime co-author) Jones was co-author of two of the others. Even *Crowley and Lowery [2000],* where there is no apparent overlap, stated that they used data supplied by Jones. This hardly amounts to "independence" in any conventional use of the term.

Many proxies are re-used in these studies, a point which *Briffa and Osborn [1999]* acknowledged, as follows:

> An uninformed reader would be forgiven for interpreting the similarity between the 1000-year temperature curve of Mann et al. and a variety of others also representing either temperature change over the NH as a whole or a large part of it (see the figure) as strong corroboration of their general validity, and, to some extent, this may well be so.

> Unfortunately, very few of the series are truly independent: There is a degree of common input to virtually every one, because there are still only a small number of long, well-dated, high-resolution proxy records.

Briffa's Polar Urals and Tornetrask series [*Briffa et al., 1995; Briffa et al., 1992b* respectively] are recurrent proxies as is Cook's Tasmania reconstruction [*Cook et al., 1991, 1992*]. The North American PC1, criticized here, is used as a proxy in *Mann et al. [1999], Mann and Jones [2003]* and *Jones and Mann [2004].*

Most importantly, even if such articles generate similar results to MBH98, that does not prove that MBH98 results were calculated correctly. Mann et al. have to support MBH98 on its own terms; appeals to other results are completely irrelevant.

For rhetorical purposes, agencies like the IPCC may well turn to these other studies for support, if MBH98 can no longer be used, but the prominent reliance on MBH98/99 in the Third Assessment Report is a matter of public record and cannot now be undone. If there is any lesson from our work it is that, before making prominent use of these other studies, each one needs to be proven replicable. However critical we may be of MBH98, the disclosure for nearly all the other studies is significantly worse:

- After over 20 requests, Crowley (pers. comm., Oct. 2004) supplied smoothed and transformed versions of proxy data used in *Crowley and Lowery [2000],* but stated that he could not find the actual data versions used so that these could be verified.
- A listing of the sites used in *Briffa et al. [2001]* has never been published or archived. The authors have not responded to requests for data.
- A listing of sites in *Esper et al. [2002]* is available, but the majority of site data is not publicly archived.
- Most of the data from *Mann and Jones [2003]* and *Jones and Mann [2004]* was eventually provided by Jones in July 2004. However, Jones was unable to provide the weightings used in the creation of the final results, as these were in the possession of co-author Mann.
- Of these studies, only *Jones et al. [1998]* has a relatively complete record.

None of these studies provides a careful, objective analysis of how the particular proxy records are selected from the thousands available, thereby leaving unanswered the possibility of cherry-picking. Replication is only the first step in assessment. One then has to assess the quality of the proxies actually used. For example, we have concerns about potential problems in Briffa's Polar Urals record *[Briffa et al., 1995],* which has a very significant effect on medieval values in several of these studies. We intend to address these issues in the future.

6. DISCUSSION

There are many large issues at stake in this discussion, mainly because of the powerful role a handful of published paleoclimate studies are playing in policy decisions.

The ability of later researchers to carry out independent due diligence in paleoclimate is severely limited by the lack of journal policies or traditions requiring contributors to promptly archive data and methods. *King [1995]* has excellent comments on replication. In this respect, paleoclimate journal editors should consider changes taking place at some prominent economics journals. For example the *American Economic Review* now requires, as a precondition of publication, archiving data and computational code at the journal. This is a response to the critique of *McCullough and Vinod [2003],* and earlier work by *Dewald et al. [1986].* The files associated with paleoclimate studies are trivial to archive. In our view, if the public archive does not permit the replication of a multiproxy study, then it should be proscribed for use in policy formation *[McCullough and Vinod, 2003].*

In addition, we are struck by the lack of policy both in paleoclimate publications and in climate policy reports (e.g. IPCC, ACIA) regarding the reporting of results adverse to their claims. While it may be assumed that results adverse to their claims would be generally disclosed, we are unaware of any paleoclimate journal which explicitly articulates this as a requirement to authors. In contrast, for a prospectus offering securities to the public, officers and directors are required to affirm that the prospectus contains "full, true and plain disclosure", which requires the disclosure of material adverse results. In MBH98, there are a number of examples, where results adverse to their claims were not reported (and in some cases, actual misrepresentations), as listed below (most of which we have discussed passim above):

- MBH98 did not report the results adverse to their conclusions from calculations excluding bristlecone pines (contained in the BACKTO_1400-CENSORED directory).
- For steps prior to 1820, MBH98 did not report verification statistics other than the RE statistic. Unlike the above case, we cannot prove on the present record that Mann et al. had calculated these other statistics, but we consider it quite likely that these statistics were calculated and not reported. (In this case, we believe that diligent referees, even under the limited scope and mandate of journal peer review, should have requested the reporting of this information.)
- MBH98 did not report results from calculations using archived Gaspé tree ring data (which did not contain the extrapolation of early values). Again, while we cannot prove that they actually carried out calculations using the archived version, we find it inconceivable that this unique extrapolation would have been made without previously doing a calculation using the archived version. Although the Corrigendum (six years after the event) disclosed the existence of this extrapolation, it did not disclose its uniqueness or the actual effect of this previously undisclosed extrapolation, disclosure which we believe to be essential for full disclosure, since the very existence of the extrapolation had been hidden from referees and previous readers by a misrepresentation of the start date of this series.
- MBH98 incorrectly stated that conventional PC methods were used, which necessarily means centered calculations. This error in their prior disclosure should have been prominently disclosed in the Corrigendum together with its effects on PC calculations described, especially since it was at the heart of our submission then

under review at *Nature*. Mann et al. could then try to argue in that context that the effect was limited (an argument with which we obviously disagree). Instead, the prior incorrect disclosure was not mentioned at all in the printed Corrigendum; in the Corrigendum SI, the incorrect prior disclosure is not specifically mentioned; the method itself is acknowledged, but it is not prominent and even carries a denial that the method made any difference (a claim discussed at length above).

- The aggressive claims that MBH98 methods were "robust" (see discussion above) are extremely problematic. As noted above, Mann et al. had carried out a sensitivity study on the exclusion of the bristlecone pines and knew that their 15th century results were not robust to these sites. We also believe that they knew the instability regarding the Gaspé series (or else they wouldn't have done the extrapolation.) We find it difficult to understand how the claims to robustness could have made under these circumstances.

We are also struck by the extremely limited extent of due diligence involved in peer review as carried out by paleoclimate journals, as compared with the level of due diligence involved in auditing financial statements or carrying out a feasibility study in mineral development. For example, "peer review" in even the most eminent paleoclimate publications, as presently practiced, does not typically involve any examination of data, replication of calculations or ensuring that data and computational procedures are archived. We are not suggesting peer reviewers should be auditors. Referees are not compensated for their efforts and journals would not be able to get unpaid peer reviewers to carry out thorough audits. We ourselves do not have explicit recommendations on resolving this problem, although ensuring the archiving of code and data as used is an obvious and inexpensive way of mitigating the problem.

But it seems self-evident to us that, recognizing the limited due diligence of paleoclimate journal peer review, it would have been prudent for someone to have actually checked MBH98 data and methods against original data before adopting MBH98 results in the main IPCC promotional graphics..

Supplementary Information accompanies the paper at www.climate2003.com.
Financial support for this research was neither sought nor received. The authors declare they have no competing financial interests.

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GEOPHYSICAL RESEARCH LETTERS, VOL. 32, L03710, doi:10.1029/2004GL021750, 2005

TAB 7

Hockey sticks, principal components, and spurious significance

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Received 14 October 2004; revised 22 December 2004; accepted 17 January 2005; published 12 February 2005.

[1] The "hockey stick" shaped temperature reconstruction of Mann et al. (1998, 1999) has been widely applied. However it has not been previously noted in print that, prior to their principal components (PCs) analysis on tree ring networks, they carried out an unusual data transformation which strongly affects the resulting PCs. Their method, when tested on persistent red noise, nearly always produces a hockey stick shaped first principal component (PC1) and overstates the first eigenvalue. In the controversial 15th century period, the MBH98 method effectively selects only one species (bristlecone pine) into the critical North American PC1, making it implausible to describe it as the "dominant pattern of variance". Through Monte Carlo analysis, we show that MBH98 benchmarks for significance of the Reduction of Error (RE) statistic are substantially under-stated and, using a range of cross-validation statistics, we show that the MBH98 15th century reconstruction lacks statistical significance. **Citation:** McIntyre, S., and R. McKitrick (2005), Hockey sticks, principal components, and spurious significance, *Geophys. Res. Lett.*, *32*, L03710, doi:10.1029/2004GL021750.

1. Introduction

[2] The term "hockey stick" is often used to describe the shape of the Northern Hemisphere (NH) mean temperature index introduced in *Mann et al.* [1998] (hereinafter referred to as MBH98). For convenience, we define the "hockey stick index" of a series as the difference between the mean of the closing sub-segment (here 1902–1980) and the mean of the entire series (typically 1400–1980 in this discussion) in units of the long-term standard deviation (σ), and a "hockey stick shaped" series is defined as one having a hockey stick index of at least 1 σ. Such series may be either upside-up (i.e., the "blade" trends upwards) or upside-down. Our focus here is on the 1400–1450 step ("AD1400 step") of MBH98, because of controversy over early 15th century temperature reconstructions [*McIntyre and McKitrick*, 2003; M. E. Mann et al., Note on paper by McIntyre and McKitrick in *Energy and Environment*, unpublished manuscript, 2003, available at ftp://holocene.evsc.virginia.edu/pub/mann/EandEPaperProblem.pdf, hereinafter referred to as Mann et al., unpublished manuscript, 2003]. Our particular interest in the performance of the Reduction of Error (RE) statistic arises out of that controversy. We also focus on the North American tree ring network ("NOAMER"), because the first principal component ("PC1") of this network has been identified as essential for controversial periods of the MBH98 temperature reconstruction [*Mann et al.*, 1999, unpublished manuscript, 2003]. MBH98 has recently been criticized on other grounds in *von Storch et al.* [2004].

[3] MBH98 used principal components (PCs) to reduce the dimensionality of tree ring networks and stated that they used "conventional" PC analysis. A conventional PC algorithm centers the data by subtracting the column means of the underlying series. For the AD1400 step highlighted here, this would be the full 1400–1980 interval. Instead, MBH98 Fortran code (ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/pca-noamer) contains an unusual data transformation prior to PC calculation that has never been reported in print. Each tree ring series was transformed by subtracting the 1902–1980 mean, then dividing by the 1902–1980 standard deviation and dividing again by the standard deviation of the residuals from fitting a linear trend in the 1902–1980 period. The PCs were then computed using singular value decomposition on the transformed data. (The effects reported here would have been partly mitigated if PCs had been calculated using the covariance or correlation matrix.) This previously unreported transformation was recently acknowledged in the Supplementary Information to a Corrigendum to MBH98 [*Mann et al.*, 2004], where they asserted that it has no effect on the results, a claim we refute herein.

[4] PCs can be strongly affected by linear transformations of the raw data. Under the MBH98 method, for those series in which the 1902–1980 mean is close to the 1400–1980 mean, subtraction of the 1902–1980 mean has little impact on weightings for the PC1. But if the 1902–1980 mean is different than the 1400–1980 mean (i.e., a hockey stick shape), the transformation translates the "shaft" off a zero mean; the magnitude of the residuals along the shaft is increased, and the series variance, which grows with the square of each residual, gets inflated. Since PC algorithms choose weights that maximize variance, the method re-allocates variance so that hockey stick shaped series get overweighted. In effect, the MBH98 data transformation results in the PC algorithm mining the data for hockey stick patterns.

[5] In a network of persistent red noise, there will be some series that randomly "trend" up or down during the ending sub-segment of the series (as well as other sub-segments). In the next section, we discuss a Monte Carlo experiment to show that these spurious "trends" in a closing segment are sufficient for the MBH98 method, when applied to a network of red noise, to yield hockey stick PC1s, even though the underlying data gener-

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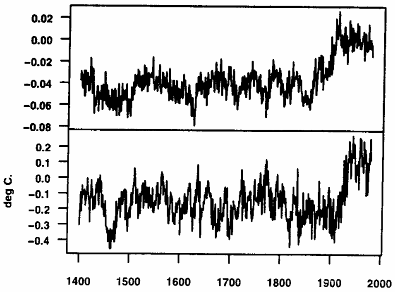

Figure 1. Simulated and MBH98 Hockey Stick Shaped Series. Top: Sample PC1 from Monte Carlo simulation using the procedure described in text applying MBH98 data transformation to persistent trendless red noise; Bottom: MBH98 Northern Hemisphere temperature index re-construction.

ating process has no trend component. We then examine the effect of this procedure on actual MBH98 weights for the North American PC1. Finally we use the simulated PC1s to establish benchmarks for the Reduction of Error (RE) verification statistic used by MBH98, and we discuss R^2 and other verification statistics for the MBH98 reconstruction.

2. Monte Carlo Simulations of Hockey Sticks on Trendless Persistent Series

[6] We generated the red noise network for Monte Carlo simulations as follows. We downloaded and collated the NOAMER tree ring site chronologies used by MBH98 from M. Mann's FTP site and selected the 70 sites used in the AD1400 step. We calculated autocorrelation functions for all 70 series for the 1400–1980 period. For each simulation, we applied the algorithm *hosking.sim* from the *waveslim* package version 1.3 downloaded from www.cran.r-project.org/doc/packages/waveslim.pdf [*Gencay et al.*, 2001], which applied a method due to *Hosking* [1984] to simulate trendless red noise based on the complete auto-correlation function. All simulations and other calculations were done in R version 1.9 downloaded from www.R-project.org [*R Development Core Team*, 2003]. Computer scripts used to generate simulations, figures and statistics, together with a sample of 100 simulated "hockey sticks" and other supplementary information, are provided in the auxiliary material[1]. We carried out 10,000 simulations, in each case obtaining 70 stationary series of length 581 (corresponding to the 1400–1980 period). By the very nature of the simulation, there were no 20th century trends, other than spurious "trends" from persistence. We applied the MBH98 data transformation to each series in the network: the 1902–1980 mean was subtracted, then the series was divided by the 1902–1980 standard deviation, then by the 1902–1980 detrended standard deviation. We carried out a singular value decomposition on the 70 transformed series (following MBH98) and saved the PC1 from each calculation.

[7] The simulations nearly always yielded PC1s with a hockey stick shape, some of which bore a quite remarkable similarity to the actual MBH98 temperature reconstruction – as shown by the example in Figure 1. A sharp inflection was regularly observed at the start of the 1902–1980 "calibration period". Figure 2 shows histograms of the hockey stick index of the simulated PC1s. Without the MBH98 transformation (top panel), a $1\ \sigma$ hockey stick occurs in the PC1 only 15.3% of the time ($1.5\ \sigma$ – 0.1%). Using the MBH98 transformation (bottom panel), a $1\ \sigma$ hockey stick occurs over 99% of the time, ($1.5\ \sigma$ – 73%; $1.75\ \sigma$ – 21% and 2σ – 0.2%).

[8] The hockey sticks were upside-up about half the time and upside-down half the time, but the 1902–1980 mean is almost never within one σ of the 1400–1980 mean under the MBH98 method. PC series have no inherent orientation and, since the MBH98 methodology uses proxies (including the NOAMER PC1) in a regression calculation, the fit of the regression is indifferent to whether the hockey stick is upside-up or upside-down. In the latter case, the slope coefficient is negative. In fact, the North American PC1 of *Mann et al.* [1999] is an upside-down hockey stick, as shown at ftp://ftp.ngdc.noaa.gov/paleo/contributions_by_author/mann1999/proxies/itrdb-namer-pc1.dat.

[9] The loadings on the first eigenvalues were inflated by the MBH98 method. Without the transformation, the median fraction of explained variance of the PC1 was only 4.1% (99th percentile–5.5%). Under the MBH98 transformation, the median fraction of explained variance from PC1 was 13% (99th percentile–23%), often making the PC1 appear

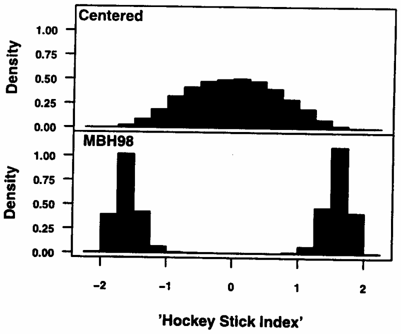

Figure 2. Histogram of 'Hockey Stick Index' for PC1s. For the 10,000 simulated PC1s described in text, the histogram shows the distribution of the difference between the 1902–1980 mean and the 1400–1980 mean, divided by the 1400–1980 standard deviation. Top: Conventional (centered) calculation; Bottom: with MBH98 data transformation.

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[1] Auxiliary material is available at ftp://ftp.agu.org/apend/gl/2004GL021750.

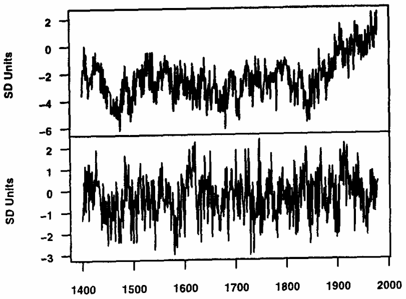

Figure 3. PC1 for AD1400 North American Tree Ring Network. Top: Result with MBH98 data transformation; Bottom: recalculated on the same data without MBH98 data transformation. Both standardized to 1902–1980 period.

to be a "dominant" signal, even though the network is only noise.

3. The PC1 in the MBH98 North American Network

[10] We now show the effect of the MBH98 algorithm on the actual NOAMER network in the controversial AD1400 step.

[11] Without the data transformation the PC1 is very similar to the unweighted mean of all the series and, as shown in the bottom panel of Figure 3, does not have a hockey stick shape. However, under the MBH98 algorithm, the PC1 has a marked hockey stick shape, as shown in the top panel of Figure 3. The MBH98 method creates a PC1 which is dominated by bristlecone pines and closely related foxtail pines. (Foxtail pines are located in an adjacent mountain range, interbreed with bristlecone pines and are included here with bristlecone pines collectively). Out of 70 sites in the network, 93% of the variance in the MBH98 PC1 is accounted for by only 15 bristlecone and foxtail pine sites collected by Donald Graybill [*Graybill and Idso*, 1993] (see Table 1). The weights in the MBH98 PC1 have a nearly linear relationship to the hockey stick index. The most heavily weighted site in the MBH98 PC1, Sheep Mountain, is a bristlecone pine site with the most pronounced hockey stick shape (1.6 σ) in the network; it receives over 390 times the weight of the least weighted site, Mayberry Slough, whose hockey stick index is near 0.

[12] Under the MBH98 data transformation, the distinctive contribution of the bristlecone pines is in the PC1, which has a spuriously high explained variance coefficient of 38% (without the transformation – 18%). Without the data transformation, the distinctive contribution of the bristlecones only appears in the PC4, which accounts for less than 8% of the total explained variance.

[13] This substantially reduced share of explained variance, together with the fact that species other than bristlecone/foxtail pines are effectively omitted from the MBH98 PC1, argues strongly against interpreting it as the "dominant component of variance" in the North American network (M. E. Mann et al., Reply to "Global-scale temperature patterns and climate forcings over the past six centuries: A comment" by S. McIntyre and R. McKitrick, unpublished manuscript, 2004, available at http://stephenschneider.stanford.edu/Publications/PDF_Papers/MannEtAl2004.pdf). *McIntyre and McKitrick* [2005] discuss, inter alia, problems relating to the interpretation of bristlecone/foxtail pine growth as a temperature proxy, and we show the impact of using conventional (centered) PC methods on the MBH98 northern hemisphere temperature index, which has a significant effect on the relative values in the 15th and 20th centuries.

4. Benchmarking the Reduction of Error Statistic for the MBH98 Algorithm

[14] In most dendroclimatic studies several verification statistics are used. For example, *Cook et al.* [1994] describe the Reduction of Error (RE), R^2, Coefficient of Efficiency (CE), sign test and product mean tests as measures of skill. MBH98 only reported RE statistics to demonstrate statistical skill, reporting an RE value for their AD1400 step of 0.51. There is no theoretical distribution of the RE statistic

Table 1. 15 Highly Weighted Sites in MGH98 PC1[a]

ID Code	Name	Species	Elevation (m)	Author	*Graybill and Idso* [1993] #
az510	San Francisco Pks	PIAR	3535	D.A. Graybill	10
ca528	Flower Lake	PIBA	3291	D.A. Graybill	13
ca529	Timber Gap Upper	PIBA	3261	D.A. Graybill	14
ca530	Cirque Peak	PIBA	3505	D.A. Graybill	12
ca533	Campito Mountain	PILO	3400	D.A. Graybill and V.C. Lamarche	5
ca534	Sheep Mountain	PILO	3475	D.A. Graybill	11
co522	Mount Goliath	PIAR	3535	D.A. Graybill	2
co523	Windy Ridge	PIAR	3570	D.A. Graybill	4
co524	Almagre Mountain	PIAR	3536	D.A. Graybill	1
co525	Hermit Lake	PIAR	3660	D.A. Graybill	3
nv510	Charleston Peak	PILO	3425	D.A. Graybill	6
nv512	Pearl Peak	PILO	3170	D.A. Graybill	9
nv513	Mount Washington	PILO	3415	D.A. Graybill	8
nv514	Spruce Mountain	PILO	3110	D.A. Graybill	
nv516	Hill 10842	PILO	3050	D.A. Graybill	

[a] 15 high-altitude bristlecone (PILO, PIAR) and foxtail (PIBA) sites dominating MBH98 PC1, constituting 13 of 14 sites listed in Table 1 of *Graybill and Idso* [1993].

and hence no exact or asymptotic tables of significance levels [*Cook et al.*, 1994]. MBH98 attempted to benchmark the significance level for the RE statistic using Monte Carlo simulations based on AR1 red noise with a lag coefficient of 0.2, yielding a 99% significance level of 0.0. However their simulation under-estimates the actual persistence of tree ring proxies and ignores the effect of the MBH98 data transformation in over-weighting hockey stick shaped series.

[15] In order to obtain more accurate significance benchmarks, we regressed each of the 10,000 simulated PC1s against the MBH98 northern hemisphere temperature series (the "sparse" subset used by MBH98 for verification ftp://ftp.ngdc.noaa.gov/paleo/paleocean/by_contributor/mann1998/nhem-sparse.dat) in the 1901–1980 calibration period – a procedure which more closely emulates actual MBH98 methods. Since the simulated PC1s are red noise series containing no information about the climate, they can be used to establish lower limits for the significance levels which the actual proxy data must exceed to indicate reconstructive skill. Since MBH98 used 22 indicators in their AD1400 step calculation, whereas the Monte Carlo simulation used only the simulated NOAMER PC1, the actual RE significance level would be higher than the benchmark calculated here, which is only a lower limit, making the arguments herein conservative.

[16] For each regression, we calculated the temperature "reconstruction" from the simulated PC1 in the verification period (1854–1901), and used the "reconstruction" to calculate the RE, R^2, CE, Sign Test and Product Mean Test. From this data, we determined the 99% significance levels in the verification period as shown in Table 2. The pattern of verification statistics was quite consistent: a high RE statistic, a very low CE statistic and a low R^2 statistic, relative to white or weakly red noise values.

[17] According to our calculations, the lower-limit critical value for 99% RE significance is 0.59 (5% – 0.54), values much higher than the 99% critical value of 0.0 reported by MBH98. The reported RE value for the AD1400 step of the MBH98 reconstruction was 0.51 (90th percentile under our RE distribution). Mann et al. have not archived supporting calculations for the AD1400 step. Accordingly, we emulated the AD1400 step of MBH98 using their data, obtaining the verification period statistics shown in Table 3. We were only able to obtain an RE statistic of 0.46 (80th percentile under our RE distribution) and an R^2 statistic of 0.02 (statistically insignificant). Other verification statistics also lack statistical significance and the high RE-low R^2 pattern is obviously similar to the patterns from comparably treated red noise.

5. Discussion and Conclusions

[18] PC analyses are sensitive to linear transformations of data, even if such transformations only appear to be "standardizations". Here we have shown, in the case of MBH98, that a "standardization" step (that the authors did not even consider sufficiently important to disclose at the time of their study) significantly affected the resulting PC series. Indeed, the effect of the transformation is so strong that a hockey-stick shaped PC1 is nearly always generated from (trendless) red noise with the persistence properties of the North American tree ring network. This result is disquieting, given that the NOAMER PC1 has been reported to be essential to the shape of the MBH98 Northern Hemisphere temperature reconstruction.

Table 2. Statistical Significance Levels[a]

Verification Statistic	99% Significance Level (Simulation)	99% Significance Cutoff Used by MBH98
RE (β)	0.59	0.00
R^2	0.15	0.20
CE	0.03	NA
Sign Test	32	NA
Product Mean Test	2.73	NA

[a] 99% benchmarks from simulations described in text in and as reported by MBH98.

Table 3. Verification Period Statistics for AD1400 Step of MBH98 Reconstruction[a]

	AD1400 Step Results	
	Emulation	MBH98 Reported
RE (β)	0.46	0.51
R^2	0.02	NA
CE	−0.26	NA
Sign Test	22	NA
Product Mean Test	1.54	NA

[a] From emulation and as reported by MBH98.

[19] For evaluation of statistical skill in paleoclimatic studies, the Reduction of Error (RE) statistic is widely used, but lacks a theoretical distribution. Practitioners use Monte Carlo models to establish significance benchmarks. Here we have shown that the benchmarks can be dramatically affected by the Monte Carlo model itself and that the 99% significance level from a Monte Carlo model more accurately representing actual MBH98 procedures is 0.59, as compared to the level of 0.0 reported in the original study. More generally, this example shows that changes in methodology will generally require new Monte Carlo modeling, that benchmarks carried forward from one methodology cannot necessarily be applied to a new methodology – even if the method changes may appear slight, and that great caution is required prior to concluding statistical significance based on RE statistics.

[20] An obvious guard against spurious RE significance is to examine other cross-validation statistics, such as the R^2 and CE statistics, as recommended, for example, by *Cook et al.* [1994]. While there are limitations to the R^2 statistic, the analysis of statistical "skill" of *Murphy* [1988] presupposes that the R^2 statistic exceeds the skill statistic and cases where the RE statistic exceeds the R^2 statistic are of particular concern [*Cook et al.*, 1994]. In the case of MBH98, unfortunately, neither the R^2 and other cross-validation statistics nor the underlying construction step have ever been reported for the controversial 15th century period. Our calculations have indicated that they are statistically insignificant. Timely reporting of these statistics (in the original article) might have led to an earlier consideration of the discrepancy between the apparently high RE value and the low values of other statistics, and thus enabled earlier identification of the underlying data transformation resulting in this problem.

[21] **Acknowledgment.** No funding was sought or received for this work.

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TAB 8

Summary for Policymakers

A Report of Working Group I of the Intergovernmental Panel on Climate Change

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Summary for Policymakers

The Third Assessment Report of Working Group I of the Intergovernmental Panel on Climate Change (IPCC) builds upon past assessments and incorporates new results from the past five years of research on climate change[1]. Many hundreds of scientists[2] from many countries participated in its preparation and review.

This Summary for Policymakers (SPM), which was approved by IPCC member governments in Shanghai in January 2001[3], describes the current state of understanding of the climate system and provides estimates of its projected future evolution and their uncertainties. Further details can be found in the underlying report, and the appended Source Information provides cross references to the report's chapters.

An increasing body of observations gives a collective picture of a warming world and other changes in the climate system.

Since the release of the Second Assessment Report (SAR[4]), additional data from new studies of current and palaeoclimates, improved analysis of data sets, more rigorous evaluation of their quality, and comparisons among data from different sources have led to greater understanding of climate change.

The global average surface temperature has increased over the 20th century by about 0.6°C.

- The global average surface temperature (the average of near surface air temperature over land, and sea surface temperature) has increased since 1861. Over the 20th century the increase has been 0.6 ± 0.2°C[5,6] (Figure 1a). This value is about 0.15°C larger than that estimated by the SAR for the period up to 1994, owing to the relatively high temperatures of the additional years (1995 to 2000) and improved methods of processing the data. These numbers take into account various adjustments, including urban heat island effects. The record shows a great deal of variability; for example, most of the warming occurred during the 20th century, during two periods, 1910 to 1945 and 1976 to 2000.
- Globally, it is very likely[7] that the 1990s was the warmest decade and 1998 the warmest year in the instrumental record, since 1861 (see Figure 1a).
- New analyses of proxy data for the Northern Hemisphere indicate that the increase in temperature in the 20th century is likely[7] to have been the largest of any century during the past 1,000 years. It is also likely[7] that, in the Northern Hemisphere, the 1990s was the warmest decade and 1998 the warmest year (Figure 1b). Because less data are available, less is known about annual averages prior to 1,000 years before present and for conditions prevailing in most of the Southern Hemisphere prior to 1861.
- On average, between 1950 and 1993, night-time daily minimum air temperatures over land increased by about 0.2°C per decade. This is about twice the rate of increase in daytime daily maximum air temperatures (0.1°C per decade). This has lengthened the freeze-free season in many mid- and high latitude regions. The increase in sea surface temperature over this period is about half that of the mean land surface air temperature.

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[1] *Climate change* in IPCC usage refers to any change in climate over time, whether due to natural variability or as a result of human activity. This usage differs from that in the Framework Convention on Climate Change, where *climate change* refers to a change of climate that is attributed directly or indirectly to human activity that alters the composition of the global atmosphere and that is in addition to natural climate variability observed over comparable time periods.

[2] In total 122 Co-ordinating Lead Authors and Lead Authors, 515 Contributing Authors, 21 Review Editors and 420 Expert Reviewers.

[3] Delegations of 99 IPCC member countries participated in the Eighth Session of Working Group I in Shanghai on 17 to 20 January 2001.

[4] The IPCC Second Assessment Report is referred to in this Summary for Policymakers as the SAR.

[5] Generally temperature trends are rounded to the nearest 0.05°C per unit time, the periods often being limited by data availability.

[6] In general, a 5% statistical significance level is used, and a 95% confidence level.

[7] In this Summary for Policymakers and in the Technical Summary, the following words have been used where appropriate to indicate judgmental estimates of confidence: *virtually certain* (greater than 99% chance that a result is true); *very likely* (90–99% chance); *likely* (66–90% chance); *medium likelihood* (33–66% chance); *unlikely* (10–33% chance); *very unlikely* (1–10% chance); *exceptionally unlikely* (less than 1% chance). The reader is referred to individual chapters for more details.

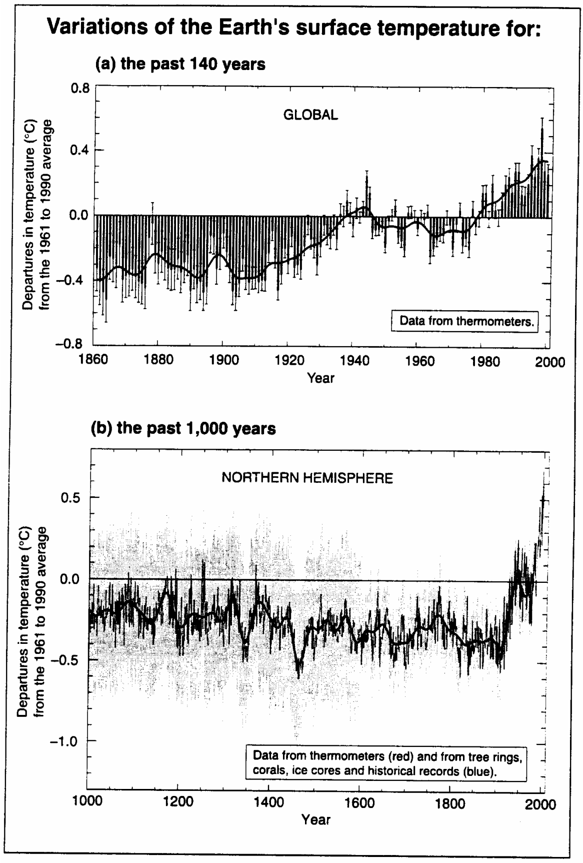

Figure 1: Variations of the Earth's surface temperature over the last 140 years and the last millennium.

(a) The Earth's surface temperature is shown year by year (red bars) and approximately decade by decade (black line, a filtered annual curve suppressing fluctuations below near decadal time-scales). There are uncertainties in the annual data (thin black whisker bars represent the 95% confidence range) due to data gaps, random instrumental errors and uncertainties, uncertainties in bias corrections in the ocean surface temperature data and also in adjustments for urbanisation over the land. Over both the last 140 years and 100 years, the best estimate is that the global average surface temperature has increased by 0.6 ± 0.2°C.

(b) Additionally, the year by year (blue curve) and 50 year average (black curve) variations of the average surface temperature of the Northern Hemisphere for the past 1000 years have been reconstructed from "proxy" data calibrated against thermometer data (see list of the main proxy data in the diagram). The 95% confidence range in the annual data is represented by the grey region. These uncertainties increase in more distant times and are always much larger than in the instrumental record due to the use of relatively sparse proxy data. Nevertheless the rate and duration of warming of the 20th century has been much greater than in any of the previous nine centuries. Similarly, it is likely[7] that the 1990s have been the warmest decade and 1998 the warmest year of the millennium.

[Based upon (a) Chapter 2, Figure 2.7c and (b) Chapter 2, Figure 2.20]

Temperatures have risen during the past four decades in the lowest 8 kilometres of the atmosphere.

- Since the late 1950s (the period of adequate observations from weather balloons), the overall global temperature increases in the lowest 8 kilometres of the atmosphere and in surface temperature have been similar at 0.1°C per decade.
- Since the start of the satellite record in 1979, both satellite and weather balloon measurements show that the global average temperature of the lowest 8 kilometres of the atmosphere has changed by +0.05 ± 0.10°C per decade, but the global average surface temperature has increased significantly by +0.15 ± 0.05°C per decade. The difference in the warming rates is statistically significant. This difference occurs primarily over the tropical and sub-tropical regions.
- The lowest 8 kilometres of the atmosphere and the surface are influenced differently by factors such as stratospheric ozone depletion, atmospheric aerosols, and the El Niño phenomenon. Hence, it is physically plausible to expect that over a short time period (e.g., 20 years) there may be differences in temperature trends. In addition, spatial sampling techniques can also explain some of the differences in trends, but these differences are not fully resolved.

Snow cover and ice extent have decreased.

- Satellite data show that there are very likely[7] to have been decreases of about 10% in the extent of snow cover since the late 1960s, and ground-based observations show that there is very likely[7] to have been a reduction of about two weeks in the annual duration of lake and river ice cover in the mid- and high latitudes of the Northern Hemisphere, over the 20th century.
- There has been a widespread retreat of mountain glaciers in non-polar regions during the 20th century.
- Northern Hemisphere spring and summer sea-ice extent has decreased by about 10 to 15% since the 1950s. It is likely[7] that there has been about a 40% decline in Arctic sea-ice thickness during late summer to early autumn in recent decades and a considerably slower decline in winter sea-ice thickness.

Global average sea level has risen and ocean heat content has increased.

- Tide gauge data show that global average sea level rose between 0.1 and 0.2 metres during the 20th century.
- Global ocean heat content has increased since the late 1950s, the period for which adequate observations of sub-surface ocean temperatures have been available.

Changes have also occurred in other important aspects of climate.

- It is very likely[7] that precipitation has increased by 0.5 to 1% per decade in the 20th century over most mid- and high latitudes of the Northern Hemisphere continents, and it is likely[7] that rainfall has increased by 0.2 to 0.3% per decade over the tropical (10°N to 10°S) land areas. Increases in the tropics are not evident over the past few decades. It is also likely[7] that rainfall has decreased over much of the Northern Hemisphere sub-tropical (10°N to 30°N) land areas during the 20th century by about 0.3% per decade. In contrast to the Northern Hemisphere, no comparable systematic changes have been detected in broad latitudinal averages over the Southern Hemisphere. There are insufficient data to establish trends in precipitation over the oceans.
- In the mid- and high latitudes of the Northern Hemisphere over the latter half of the 20th century, it is likely[7] that there has been a 2 to 4% increase in the frequency of heavy precipitation events. Increases in heavy precipitation events can arise from a number of causes, e.g., changes in atmospheric moisture, thunderstorm activity and large-scale storm activity.
- It is likely[7] that there has been a 2% increase in cloud cover over mid- to high latitude land areas during the 20th century. In most areas the trends relate well to the observed decrease in daily temperature range.
- Since 1950 it is very likely[7] that there has been a reduction in the frequency of extreme low temperatures, with a smaller increase in the frequency of extreme high temperatures.

- Warm episodes of the El Niño-Southern Oscillation (ENSO) phenomenon (which consistently affects regional variations of precipitation and temperature over much of the tropics, sub-tropics and some mid-latitude areas) have been more frequent, persistent and intense since the mid-1970s, compared with the previous 100 years.
- Over the 20th century (1900 to 1995), there were relatively small increases in global land areas experiencing severe drought or severe wetness. In many regions, these changes are dominated by inter-decadal and multi-decadal climate variability, such as the shift in ENSO towards more warm events.
- In some regions, such as parts of Asia and Africa, the frequency and intensity of droughts have been observed to increase in recent decades.

Some important aspects of climate appear not to have changed.

- A few areas of the globe have not warmed in recent decades, mainly over some parts of the Southern Hemisphere oceans and parts of Antarctica.
- No significant trends of Antarctic sea-ice extent are apparent since 1978, the period of reliable satellite measurements.
- Changes globally in tropical and extra-tropical storm intensity and frequency are dominated by inter-decadal to multi-decadal variations, with no significant trends evident over the 20th century. Conflicting analyses make it difficult to draw definitive conclusions about changes in storm activity, especially in the extra-tropics.
- No systematic changes in the frequency of tornadoes, thunder days, or hail events are evident in the limited areas analysed.

Emissions of greenhouse gases and aerosols due to human activities continue to alter the atmosphere in ways that are expected to affect the climate.

Changes in climate occur as a result of both internal variability within the climate system and external factors (both natural and anthropogenic). The influence of external factors on climate can be broadly compared using the concept of radiative forcing[8]. A positive radiative forcing, such as that produced by increasing concentrations of greenhouse gases, tends to warm the surface. A negative radiative forcing, which can arise from an increase in some types of aerosols (microscopic airborne particles) tends to cool the surface. Natural factors, such as changes in solar output or explosive volcanic activity, can also cause radiative forcing. Characterisation of these climate forcing agents and their changes over time (see Figure 2) is required to understand past climate changes in the context of natural variations and to project what climate changes could lie ahead. Figure 3 shows current estimates of the radiative forcing due to increased concentrations of atmospheric constituents and other mechanisms.

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[8] *Radiative forcing* is a measure of the influence a factor has in altering the balance of incoming and outgoing energy in the Earth-atmosphere system, and is an index of the importance of the factor as a potential climate change mechanism. It is expressed in Watts per square metre (Wm^{-2}).

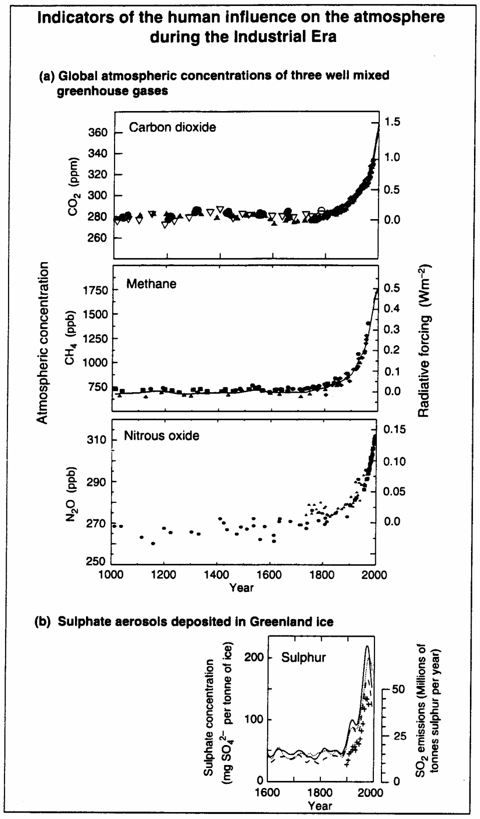

Figure 2: Long records of past changes in atmospheric composition provide the context for the influence of anthropogenic emissions.

(a) shows changes in the atmospheric concentrations of carbon dioxide (CO_2), methane (CH_4), and nitrous oxide (N_2O) over the past 1000 years. The ice core and firn data for several sites in Antarctica and Greenland (shown by different symbols) are supplemented with the data from direct atmospheric samples over the past few decades (shown by the line for CO_2 and incorporated in the curve representing the global average of CH_4). The estimated positive radiative forcing of the climate system from these gases is indicated on the right-hand scale. Since these gases have atmospheric lifetimes of a decade or more, they are well mixed, and their concentrations reflect emissions from sources throughout the globe. All three records show effects of the large and increasing growth in anthropogenic emissions during the Industrial Era.

(b) illustrates the influence of industrial emissions on atmospheric sulphate concentrations, which produce negative radiative forcing. Shown is the time history of the concentrations of sulphate, not in the atmosphere but in ice cores in Greenland (shown by lines; from which the episodic effects of volcanic eruptions have been removed). Such data indicate the local deposition of sulphate aerosols at the site, reflecting sulphur dioxide (SO_2) emissions at mid-latitudes in the Northern Hemisphere. This record, albeit more regional than that of the globally-mixed greenhouse gases, demonstrates the large growth in anthropogenic SO_2 emissions during the Industrial Era. The pluses denote the relevant regional estimated SO_2 emissions (right-hand scale).

[Based upon (a) Chapter 3, Figure 3.2b (CO_2); Chapter 4, Figure 4.1a and b (CH_4) and Chapter 4, Figure 4.2 (N_2O) and (b) Chapter 5, Figure 5.4a]

Concentrations of atmospheric greenhouse gases and their radiative forcing have continued to increase as a result of human activities.

- The atmospheric concentration of carbon dioxide (CO_2) has increased by 31% since 1750. The present CO_2 concentration has not been exceeded during the past 420,000 years and likely[7] not during the past 20 million years. The current rate of increase is unprecedented during at least the past 20,000 years.

- About three-quarters of the anthropogenic emissions of CO_2 to the atmosphere during the past 20 years is due to fossil fuel burning. The rest is predominantly due to land-use change, especially deforestation.

- Currently the ocean and the land together are taking up about half of the anthropogenic CO_2 emissions. On land, the uptake of anthropogenic CO_2 very likely[7] exceeded the release of CO_2 by deforestation during the 1990s.

- The rate of increase of atmospheric CO_2 concentration has been about 1.5 ppm[9] (0.4%) per year over the past two decades. During the 1990s the year to year increase varied from 0.9 ppm (0.2%) to 2.8 ppm (0.8%). A large part of this variability is due to the effect of climate variability (e.g., El Niño events) on CO_2 uptake and release by land and oceans.

- The atmospheric concentration of methane (CH_4) has increased by 1060 ppb[9] (151%) since 1750 and continues to increase. The present CH_4 concentration has not been exceeded during the past 420,000 years. The annual growth in CH_4 concentration slowed and became more variable in the 1990s, compared with the 1980s. Slightly more than half of current CH_4 emissions are anthropogenic (e.g., use of fossil fuels, cattle, rice agriculture and landfills). In addition, carbon monoxide (CO) emissions have recently been identified as a cause of increasing CH_4 concentration.

- The atmospheric concentration of nitrous oxide (N_2O) has increased by 46 ppb (17%) since 1750 and continues to increase. The present N_2O concentration has not been exceeded during at least the past thousand years. About a third of current N_2O emissions are anthropogenic (e.g., agricultural soils, cattle feed lots and chemical industry).

- Since 1995, the atmospheric concentrations of many of those halocarbon gases that are both ozone-depleting and greenhouse gases (e.g., $CFCl_3$ and CF_2Cl_2), are either increasing more slowly or decreasing, both in response to reduced emissions under the regulations of the Montreal Protocol and its Amendments. Their substitute compounds (e.g., CHF_2Cl and CF_3CH_2F) and some other synthetic compounds (e.g., perfluorocarbons (PFCs) and sulphur hexafluoride (SF_6)) are also greenhouse gases, and their concentrations are currently increasing.

- The radiative forcing due to increases of the well-mixed greenhouse gases from 1750 to 2000 is estimated to be 2.43 Wm^{-2}: 1.46 Wm^{-2} from CO_2; 0.48 Wm^{-2} from CH_4; 0.34 Wm^{-2} from the halocarbons; and 0.15 Wm^{-2} from N_2O. (See Figure 3, where the uncertainties are also illustrated.)

- The observed depletion of the stratospheric ozone (O_3) layer from 1979 to 2000 is estimated to have caused a negative radiative forcing (–0.15 Wm^{-2}). Assuming full compliance with current halocarbon regulations, the positive forcing of the halocarbons will be reduced as will the magnitude of the negative forcing from stratospheric ozone depletion as the ozone layer recovers over the 21st century.

- The total amount of O_3 in the troposphere is estimated to have increased by 36% since 1750, due primarily to anthropogenic emissions of several O_3-forming gases. This corresponds to a positive radiative forcing of 0.35 Wm^{-2}. O_3 forcing varies considerably by region and responds much more quickly to changes in emissions than the long-lived greenhouse gases, such as CO_2.

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[9] ppm (parts per million) or ppb (parts per billion, 1 billion = 1,000 million) is the ratio of the number of greenhouse gas molecules to the total number of molecules of dry air. For example: 300 ppm means 300 molecules of a greenhouse gas per million molecules of dry air.

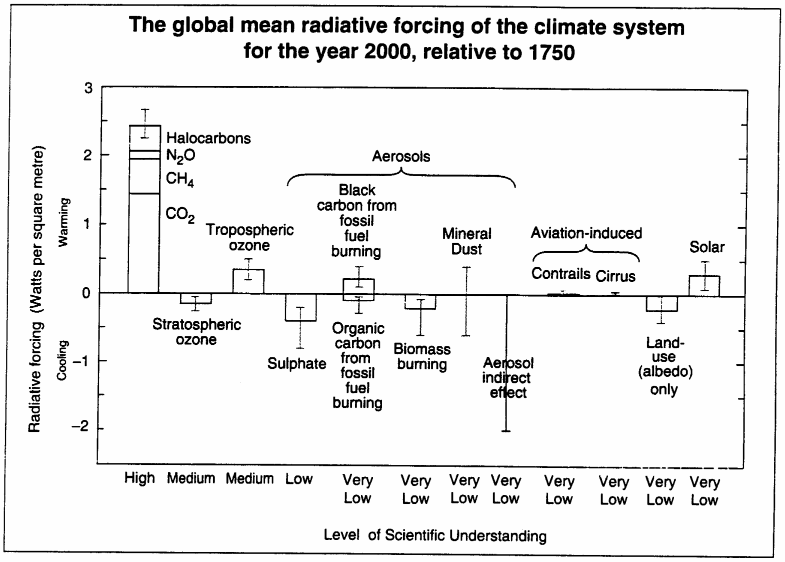

Figure 3: Many external factors force climate change.

These radiative forcings arise from changes in the atmospheric composition, alteration of surface reflectance by land use, and variation in the output of the sun. Except for solar variation, some form of human activity is linked to each. The rectangular bars represent estimates of the contributions of these forcings – some of which yield warming, and some cooling. Forcing due to episodic volcanic events, which lead to a negative forcing lasting only for a few years, is not shown. The indirect effect of aerosols shown is their effect on the size and number of cloud droplets. A second indirect effect of aerosols on clouds, namely their effect on cloud lifetime, which would also lead to a negative forcing, is not shown. Effects of aviation on greenhouse gases are included in the individual bars. The vertical line about the rectangular bars indicates a range of estimates, guided by the spread in the published values of the forcings and physical understanding. Some of the forcings possess a much greater degree of certainty than others. A vertical line without a rectangular bar denotes a forcing for which no best estimate can be given owing to large uncertainties. The overall level of scientific understanding for each forcing varies considerably, as noted. Some of the radiative forcing agents are well mixed over the globe, such as CO_2, thereby perturbing the global heat balance. Others represent perturbations with stronger regional signatures because of their spatial distribution, such as aerosols. For this and other reasons, a simple sum of the positive and negative bars cannot be expected to yield the net effect on the climate system. The simulations of this assessment report (for example, Figure 5) indicate that the estimated net effect of these perturbations is to have warmed the global climate since 1750. [Based upon Chapter 6, Figure 6.6]

Anthropogenic aerosols are short-lived and mostly produce negative radiative forcing.

- The major sources of anthropogenic aerosols are fossil fuel and biomass burning. These sources are also linked to degradation of air quality and acid deposition.
- Since the SAR, significant progress has been achieved in better characterising the direct radiative roles of different types of aerosols. Direct radiative forcing is estimated to be –0.4 Wm^{-2} for sulphate, –0.2 Wm^{-2} for biomass burning aerosols, –0.1 Wm^{-2} for fossil fuel organic carbon and +0.2 Wm^{-2} for fossil fuel black carbon aerosols. There is much less confidence in the ability to quantify the total aerosol direct effect, and its evolution over time, than that for the gases listed above. Aerosols also vary considerably by region and respond quickly to changes in emissions.
- In addition to their direct radiative forcing, aerosols have an indirect radiative forcing through their effects on clouds. There is now more evidence for this indirect effect, which is negative, although of very uncertain magnitude.

Natural factors have made small contributions to radiative forcing over the past century.

- The radiative forcing due to changes in solar irradiance for the period since 1750 is estimated to be about +0.3 Wm^{-2}, most of which occurred during the first half of the 20th century. Since the late 1970s, satellite instruments have observed small oscillations due to the 11-year solar cycle. Mechanisms for the amplification of solar effects on climate have been proposed, but currently lack a rigorous theoretical or observational basis.
- Stratospheric aerosols from explosive volcanic eruptions lead to negative forcing, which lasts a few years. Several major eruptions occurred in the periods 1880 to 1920 and 1960 to 1991.
- The combined change in radiative forcing of the two major natural factors (solar variation and volcanic aerosols) is estimated to be negative for the past two, and possibly the past four, decades.

Confidence in the ability of models to project future climate has increased.

Complex physically-based climate models are required to provide detailed estimates of feedbacks and of regional features. Such models cannot yet simulate all aspects of climate (e.g., they still cannot account fully for the observed trend in the surface-troposphere temperature difference since 1979) and there are particular uncertainties associated with clouds and their interaction with radiation and aerosols. Nevertheless, confidence in the ability of these models to provide useful projections of future climate has improved due to their demonstrated performance on a range of space and time-scales.

- Understanding of climate processes and their incorporation in climate models have improved, including water vapour, sea-ice dynamics, and ocean heat transport.
- Some recent models produce satisfactory simulations of current climate without the need for non-physical adjustments of heat and water fluxes at the ocean-atmosphere interface used in earlier models.
- Simulations that include estimates of natural and anthropogenic forcing reproduce the observed large-scale changes in surface temperature over the 20th century (Figure 4). However, contributions from some additional processes and forcings may not have been included in the models. Nevertheless, the large-scale consistency between models and observations can be used to provide an independent check on projected warming rates over the next few decades under a given emissions scenario.
- Some aspects of model simulations of ENSO, monsoons and the North Atlantic Oscillation, as well as selected periods of past climate, have improved.

There is new and stronger evidence that most of the warming observed over the last 50 years is attributable to human activities.

The SAR concluded: "The balance of evidence suggests a discernible human influence on global climate". That report also noted that the anthropogenic signal was still emerging from the background of natural climate variability. Since the SAR, progress has been made in reducing uncertainty, particularly with respect to distinguishing and quantifying the magnitude of responses to different external influences. Although many of the sources of uncertainty identified in the SAR still remain to some degree, new evidence and improved understanding support an updated conclusion.

- There is a longer and more closely scrutinised temperature record and new model estimates of variability. The warming over the past 100 years is very unlikely[7] to be due to internal variability alone, as estimated by current models. Reconstructions of climate data for the past 1,000 years (Figure 1b) also indicate that this warming was unusual and is unlikely[7] to be entirely natural in origin.
- There are new estimates of the climate response to natural and anthropogenic forcing, and new detection techniques have been applied. Detection and attribution studies consistently find evidence for an anthropogenic signal in the climate record of the last 35 to 50 years.
- Simulations of the response to natural forcings alone (i.e., the response to variability in solar irradiance and volcanic eruptions) do not explain the warming in the second half of the 20th century (see for example Figure 4a). However, they indicate that natural forcings may have contributed to the observed warming in the first half of the 20th century.
- The warming over the last 50 years due to anthropogenic greenhouse gases can be identified despite uncertainties in forcing due to anthropogenic sulphate aerosol and natural factors (volcanoes and solar irradiance). The anthropogenic sulphate aerosol forcing, while uncertain, is negative over this period and therefore cannot explain the warming. Changes in natural forcing during most of this period are also estimated to be negative and are unlikely[7] to explain the warming.
- Detection and attribution studies comparing model simulated changes with the observed record can now take into account uncertainty in the magnitude of modelled response to external forcing, in particular that due to uncertainty in climate sensitivity.
- Most of these studies find that, over the last 50 years, the estimated rate and magnitude of warming due to increasing concentrations of greenhouse gases alone are comparable with, or larger than, the observed warming. Furthermore, most model estimates that take into account both greenhouse gases and sulphate aerosols are consistent with observations over this period.
- The best agreement between model simulations and observations over the last 140 years has been found when all the above anthropogenic and natural forcing factors are combined, as shown in Figure 4c. These results show that the forcings included are sufficient to explain the observed changes, but do not exclude the possibility that other forcings may also have contributed.

In the light of new evidence and taking into account the remaining uncertainties, most of the observed warming over the last 50 years is likely[7] to have been due to the increase in greenhouse gas concentrations.

Furthermore, it is very likely[7] that the 20th century warming has contributed significantly to the observed sea level rise, through thermal expansion of sea water and widespread loss of land ice. Within present uncertainties, observations and models are both consistent with a lack of significant acceleration of sea level rise during the 20th century.

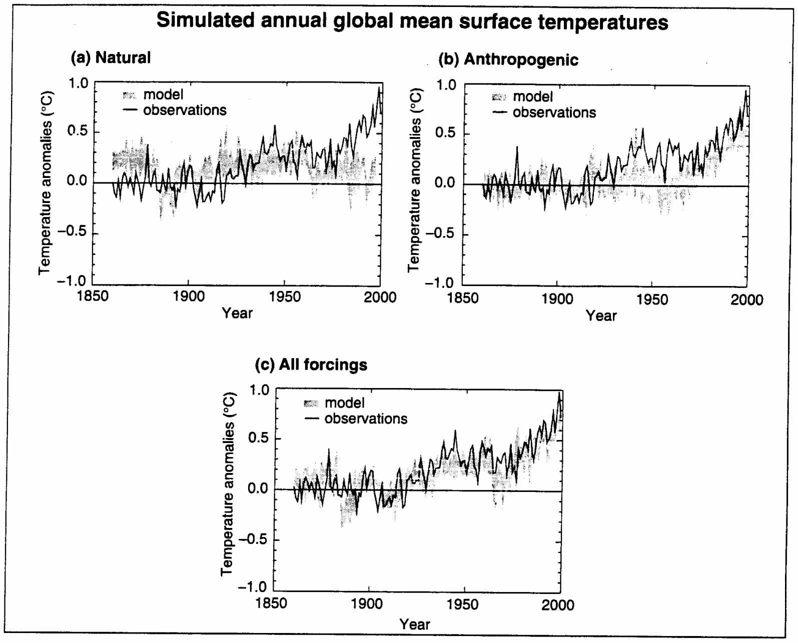

Figure 4: Simulating the Earth's temperature variations, and comparing the results to measured changes, can provide insight into the underlying causes of the major changes.

A climate model can be used to simulate the temperature changes that occur both from natural and anthropogenic causes. The simulations represented by the band in (a) were done with only natural forcings: solar variation and volcanic activity. Those encompassed by the band in (b) were done with anthropogenic forcings: greenhouse gases and an estimate of sulphate aerosols, and those encompassed by the band in (c) were done with both natural and anthropogenic forcings included. From (b), it can be seen that inclusion of anthropogenic forcings provides a plausible explanation for a substantial part of the observed temperature changes over the past century, but the best match with observations is obtained in (c) when both natural and anthropogenic factors are included. These results show that the forcings included are sufficient to explain the observed changes, but do not exclude the possibility that other forcings may also have contributed. The bands of model results presented here are for four runs from the same model. Similar results to those in (b) are obtained with other models with anthropogenic forcing. [Based upon Chapter 12, Figure 12.7]

Human influences will continue to change atmospheric composition throughout the 21st century.

Models have been used to make projections of atmospheric concentrations of greenhouse gases and aerosols, and hence of future climate, based upon emissions scenarios from the IPCC Special Report on Emission Scenarios (SRES) (Figure 5). These scenarios were developed to update the IS92 series, which were used in the SAR and are shown for comparison here in some cases.

Greenhouse gases

- Emissions of CO_2 due to fossil fuel burning are virtually certain[7] to be the dominant influence on the trends in atmospheric CO_2 concentration during the 21st century.
- As the CO_2 concentration of the atmosphere increases, ocean and land will take up a decreasing fraction of anthropogenic CO_2 emissions. The net effect of land and ocean climate feedbacks as indicated by models is to further increase projected atmospheric CO_2 concentrations, by reducing both the ocean and land uptake of CO_2.
- By 2100, carbon cycle models project atmospheric CO_2 concentrations of 540 to 970 ppm for the illustrative SRES scenarios (90 to 250% above the concentration of 280 ppm in the year 1750), Figure 5b. These projections include the land and ocean climate feedbacks. Uncertainties, especially about the magnitude of the climate feedback from the terrestrial biosphere, cause a variation of about −10 to +30% around each scenario. The total range is 490 to 1260 ppm (75 to 350% above the 1750 concentration).
- Changing land use could influence atmospheric CO_2 concentration. Hypothetically, if all of the carbon released by historical land-use changes could be restored to the terrestrial biosphere over the course of the century (e.g., by reforestation), CO_2 concentration would be reduced by 40 to 70 ppm.
- Model calculations of the concentrations of the non-CO_2 greenhouse gases by 2100 vary considerably across the SRES illustrative scenarios, with CH_4 changing by −190 to +1,970 ppb (present concentration 1,760 ppb), N_2O changing by +38 to +144 ppb (present concentration 316 ppb), total tropospheric O_3 changing by −12 to +62%, and a wide range of changes in concentrations of HFCs, PFCs and SF_6, all relative to the year 2000. In some scenarios, total tropospheric O_3 would become as important a radiative forcing agent as CH_4 and, over much of the Northern Hemisphere, would threaten the attainment of current air quality targets.
- Reductions in greenhouse gas emissions and the gases that control their concentration would be necessary to stabilise radiative forcing. For example, for the most important anthropogenic greenhouse gas, carbon cycle models indicate that stabilisation of atmospheric CO_2 concentrations at 450, 650 or 1,000 ppm would require global anthropogenic CO_2 emissions to drop below 1990 levels, within a few decades, about a century, or about two centuries, respectively, and continue to decrease steadily thereafter. Eventually CO_2 emissions would need to decline to a very small fraction of current emissions.

Aerosols

- The SRES scenarios include the possibility of either increases or decreases in anthropogenic aerosols (e.g., sulphate aerosols (Figure 5c), biomass aerosols, black and organic carbon aerosols) depending on the extent of fossil fuel use and policies to abate polluting emissions. In addition, natural aerosols (e.g., sea salt, dust and emissions leading to the production of sulphate and carbon aerosols) are projected to increase as a result of changes in climate.

Radiative forcing over the 21st century

- For the SRES illustrative scenarios, relative to the year 2000, the global mean radiative forcing due to greenhouse gases continues to increase through the 21st century, with the fraction due to CO_2 projected to increase from slightly more than half to about three quarters. The change in the direct plus indirect aerosol radiative forcing is projected to be smaller in magnitude than that of CO_2.

Global average temperature and sea level are projected to rise under all IPCC SRES scenarios.

In order to make projections of future climate, models incorporate past, as well as future emissions of greenhouse gases and aerosols. Hence, they include estimates of warming to date and the commitment to future warming from past emissions.

Temperature

- The globally averaged surface temperature is projected to increase by 1.4 to 5.8°C (Figure 5d) over the period 1990 to 2100. These results are for the full range of 35 SRES scenarios, based on a number of climate models[10,11].
- Temperature increases are projected to be greater than those in the SAR, which were about 1.0 to 3.5°C based on the six IS92 scenarios. The higher projected temperatures and the wider range are due primarily to the lower projected sulphur dioxide emissions in the SRES scenarios relative to the IS92 scenarios.
- The projected rate of warming is much larger than the observed changes during the 20th century and is very likely[7] to be without precedent during at least the last 10,000 years, based on palaeoclimate data.
- By 2100, the range in the surface temperature response across the group of climate models run with a given scenario is comparable to the range obtained from a single model run with the different SRES scenarios.
- On timescales of a few decades, the current observed rate of warming can be used to constrain the projected response to a given emissions scenario despite uncertainty in climate sensitivity. This approach suggests that anthropogenic warming is likely[7] to lie in the range of 0.1 to 0.2°C per decade over the next few decades under the IS92a scenario, similar to the corresponding range of projections of the simple model used in Figure 5d.
- Based on recent global model simulations, it is very likely[7] that nearly all land areas will warm more rapidly than the global average, particularly those at northern high latitudes in the cold season. Most notable of these is the warming in the northern regions of North America, and northern and central Asia, which exceeds global mean warming in each model by more than 40%. In contrast, the warming is less than the global mean change in south and southeast Asia in summer and in southern South America in winter.
- Recent trends for surface temperature to become more El Niño-like in the tropical Pacific, with the eastern tropical Pacific warming more than the western tropical Pacific, with a corresponding eastward shift of precipitation, are projected to continue in many models.

Precipitation

- Based on global model simulations and for a wide range of scenarios, global average water vapour concentration and precipitation are projected to increase during the 21st century. By the second half of the 21st century, it is likely[7] that precipitation will have increased over northern mid- to high latitudes and Antarctica in winter. At low latitudes there are both regional increases and decreases over land areas. Larger year to year variations in precipitation are very likely[7] over most areas where an increase in mean precipitation is projected.

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[10] Complex physically based climate models are the main tool for projecting future climate change. In order to explore the full range of scenarios, these are complemented by simple climate models calibrated to yield an equivalent response in temperature and sea level to complex climate models. These projections are obtained using a simple climate model whose climate sensitivity and ocean heat uptake are calibrated to each of seven complex climate models. The climate sensitivity used in the simple model ranges from 1.7 to 4.2°C, which is comparable to the commonly accepted range of 1.5 to 4.5°C.

[11] This range does not include uncertainties in the modelling of radiative forcing, e.g. aerosol forcing uncertainties. A small carbon-cycle climate feedback is included.

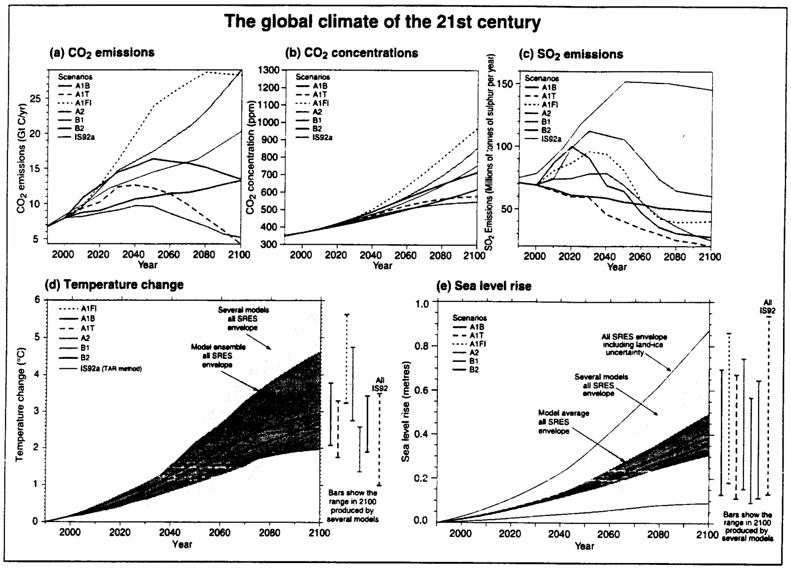

Figure 5: The global climate of the 21st century will depend on natural changes and the response of the climate system to human activities.

Climate models project the response of many climate variables – such as increases in global surface temperature and sea level – to various scenarios of greenhouse gas and other human-related emissions. (a) shows the CO_2 emissions of the six illustrative SRES scenarios, which are summarised in the box on page 18, along with IS92a for comparison purposes with the SAR. (b) shows projected CO_2 concentrations. (c) shows anthropogenic SO_2 emissions. Emissions of other gases and other aerosols were included in the model but are not shown in the figure. (d) and (e) show the projected temperature and sea level responses, respectively. The "several models all SRES envelope" in (d) and (e) shows the temperature and sea level rise, respectively, for the simple model when tuned to a number of complex models with a range of climate sensitivities. All SRES envelopes refer to the full range of 35 SRES scenarios. The "model average all SRES envelope" shows the average from these models for the range of scenarios. Note that the warming and sea level rise from these emissions would continue well beyond 2100. Also note that this range does not allow for uncertainty relating to ice dynamical changes in the West Antarctic ice sheet, nor does it account for uncertainties in projecting non-sulphate aerosols and greenhouse gas concentrations. [Based upon (a) Chapter 3, Figure 3.12, (b) Chapter 3, Figure 3.12, (c) Chapter 5, Figure 5.13, (d) Chapter 9, Figure 9.14, (e) Chapter 11, Figure 11.12, Appendix II]

Extreme Events

Table 1 depicts an assessment of confidence in observed changes in extremes of weather and climate during the latter half of the 20th century (left column) and in projected changes during the 21st century (right column)[a]. This assessment relies on observational and modelling studies, as well as the physical plausibility of future projections across all commonly-used scenarios and is based on expert judgement[7].

- For some other extreme phenomena, many of which may have important impacts on the environment and society, there is currently insufficient information to assess recent trends, and climate models currently lack the spatial detail required to make confident projections. For example, very small-scale phenomena, such as thunderstorms, tornadoes, hail and lightning, are not simulated in climate models.

Table 1: Estimates of confidence in observed and projected changes in extreme weather and climate events.

Confidence in observed changes (latter half of the 20th century)	Changes in Phenomenon	Confidence in projected changes (during the 21st century)
Likely[7]	**Higher maximum temperatures and more hot days over nearly all land areas**	Very likely[7]
Very likely[7]	**Higher minimum temperatures, fewer cold days and frost days over nearly all land areas**	Very likely[7]
Very likely[7]	**Reduced diurnal temperature range over most land areas**	Very likely[7]
Likely[7], over many areas	**Increase of heat index[12] over land areas**	Very likely[7], over most areas
Likely[7], over many Northern Hemisphere mid- to high latitude land areas	**More intense precipitation events[b]**	Very likely[7], over many areas
Likely[7], in a few areas	**Increased summer continental drying and associated risk of drought**	Likely[7], over most mid-latitude continental interiors. (Lack of consistent projections in other areas)
Not observed in the few analyses available	**Increase in tropical cyclone peak wind intensities[c]**	Likely[7], over some areas
Insufficient data for assessment	**Increase in tropical cyclone mean and peak precipitation intensities[c]**	Likely[7], over some areas

[a] For more details see Chapter 2 (observations) and Chapter 9, 10 (projections).

[b] For other areas, there are either insufficient data or conflicting analyses.

[c] Past and future changes in tropical cyclone location and frequency are uncertain.

---

[12] Heat index: A combination of temperature and humidity that measures effects on human comfort.

El Niño

- Confidence in projections of changes in future frequency, amplitude, and spatial pattern of El Niño events in the tropical Pacific is tempered by some shortcomings in how well El Niño is simulated in complex models. Current projections show little change or a small increase in amplitude for El Niño events over the next 100 years.

- Even with little or no change in El Niño amplitude, global warming is likely[7] to lead to greater extremes of drying and heavy rainfall and increase the risk of droughts and floods that occur with El Niño events in many different regions.

Monsoons

- It is likely[7] that warming associated with increasing greenhouse gas concentrations will cause an increase of Asian summer monsoon precipitation variability. Changes in monsoon mean duration and strength depend on the details of the emission scenario. The confidence in such projections is also limited by how well the climate models simulate the detailed seasonal evolution of the monsoons.

Thermohaline circulation

- Most models show weakening of the ocean thermohaline circulation which leads to a reduction of the heat transport into high latitudes of the Northern Hemisphere. However, even in models where the thermohaline circulation weakens, there is still a warming over Europe due to increased greenhouse gases. The current projections using climate models do not exhibit a complete shut-down of the thermohaline circulation by 2100. Beyond 2100, the thermohaline circulation could completely, and possibly irreversibly, shut-down in either hemisphere if the change in radiative forcing is large enough and applied long enough.

Snow and ice

- Northern Hemisphere snow cover and sea-ice extent are projected to decrease further.

- Glaciers and ice caps are projected to continue their widespread retreat during the 21st century.

- The Antarctic ice sheet is likely[7] to gain mass because of greater precipitation, while the Greenland ice sheet is likely[7] to lose mass because the increase in runoff will exceed the precipitation increase.

- Concerns have been expressed about the stability of the West Antarctic ice sheet because it is grounded below sea level. However, loss of grounded ice leading to substantial sea level rise from this source is now widely agreed to be very unlikely[7] during the 21st century, although its dynamics are still inadequately understood, especially for projections on longer time-scales.

Sea level

- Global mean sea level is projected to rise by 0.09 to 0.88 metres between 1990 and 2100, for the full range of SRES scenarios. This is due primarily to thermal expansion and loss of mass from glaciers and ice caps (Figure 5e). The range of sea level rise presented in the SAR was 0.13 to 0.94 metres based on the IS92 scenarios. Despite the higher temperature change projections in this assessment, the sea level projections are slightly lower, primarily due to the use of improved models, which give a smaller contribution from glaciers and ice sheets.

Anthropogenic climate change will persist for many centuries.

- Emissions of long-lived greenhouse gases (i.e., CO_2, N_2O, PFCs, SF_6) have a lasting effect on atmospheric composition, radiative forcing and climate. For example, several centuries after CO_2 emissions occur, about a quarter of the increase in CO_2 concentration caused by these emissions is still present in the atmosphere.
- After greenhouse gas concentrations have stabilised, global average surface temperatures would rise at a rate of only a few tenths of a degree per century rather than several degrees per century as projected for the 21st century without stabilisation. The lower the level at which concentrations are stabilised, the smaller the total temperature change.
- Global mean surface temperature increases and rising sea level from thermal expansion of the ocean are projected to continue for hundreds of years after stabilisation of greenhouse gas concentrations (even at present levels), owing to the long timescales on which the deep ocean adjusts to climate change.
- Ice sheets will continue to react to climate warming and contribute to sea level rise for thousands of years after climate has been stabilised. Climate models indicate that the local warming over Greenland is likely[7] to be one to three times the global average. Ice sheet models project that a local warming of larger than 3°C, if sustained for millennia, would lead to virtually a complete melting of the Greenland ice sheet with a resulting sea level rise of about 7 metres. A local warming of 5.5°C, if sustained for 1,000 years, would be likely[7] to result in a contribution from Greenland of about 3 metres to sea level rise.
- Current ice dynamic models suggest that the West Antarctic ice sheet could contribute up to 3 metres to sea level rise over the next 1,000 years, but such results are strongly dependent on model assumptions regarding climate change scenarios, ice dynamics and other factors.

Further action is required to address remaining gaps in information and understanding.

Further research is required to improve the ability to detect, attribute and understand climate change, to reduce uncertainties and to project future climate changes. In particular, there is a need for additional systematic and sustained observations, modelling and process studies. A serious concern is the decline of observational networks. The following are high priority areas for action.

- Systematic observations and reconstructions:
  - Reverse the decline of observational networks in many parts of the world.
  - Sustain and expand the observational foundation for climate studies by providing accurate, long-term, consistent data including implementation of a strategy for integrated global observations.
  - Enhance the development of reconstructions of past climate periods.
  - Improve the observations of the spatial distribution of greenhouse gases and aerosols.
- Modelling and process studies:
  - Improve understanding of the mechanisms and factors leading to changes in radiative forcing.
  - Understand and characterise the important unresolved processes and feedbacks, both physical and biogeochemical, in the climate system.
  - Improve methods to quantify uncertainties of climate projections and scenarios, including long-term ensemble simulations using complex models.
  - Improve the integrated hierarchy of global and regional climate models with a focus on the simulation of climate variability, regional climate changes and extreme events.
  - Link more effectively models of the physical climate and the biogeochemical system, and in turn improve coupling with descriptions of human activities.

Cutting across these foci are crucial needs associated with strengthening international co-operation and co-ordination in order to better utilise scientific, computational and observational resources. This should also promote the free exchange of data among scientists. A special need is to increase the observational and research capacities in many regions, particularly in developing countries. Finally, as is the goal of this assessment, there is a continuing imperative to communicate research advances in terms that are relevant to decision making.

The Emissions Scenarios of the Special Report on Emissions Scenarios (SRES)

A1. The A1 storyline and scenario family describes a future world of very rapid economic growth, global population that peaks in mid-century and declines thereafter, and the rapid introduction of new and more efficient technologies. Major underlying themes are convergence among regions, capacity building and increased cultural and social interactions, with a substantial reduction in regional differences in per capita income. The A1 scenario family develops into three groups that describe alternative directions of technological change in the energy system. The three A1 groups are distinguished by their technological emphasis: fossil intensive (A1FI), non-fossil energy sources (A1T), or a balance across all sources (A1B) (where balanced is defined as not relying too heavily on one particular energy source, on the assumption that similar improvement rates apply to all energy supply and end use technologies).

A2. The A2 storyline and scenario family describes a very heterogeneous world. The underlying theme is self-reliance and preservation of local identities. Fertility patterns across regions converge very slowly, which results in continuously increasing population. Economic development is primarily regionally oriented and per capita economic growth and technological change more fragmented and slower than other storylines.

B1. The B1 storyline and scenario family describes a convergent world with the same global population, that peaks in mid-century and declines thereafter, as in the A1 storyline, but with rapid change in economic structures toward a service and information economy, with reductions in material intensity and the introduction of clean and resource-efficient technologies. The emphasis is on global solutions to economic, social and environmental sustainability, including improved equity, but without additional climate initiatives.

B2. The B2 storyline and scenario family describes a world in which the emphasis is on local solutions to economic, social and environmental sustainability. It is a world with continuously increasing global population, at a rate lower than A2, intermediate levels of economic development, and less rapid and more diverse technological change than in the B1 and A1 storylines. While the scenario is also oriented towards environmental protection and social equity, it focuses on local and regional levels.

An illustrative scenario was chosen for each of the six scenario groups A1B, A1FI, A1T, A2, B1 and B2. All should be considered equally sound.

The SRES scenarios do not include additional climate initiatives, which means that no scenarios are included that explicitly assume implementation of the United Nations Framework Convention on Climate Change or the emissions targets of the Kyoto Protocol.

Source Information: Summary for Policymakers

This appendix provides the cross-reference of the topics in the Summary for Policymakers (page and bullet point topic) to the sections of the chapters of the full report that contain expanded information about the topic.

An increasing body of observations gives a collective picture of a warming world and other changes in the climate system.

SPM Page	Cross-Reference: SPM Topic – Chapter Section
2	*The global average surface temperature has increased over the 20th century by about 0.6°C.* ● Chapter 2.2.2 ● Chapter 2.2.2 ● Chapter 2.3 ● Chapter 2.2.2
4	*Temperatures have risen during the past four decades in the lowest 8 kilometres of the atmosphere.* ● Chapter 2.2.3 and 2.2.4 ● Chapter 2.2.3 and 2.2.4 ● Chapter 2.2.3, 2.2.4 and Chapter 12.3.2
4	*Snow cover and ice extent have decreased.* All three bullet points: Chapter 2.2.5 and 2.2.6
4	*Global average sea level has risen and ocean heat content has increased.* ● Chapter 11.3.2 ● Chapter 2.2.2 and Chapter 11.2.1
4 – 5	*Changes have also occurred in other important aspects of climate.* ● Chapter 2.5.2 ● Chapter 2.7.2 ● Chapter 2.2.2 and 2.5.5 ● Chapter 2.7.2 ● Chapter 2.6.2 and 2.6.3 ● Chapter 2.7.3 ● Chapter 2.7.3
5	*Some important aspects of climate appear not to have changed.* ● Chapter 2.2.2 ● Chapter 2.2.5 ● Chapter 2.7.3 ● Chapter 2.7.3

Emissions of greenhouse gases and aerosols due to human activities continue to alter the atmosphere in ways that are expected to affect the climate system.

SPM Page	Cross-Reference: SPM Topic – Chapter Section
5	Chapeau: "Changes in climate occur ..." Chapter 1, Chapter 3.1, Chapter 4.1, Chapter 5.1, Chapter 6.1, 6.2, 6.9, 6.11 and 6.13
7	*Concentrations of atmospheric greenhouse gases and their radiative forcing have continued to increase as a result of human activities.*
	Carbon dioxide: ● Chapter 3.3.1, 3.3.2, 3.3.3 and 3.5.1 ● Chapter 3.5.1 ● Chapter 3.2.2, 3.2.3, 3.5.1 and Table 3.1 ● Chapter 3.5.1 and 3.5.2
	Methane: ● Chapter 4.2.1
	Nitrous oxide: ● Chapter 4.2.1
	Halocarbons: ● Chapter 4.2.2
	Radiative forcing of well-mixed gases: ● Chapter 4.2.1 and Chapter 6.3
	Stratospheric ozone: ● Chapter 4.2.2 and Chapter 6.4
	Tropospheric ozone: ● Chapter 4.2.4 and Chapter 6.5
9	*Anthropogenic aerosols are short-lived and mostly produce negative radiative forcing.* ● Chapter 5.2 and 5.5.4 ● Chapter 5.1, 5.2 and Chapter 6.7 ● Chapter 5.3.2, 5.4.3 and Chapter 6.8
9	*Natural factors have made small contributions to radiative forcing over the past century.* ● Chapter 6.11 and 6.15.1 ● Chapter 6.9 and 6.15.1 ● Chapter 6.15.1

Confidence in the ability of models to project future climate has increased.

SPM Page	Cross-Reference: SPM Topic – Chapter Section
9	Chapeau: "Complex physically-based ..." Chapter 8.3.2, 8.5.1, 8.6.1, 8.10.3 and Chapter 12.3.2
9	● Chapter 7.2.1, 7.5.2 and 7.6.1 ● Chapter 8.4.2 ● Chapter 8.6.3 and Chapter 12.3.2 ● Chapter 8.5.5, 8.7.1 and 8.7.5

There is new and stronger evidence that most of the warming observed over the last 50 years is attributable to human activities.

SPM Page	Cross-Reference: SPM Topic – Chapter Section
10	Chapeau: "The SAR concluded: The balance of evidence suggests ..." Chapter 12.1.2 and 12.6
10	● Chapter 12.2.2, 12.4.3 and 12.6 ● Chapter 12.4.1, 12.4.2, 12.4.3 and 12.6 ● Chapter 12.2.3, 12.4.1, 12.4.2, 12.4.3 and 12.6 ● Chapter 12.4.3 and 12.6. ● Chapter 12.6 ● Chapter 12.4.3 ● Chapter 12.4.3 and 12.6
10	"In the light of new evidence and taking into account the ..." Chapter 12.4 and 12.6
10	"Furthermore, it is very likely that the 20th century warming has ..." Chapter 11.4

Human influences will continue to change atmospheric composition throughout the 21st century.

SPM Page	Cross-Reference: SPM Topic – Chapter Section
12	Chapeau: "Models have been used to make projections ..." Chapter 4.4.5 and Appendix II
12	*Greenhouse gases* ● Chapter 3.7.3 and Appendix II ● Chapter 3.7.1, 3.7.2, 3.7.3 and Appendix II ● Chapter 3.7.3 and Appendix II ● Chapter 3.2.2 and Appendix II ● Chapter 4.4.5, 4.5, 4.6 and Appendix II ● Chapter 3.7.3
12	*Aerosols* ● Chapter 5.5.2, 5.5.3 and Appendix II
12	*Radiative forcing over the 21st century* ● Chapter 6.15.2 and Appendix II

Global average temperature and sea level are projected to rise under all IPCC SRES scenarios.

SPM Page	Cross-Reference: SPM Topic – Chapter Section
13	*Temperature* ● Chapter 9.3.3 ● Chapter 9.3.3 ● Chapter 2.2.2, 2.3.2 and 2.4 ● Chapter 9.3.3 and Chapter 10.3.2 ● Chapter 8.6.1, Chapter 12.4.3, Chapter 13.5.1 and 13.5.2 ● Chapter 10.3.2 and Box 10.1 ● Chapter 9.3.2
13	*Precipitation* ● Chapter 9.3.1, 9.3.6, Chapter 10.3.2 and Box 10.1
15	*Extreme events* Table 1: Chapter 2.1, 2.2, 2.5, 2.7.2, 2.7.3, Chapter 9.3.6 and Chapter 10.3.2 ● Chapter 2.7.3 and Chapter 9.3.6
16	*El Niño* ● Chapter 9.3.5 ● Chapter 9.3.5
16	*Monsoons* ● Chapter 9.3.5
16	*Thermohaline circulation* ● Chapter 9.3.4
16	*Snow and ice* ● Chapter 9.3.2 ● Chapter 11.5.1 ● Chapter 11.5.1 ● Chapter 11.5.4
16	*Sea level* ● Chapter 11.5.1

Anthropogenic climate change will persist for many centuries.

SPM Page	Cross-Reference: SPM Topic – Chapter Section
17	● Chapter 3.2.3, Chapter 4.4 and Chapter 6.15 ● Chapter 9.3.3 and 9.3.4 ● Chapter 11.5.4 ● Chapter 11.5.4 ● Chapter 11.5.4

Further work is required to address remaining gaps in information and understanding.

SPM Page	Cross-Reference: SPM Topic – Chapter Section
17 – 18	All bullet points: Chapter 14, Executive Summary

2

Observed Climate Variability and Change

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Contents

Executive Summary 101

2.1 Introduction 105

2.2 How Much is the World Warming? 105
- 2.2.1 Background 105
- 2.2.2 Temperature in the Instrumental Record for Land and Oceans 105
  - 2.2.2.1 Land-surface air temperature 105
  - 2.2.2.2 Sea surface temperature and ocean air temperature 110
  - 2.2.2.3 Land and sea combined 112
  - 2.2.2.4 Are the land and ocean surface temperature changes mutually consistent? 116
  - 2.2.2.5 Sub-surface ocean temperatures and salinities 118
- 2.2.3 Temperature of the Upper Air 119
- 2.2.4 How do Surface and Upper Air Temperature Variations Compare? 121
- 2.2.5 Changes in the Cryosphere 123
  - 2.2.5.1 Snow cover, including snowfall 123
  - 2.2.5.2 Sea-ice extent and thickness 124
  - 2.2.5.3 Permafrost 127
  - 2.2.5.4 Mountain glaciers 127
  - 2.2.5.5 Lake and river ice 129
- 2.2.6 Are the Retreat of Glaciers, Sea Ice, and Snow Cover Consistent with the Surface Temperature Trends? 129
- 2.2.7 Summary 129

2.3 Is the Recent Warming Unusual? 130
- 2.3.1 Background 130
- 2.3.2 Temperature of the Past 1,000 Years 130
  - 2.3.2.1 Palaeoclimate proxy indicators 130
  - 2.3.2.2 Multi-proxy synthesis of recent temperature change 133
- 2.3.3 Was there a "Little Ice Age" and a "Medieval Warm Period"? 133
- 2.3.4 Volcanic and Solar Effects in the Recent Record 136
- 2.3.5 Summary 136

2.4 How Rapidly did Climate Change in the Distant Past? 136
- 2.4.1 Background 136
- 2.4.2 How Stable was the Holocene Climate? 138
- 2.4.3 How Fast did Climate Change during the Glacial Period? 140
- 2.4.4 How Stable was the Previous Inter-glacial? 141
- 2.4.5 Summary 142

2.5 How have Precipitation and Atmospheric Moisture Changed? 142
- 2.5.1 Background 142
- 2.5.2 Changes in Precipitation and Related Variables 142
  - 2.5.2.1 Land 142
  - 2.5.2.2 Palaeo-drought 143
  - 2.5.2.3 Ocean 145
- 2.5.3 Water Vapour 146
  - 2.5.3.1 Surface water vapour 146
  - 2.5.3.2 Lower-tropospheric water vapour 147
  - 2.5.3.3 Upper-tropospheric and lower-stratospheric water vapour 147
- 2.5.4 Evaporation 148
  - 2.5.4.1 Land 148
- 2.5.5 Clouds 148
  - 2.5.5.1 Land 149
  - 2.5.5.2 Ocean 149
  - 2.5.5.3 Global 149
- 2.5.6 Summary 149

2.6 Are the Atmospheric/Oceanic Circulations Changing? 150
- 2.6.1 Background 150
- 2.6.2 El Niño-Southern Oscillation and Tropical/Extra-tropical Interaction 150
- 2.6.3 Decadal to Inter-decadal Pacific Oscillation, and the North Pacific Oscillation 151
- 2.6.4 Monsoons 152
- 2.6.5 The Northern Hemisphere, excluding the North Pacific Ocean 152
- 2.6.6 The Southern Hemisphere 153
- 2.6.7 Summary 154

2.7 Has Climate Variability, or have Climate Extremes, Changed? 155
- 2.7.1 Background 155
- 2.7.2 Is There Evidence for Changes in Variability or Extremes? 155
  - 2.7.2.1 Temperature 156
  - 2.7.2.2 Precipitation 157
- 2.7.3 Is There Evidence for Changes in Extreme Weather or Climate Events? 160
  - 2.7.3.1 Tropical cyclones 160
  - 2.7.3.2 Extra-tropical cyclones 160
  - 2.7.3.3 Droughts and wet spells 162
  - 2.7.3.4 Tornadoes, hail and other severe local weather 163
- 2.7.4 Summary 163

2.8 Are the Observed Trends Internally Consistent? 163

References 165

2.1 Introduction

Observed climate change and variability (for definitions, see the IPCC Glossary, Appendix I) are considered in this chapter by addressing seven commonly asked questions related to the detection of climate change and sensitivity of the climate to anthropogenic activity. The questions are:

How much is the world warming?
Is the recent warming unusual?
How rapidly did climate change in the distant past?
Have precipitation and atmospheric moisture changed?
Are the atmospheric/oceanic circulations changing?
Has climate variability, or have climate extremes, changed?
Are the observed trends internally consistent?

This chapter emphasises change against a background of variability. The certainty of conclusions that can be drawn about climate from observations depends critically on the availability of accurate, complete and consistent series of observations. For many variables important in documenting, detecting, and attributing climate change, Karl *et al.* (1995a) demonstrate that the data are still not good enough for really firm conclusions to be reached, as noted in the IPCC WGI Second Assessment Report (IPCC, 1996) (hereafter SAR). This especially applies to global trends in variables that have large regional variations, such as precipitation, whereas conclusions about temperature changes are often considerably more firmly based. The recently designated Global Climate Observing System (GCOS) upper air network (Wallis, 1998) and a GCOS surface network (Peterson *et al.*, 1997), maintained and reporting to higher standards, may have had a limited positive impact on the quality and availability of some of our results. New data sets e.g., on surface humidity, sea-ice thickness and sub-surface ocean temperature, have widened the range of conclusions than can be drawn since the SAR, albeit tentatively. However, a wider range of analytical techniques and tests of the data have increased our confidence in areas such as surface temperature changes.

Throughout the chapter we try to consistently indicate the degree of our confidence in trends and other results. Sometimes we provide quantitative estimates of uncertainty, as far as possible the value of twice the standard error, or we estimate statistical significance at the 0.05 (5%) level. This is the appropriate terminology and implies that what we see is very unusual, given the null hypothesis. We use the word "trend" to designate a generally progressive change in the level of a variable. Where numerical values are given, they are equivalent linear trends, though more complex changes in the variable will often be clear from the description. We use the word "consistent" to imply similarity between results or data sets that are expected to be related on physical grounds. Where this is not possible, we use the following words to indicate judgmental estimates of confidence: virtually certain (>99% chance that a result is true); very likely (≥90% but ≤99% chance); likely (>66% but <90% chance); medium likelihood (>33% but ≤66% chance), unlikely (>10% but ≤33% chance); very unlikely (≥1% but ≤10% chance) and exceptionally unlikely (<1% chance).

2.2 How Much is the World Warming?

2.2.1 Background

The SAR concluded that, on a global average, land-surface air and sea surface temperature rose by between 0.3°C and 0.6°C between the late 19th century and 1994. In this section, the recent warming is re-examined, using updated data. We include recent analyses of the diurnal asymmetry of the warming and its geographical structure. Conventional temperature observations are supplemented by indirect evidence and by satellite-based data. For the first time, we make objective estimates of uncertainties in the surface temperature data, though these are preliminary. We also assess recent work in compiling hemispheric and global temperature records from palaeoclimatic data, especially for the most recent millennium.

2.2.2 Temperature in the Instrumental Record for Land and Oceans

Note that all data sets are adjusted to have zero anomaly when averaged over the period 1961 to 1990.

2.2.2.1 Land-surface air temperature

The SAR reviewed the three databases of land-surface air temperature due to Jones (1994), Hansen and Lebedeff (1988) and Vinnikov *et al.* (1990). The first and second databases have been updated by Jones *et al.* (2001) and Hansen *et al.* (1999), respectively, and a further analysis has become available (Peterson and Vose, 1997; Peterson *et al.*, 1998a, 1999). The last paper also separates rural temperature stations in the Global Historical Climatology Network (GHCN) (Peterson and Vose, 1997) from the full set of stations which, in common with the other three analyses, have been screened for urbanisation effects. While there is little difference in the long-term (1880 to 1998) rural (0.70°C/century) and full set of station temperature trends (actually less at 0.65°C/century), more recent data (1951 to 1989), as cited in Peterson *et al.* (1999), do suggest a slight divergence in the rural (0.80°C/century) and full set of station trends (0.92°C/century). However, neither pair of differences is statistically significant. In addition, while not reported in Peterson *et al.*, the 1951 to 1989 trend for urban stations alone was 0.10°C/decade. We conclude that estimates of long-term (1880 to 1998) global land-surface air temperature variations and trends are relatively little affected by whether the station distribution typically used by the four global analyses is used, or whether a special effort is made to concentrate on rural stations using elaborate criteria to identify them. Part of the reason for this lack of sensitivity is that the average trends in available worldwide *urban* stations for 1951 to 1989 are not greatly more than those for all land stations (0.09°C/decade). The differences in trend between rural and all stations are also virtually unaffected by elimination of areas of largest temperature change, like Siberia, because such areas are well represented in both sets of stations.

These results confirm the conclusions of Jones *et al.* (1990) and Easterling *et al.* (1997) that urban effects on 20th century globally and hemispherically averaged land air temperature time-

Analyses of overall temperature trends in the low to mid-oposphere and near the surface since 1958 are in good agreement, with a warming of about 0.1°C per decade. Since the beginning of the satellite record (1979), however, low to mid-troposphere temperatures have warmed in both satellite and weather balloon records at a global rate of only 0.04 and 0.03°C/decade respectively. This is about 0.12°C/decade less than the rate of temperature increase near the surface since 1979. About half of this difference in warming rate is very likely to be due to the combination of differences in spatial coverage and the real physical affects of volcanoes and ENSO (Santer *et al.*, 2000), see also Chapter 12. The remaining difference remains unexplained, but is likely to be real. In the stratosphere, both satellites and weather balloons continue to show substantial cooling. The faster rate of recession of tropical mountain glaciers in the last twenty years than might have been expected from the MSU and radiosonde records remains unexplained, though some glaciers may still be responding to the warming indicated by radiosondes that occurred around 1976 to 1981.

2.3 Is the Recent Warming Unusual?

2.3.1 Background

To determine whether 20th century warming is unusual, it is essential to place it in the context of longer-term climate variability. Owing to the sparseness of instrumental climate records prior to the 20th century (especially prior to the mid-19th ntury), estimates of global climate variability during past centuries must often rely upon indirect "proxy" indicators – natural or human documentary archives that record past climate variations, but must be calibrated against instrumental data for a meaningful climate interpretation (Bradley, 1999, gives a review). Coarsely resolved climate trends over several centuries are evident in many regions e.g., from the recession of glaciers (Grove and Switsur, 1994; and Section 2.2.5.4) or the geothermal information provided by borehole measurements (Pollack *et al.*, 1998). Large-scale estimates of decadal, annual or seasonal climate variations in past centuries, however, must rely upon sources that resolve annual or seasonal climatic variations. Such proxy information includes width and density measurements from tree rings (e.g., Cook, 1995; see Fritts, 1991, for a review), layer thickness from laminated sediment cores (e.g., Hughen *et al.*, 1996; Lamoureux and Bradley, 1996), isotopes, chemistry, and accumulation from annually resolved ice cores (e.g., Claussen *et al.*, 1995; Fisher *et al.*, 1998), isotopes from corals (e.g., Tudhope *et al.*, 1995; Dunbar and Cole, 1999), and the sparse historical documentary evidence available over the globe during the past few centuries (see e.g., Bradley and Jones, 1995; Pfister *et al.*, 1998). Taken as a whole, such proxy climate data can provide global scale sampling of climate variations several centuries into the past, with the potential to resolve large-scale patterns of climate change prior to the instrumental period, albeit with important limitations and uncertainties.

The SAR examined evidence for climate change in the past, n time-scales of centuries to millennia. Based on information rom a variety of proxy climate indicators, reconstructions of mountain glacier mass and extent, and geothermal sub-surface information from boreholes, it was concluded that summer temperatures in the Northern Hemisphere during recent decades are the warmest in at least six centuries. While data prior to AD 1400 were considered too sparse for reliable inferences regarding hemispheric or global mean temperatures, regional inferences were nonetheless made about climate changes further back in time.

Since the SAR, a number of studies based on considerably expanded databases of palaeoclimate information have allowed more decisive conclusions about the spatial and temporal patterns of climate change in past centuries. A number of important advances have been in key areas such as ice core palaeoclimatology (e.g., White *et al.*, 1998a), dendroclimatology (e.g., Cook, 1995; Briffa *et al.*, 1998b), and geothermal palaeo-temperature estimation (e.g., Pollack *et al.*, 1998). Moreover, the latest studies based on global networks of "multi-proxy" data have proved particularly useful for describing global or hemispheric patterns of climate variability in past centuries (e.g., Bradley and Jones, 1993; Hughes and Diaz, 1994; Mann *et al.*, 1995; Fisher, 1997; Overpeck *et al.*, 1997; Mann *et al.*, 1998, 1999). Such estimates allow the observed trends of the 20th century to be put in a longer-term perspective. These have also allowed better comparisons with possible physical influences on climate forcings (Lean *et al.*, 1995; Crowley and Kim, 1996, 1999; Overpeck *et al.*, 1997; Mann *et al.*, 1998; Waple *et al.*, 2001), and for new evaluations of the low-frequency climate variability exhibited by numerical climate models (Barnett *et al.*, 1996; Jones *et al.*, 1998; Crowley and Kim, 1999; Delworth and Mann, 2000).

2.3.2 Temperature of the Past 1,000 Years

The past 1,000 years are a particularly important time-frame for assessing the background natural variability of the climate for climate change detection. Astronomical boundary conditions have strayed relatively little from their modern-day values over this interval (but see Section 2.3.4 for a possible caveat) and, with the latest evidence, the spatial extent of large-scale climate change during the past millennium can now be meaningfully characterised (Briffa *et al.*, 1998b; Jones *et al.*, 1998; Mann *et al.*, 1998; 1999; 2000a; 2000b). Moreover, estimates of volcanic and solar climate forcings are also possible over this period, allowing model-based estimates of their climate effects (Crowley and Kim, 1999; Free and Robock, 1999).

2.3.2.1 Palaeoclimate proxy indicators

A "proxy" climate indicator is a local record that is interpreted using physical or biophysical principles to represent some combination of climate-related variations back in time. Palaeoclimate proxy indicators have the potential to provide evidence for large-scale climatic changes prior to the existence of widespread instrumental or historical documentary records. Typically, the interpretation of a proxy climate record is complicated by the presence of "noise" in which climate information is immersed, and a variety of possible distortions of the underlying climate information (e.g., Bradley, 1999; Ren, 1999a,b). Careful calibration and cross-validation procedures are necessary to establish a reliable relationship between a proxy indicator and the climatic variable or variables it is assumed to represent, providing a "transfer" function through which past climatic conditions can be estimated. High-resolution proxy climate indicators, including tree rings, corals, ice cores, and laminated lake/ocean sediments, can be used to provide detailed information on annual or near-annual climate variations back in time. Certain coarser resolution proxy information (from e.g., boreholes, glacial moraines, and non-laminated ocean sediment records) can usefully supplement this high-resolution information. Important recent advances in the development and interpretation of proxy climate indicators are described below.

Tree rings

Tree-ring records of past climate are precisely dated, annually resolved, and can be well calibrated and verified (Fritts, 1976). They typically extend from the present to several centuries or more into the past, and so are useful for documenting climate change in terrestrial regions of the globe. Many recent studies have sought to reconstruct warm-season and annual temperatures several centuries or more ago from either the width or the density of annual growth rings (Briffa *et al.*, 1995; D'Arrigo *et al.*, 1996; Jacoby *et al.*, 1996; D'Arrigo *et al.*, 1998; Wiles *et al.*, 1998; Hughes *et al.*, 1999; Cook *et al.*, 2000). Recently, there has been a concerted effort to develop spatial reconstructions of past temperature variations (e.g., Briffa *et al.*, 1996) and estimates of hemispheric and global temperature change (e.g., Briffa *et al.*, 1998b; Briffa, 2000). Tree-ring networks are also now being used to reconstruct important indices of climate variability over several centuries such as the Southern Oscillation Index (Stahle *et al.*, 1998), the North Atlantic Oscillation (Cook *et al.*, 1998; Cullen *et al.*, 2001) and the Antarctic Oscillation Index (Villalba *et al.*, 1997) (see also Section 2.6), as well as patterns of pre-instrumental precipitation and drought (Section 2.5.2.2).

Several important caveats must be borne in mind when using tree-ring data for palaeoclimate reconstructions. Not least is the intrinsic sampling bias. Tree-ring information is available only in terrestrial regions, so is not available over substantial regions of the globe, and the climate signals contained in tree-ring density or width data reflect a complex biological response to climate forcing. Non-climatic growth trends must be removed from the tree-ring chronology, making it difficult to resolve time-scales longer than the lengths of the constituent chronologies (Briffa, 2000). Furthermore, the biological response to climate forcing may change over time. There is evidence, for example, that high latitude tree-ring density variations have changed in their response to temperature in recent decades, associated with possible non-climatic factors (Briffa *et al.*, 1998a). By contrast, Vaganov *et al.* (1999) have presented evidence that such changes may actually be climatic and result from the effects of increasing winter precipitation on the starting date of the growing season (see Section 2.7.2.2). Carbon dioxide fertilization may also have an influence, particularly on high-elevation drought-sensitive tree species, although attempts have been made to correct for this effect where appropriate (Mann *et al.*, 1999). Thus climate reconstructions based entirely on tree-ring data are susceptible to several sources of contamination or non-stationarity of response. For these reasons, investigators have increasingly found tree-ring data most useful when supplemented by other types of proxy information in "multi-proxy" estimates of past temperature change (Overpeck *et al.*, 1997; Jones *et al.*, 1998; Mann *et al.*, 1998; 1999; 2000a; 2000b; Crowley and Lowery, 2000).

Corals

Palaeoclimate reconstructions from corals provide insights into the past variability of the tropical and sub-tropical oceans and atmosphere, prior to the instrumental period, at annual or seasonal resolutions, making them a key addition to terrestrial information. Because of their potential to sample climate variations in ENSO-sensitive regions, a modest network of high-quality coral site records can resolve key large-scale patterns of climate variability (Evans *et al.*, 1998). The corals used for palaeoclimate reconstruction grow throughout the tropics in relatively shallow waters, often living for several centuries. Accurate annual age estimates are possible for most sites using a combination of annual variations in skeletal density and geochemical parameters. Palaeoclimate reconstructions from corals generally rely on geochemical characteristics of the coral skeleton such as temporal variations in trace elements or stable isotopes or, less frequently, on density or variations in fluorescence. Dunbar and Cole (1999) review the use of coral records for palaeoclimatic reconstruction.

Ice cores

Ice cores from polar regions of northern Greenland, Canada and the islands of the North Atlantic and Arctic Oceans, Antarctica, and alpine, tropical and sub-tropical locations (e.g., Thompson, 1996) can provide several climate-related indicators. These indicators include stable isotopes (e.g., ^{18}O), the fraction of melting ice, the rate of accumulation of precipitation, concentrations of various salts and acids, the implied atmospheric loading of dust pollen, and trace gases such as CH_4 and CO_2.

Recently, there has been increased activity in creating high-resolution Antarctic ice core series e.g., for the past millennium (Peel *et al.*, 1996; Mayewski and Goodwin, 1997; Morgan and van Ommen, 1997). In certain regions, isotope information from ice cores shows the late 20th century temperatures as the warmest few decades in the last 1,000 years (Thompson *et al.*, 2000a). Key strengths of ice core information are their high resolution (annual or even seasonal where accumulations rates are particularly high – see van Ommen and Morgan, 1996, 1997), availability in polar and high-elevation regions where other types of proxy climate information like tree-ring data are not available, and their provision of multiple climate- and atmosphere-related variables from the same reasonably well dated physical location (e.g., the GISP2 core; White *et al.*, 1998a). A weakness of ice core data is regional sampling bias (high elevation or high latitude) and melt water and precipitation accumulation data are not easy to date accurately.

The best dated series are based on sub-annual sampling of cores and the counting of seasonal ice layers. Such series may have absolute dating errors as small as a few years in a millennium (Fisher *et al.*, 1996). Dating is sometimes performed using volcanic acid layers with assumed dates (e.g., Clausen *et al.*, 1995) but uncertainties in the volcanic dates can result in dating uncertainties throughout the core (Fisher *et al.*, 1998).

Lake and ocean sediments

Annually laminated (varved) lake sediments offer considerable potential as high-resolution archives of palaeo-environmental conditions where other high-resolution proxy indicators are not available (e.g., arid terrestrial regions), and latitudes poleward of the treeline (Lamoureux and Bradley, 1996; Wohlfarth *et al.*, 1998; Hughen *et al.*, 2000). When annual deposition of the varves can be independently confirmed (e.g., through radiometric dating), they provide seasonal to interannual resolution over centuries to millennia. Varved sediments can be formed from biological processes or from the deposition of inorganic sediments, both of which are often influenced by climate variations. Three primary climate variables may influence lake varves: (a) summer temperature, serving as an index of the energy available to melt the seasonal snowpack, or snow and ice on glaciers; (b) winter snowfall, which governs the volume of discharge capable of mobilising sediments when melting; and (c) rainfall. Laminated lake sediments dominated by (a) can be used for inferences about past high latitude summer temperature changes (e.g., Overpeck *et al.*, 1997), while sediments dominated by the latter two influences can be used to estimate past drought and precipitation patterns (Section 2.5.2.2).

Ocean sediments may also be useful for high-resolution climate reconstructions. In rare examples, annually laminated sediments can be found (e.g., Hughen *et al.*, 1996; Black *et al.*, 1999) and it is possible to incorporate isotope and other information in climate reconstructions, much as varved lake sediments re used. Otherwise, sedimentation rates may sometimes still be sufficiently high that century-scale variability is resolvable (e.g., the Bermuda rise ocean sediment oxygen isotope record of Keigwin, 1996). Dating in such cases, however, must rely on radiometric methods with relatively poor age control.

Borehole measurements

Borehole measurements attempt to relate profiles of temperature with depth to the history of temperature change at the ground surface. The present global database of more than 600 borehole temperature-depth profiles has the densest geographic coverage in North America and Europe, but sparser data are available in other regions (e.g., Australia, Asia, Africa and South America). The depths of the temperature profiles range from about 200 to greater than 1,000 m, allowing palaeo-temperature reconstructions back several hundred to a thousand years. Although large-scale temperature reconstructions have been made to more than a millennium ago (Huang *et al.*, 1997), they show substantial sensitivity to assumptions that are needed to convert the temperature profiles to ground surface temperature changes. Borehole data are probably most useful for climate reconstructions over the last five centuries (Pollack *et al.*, 1998).

Figure 2.19 shows a reconstructed global ground surface temperature history (Pollack *et al.*, 1998; see also Huang *et al.*, 2000) from an average of the 358 individual sites, most located in North America and Eurasia, but some located in Africa, South America and Australia (similar results are obtained by Huang *et al.*, 2000, using an updated network of 616 sites). Superimposed an instrumental estimate of global surface air temperature (Jones and Briffa, 1992). The ensemble of reconstructions shows that the average ground temperature of the Earth has increased by about 0.5°C during the 20th century, and that this was the warmest of the past five centuries. About 80% of the sites experienced a net warming over this period. The estimated mean cumulative ground surface temperature change since 1500 is close to 1.0 ± 0.3°C. Uncertainties due to spatial sampling (see Pollack *et al.*, 1998 and Huang *et al.*, 2000) are also shown. It should be noted that the temporal resolution of the borehole estimates decreases sharply back in time, making it perilous to compare the shape of the trend shown in Figure 2.19 with better-resolved trends determined from higher-resolution climate proxy data discussed below.

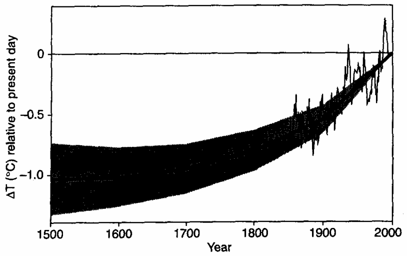

Figure 2.19: Reconstructed global ground temperature estimate from borehole data over the past five centuries, relative to present day. Shaded areas represent ± two standard errors about the mean history (Pollack *et al.*, 1998). Superimposed is a smoothed (five-year running average) of the global surface air temperature instrumental record since 1860 (Jones and Briffa, 1992).

While borehole data provide a direct estimate of ground surface temperatures under certain simplifying assumptions about the geothermal properties of the earth near the borehole, a number of factors complicate their interpretation. Non-temperature-related factors such as land-use changes, natural land cover variations, long-term variations in winter snow cover and soil moisture change the sub-surface thermal properties and weaken the interpretation of the reconstructions as estimates of surface air temperature change. In central England, where seasonal snow cover is not significant, and major land-use changes occurred many centuries ago, borehole ground surface temperature trends do tend to be similar to those in long instrumental records (Jones, 1999). In contrast, Skinner and Majorowicz (1999) show that borehole estimates of ground surface temperature warming during the 20th century in north-western North America are 1 to 2°C greater than in corresponding instrumental estimates of surface air temperature. They suggest that this discrepancy may be due to land-use changes that can enhance warming of the ground surface relative to that of the overlying atmospheric boundary layer (see also Lewis, 1998). Such factors need to be better understood before borehole temperature measurements can be confidently interpreted.

Documentary evidence

Historical documentary data are valuable sources of information about past climate (e.g., Brown and Issar, 1998; Bradley, 1999). However, their use requires great care, as such documents may be biased towards describing only the more extreme events, and are, in certain cases, prone to the use of inconsistent language between different writers and different epochs, and to errors in dating. As for all proxy information, historical documents require careful calibration and verification against modern instrumental data. Two areas particularly strong in historical documents describing climate are Europe and China. In Europe, attempts have been made to extend long climate series back in time using a combination of documentary evidence and fragmentary instrumental records (e.g., Pfister, 1995; Pfister *et al.*, 1998). Additional information about past climate change has also been obtained purely from documentary records in Europe (e.g., Martin-Vide and Barriendos, 1995; Brázdil, 1996; Pfister *et al.*, 1996, 1998, 1999; Pfister and Brázdil, 1999; Rodrigo *et al.*, 1999). In China, regional instrumental temperature series have been extended back over much of the past millennium using documentary data combined with inferences from ice cores and tree rings (Wang *et al.*, 1998a, 1998b; Wang and Gong, 2000).

Mountain glacier moraines

The position of moraines or till left behind by receding glaciers can provide information on the advances (and, less accurately, the retreats) of mountain glaciers. Owing to the complex balance between local changes in melting and ice accumulation, and the effects of topography which influence mountain glaciers (see Section 2.2.5.4), it is difficult to reconstruct regional (as opposed to global) climate changes from the extent of mountain glaciers alone (Oerlemans, 1989). For example, both increased winter precipitation (through greater accumulation) and lower summer temperatures (through decreased melting or "ablation") can lead to more positive glacial mass balances. The inertia of large glaciers dictates that they respond to climate change relatively slowly, with delays of decades or occasionally centuries. For smaller, fast moving glaciers in regions where precipitation and accumulation are moderate, temperature changes are usually the dominant factor influencing mountain glacier masses and lengths. Here glacier moraine evidence in combination with other lines of evidence can provide reliable information on past regional temperature changes (Salinger, 1995; Holzhauser and Zumbühl, 1996; Raper *et al.*, 1996; Salinger *et al.*, 1996).

2.3.2.2 *Multi-proxy synthesis of recent temperature change*

Since the SAR there have been several attempts to combine various types of high-resolution proxy climate indicators to create large-scale palaeoclimate reconstructions that build on earlier work by e.g., Bradley and Jones (1993); Hughes and Diaz (1994) and Mann *et al.* (1995). Overpeck *et al.* (1997) and Fisher (1997) have sought to combine information from ice cores, varved lake sediment cores, and tree rings to reconstruct high latitude climate trends for past centuries. Jones *et al.* (1998) estimated extra-tropical Northern and Southern Hemisphere warm-season temperature changes during the past millennium using a sparse set of extra-tropical warm-season temperature proxy indicators (10 and 8 respectively). Mann *et al.* (1998) reconstructed global patterns of annual surface temperature several centuries back in time. They calibrated a combined terrestrial (tree ring, ice core and historical documentary indicator) and marine (coral) multi-proxy climate network against dominant patterns of 20th century global surface temperature. Averaging the reconstructed temperature patterns over the far more data-rich Northern Hemisphere half of the global domain, they estimated the Northern Hemisphere mean temperature back to AD 1400, a reconstruction which had significant skill in independent cross-validation tests. Self-consistent estimates were also made of the uncertainties. This work has now been extended back to AD 1000 (Figure 2.20, based on Mann *et al.*, 1999). The uncertainties (the shaded region in Figure 2.20) expand considerably in earlier centuries because of the sparse network of proxy data. Taking into account these substantial uncertainties, Mann *et al.* (1999) concluded that the 1990s were likely to have been the warmest decade, and 1998 the warmest year, of the past millennium for at least the Northern Hemisphere. Jones *et al.* (1998) came to a similar conclusion from largely independent data and an entirely independent methodology. Crowley and Lowery (2000) reached the similar conclusion that medieval temperatures were no warmer than mid-20th century temperatures. Borehole data (Pollack *et al.*, 1998) independently support this conclusion for the past 500 years although, as discussed earlier (Section 2.3.2.1), detailed interpretations comparison with long-term trends from such of such data are perilous owing to loss of temporal resolution back in time.

The largely independent multi-proxy Northern Hemisphere temperature reconstructions of Jones *et al.* (1998) and Mann *et al.* (1999) are compared in Figure 2.21, together with an independent (extra-tropical, warm-season) Northern Hemisphere temperature estimate by Briffa (2000) based on tree-ring density data. The estimated uncertainties shown are those for the smoothed Mann *et al.* series. Significant differences between the three reconstructions are evident during the 17th and early 19th centuries where either the Briffa *et al.* or Jones *et al.* series lie outside the estimated uncertainties in the Mann *et al.* series. Much of these differences appear to result from the different latitudinal and seasonal emphases of the temperature estimates. This conclusion is supported by the observation that the Mann *et al.* hemispheric temperature average, when restricted to just the extra-tropical (30 to 70°N band) region of the Northern Hemisphere, shows greater similarity in its trend over the past few centuries to the Jones *et al.* reconstruction. The differences between these reconstructions emphasise the importance of regional and seasonal variations in climate change. These are discussed in the next section.

2.3.3 Was there a "Little Ice Age" and a "Medieval Warm Period"?

The terms "Little Ice Age" and "Medieval Warm Period" have been used to describe two past climate epochs in Europe and neighbouring regions during roughly the 17th to 19th and 11th to 14th centuries, respectively. The timing, however, of these cold and warm periods has recently been demonstrated to vary geographically over the globe in a considerable way (Bradley and

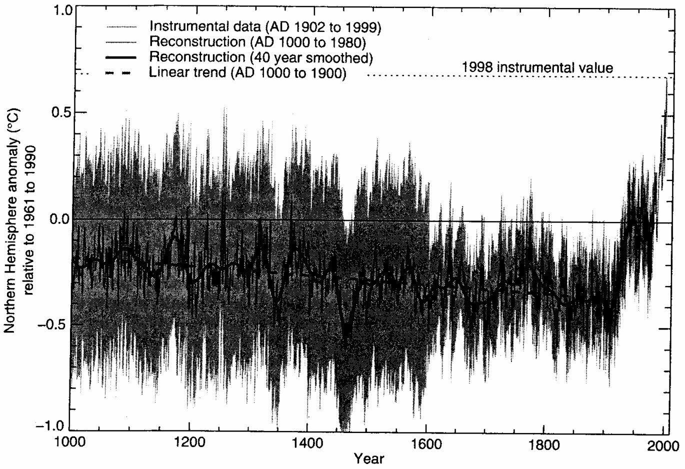

Figure 2.20: Millennial Northern Hemisphere (NH) temperature reconstruction (blue) and instrumental data (red) from AD 1000 to 1999, adapted from Mann *et al.* (1999). Smoother version of NH series (black), linear trend from AD 1000 to 1850 (purple-dashed) and two standard error limits (grey shaded) are shown.

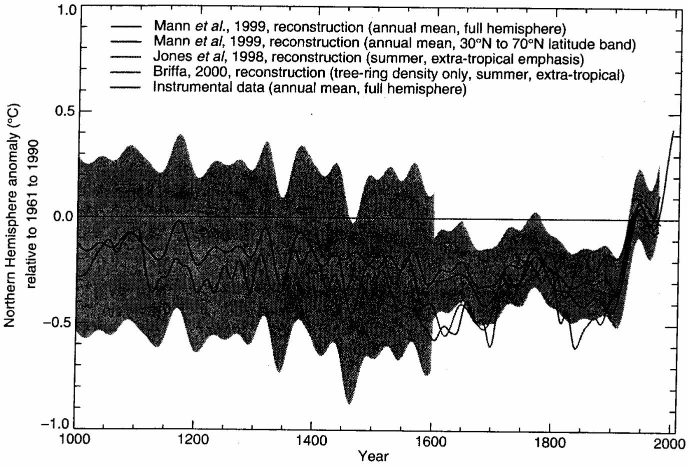

Figure 2.21: Comparison of warm-season (Jones *et al.*, 1998) and annual mean (Mann *et al.*, 1998, 1999) multi-proxy-based and warm season tree-ring-based (Briffa, 2000) millennial Northern Hemisphere temperature reconstructions. The recent instrumental annual mean Northern Hemisphere temperature record to 1999 is shown for comparison. Also shown is an extra-tropical sampling of the Mann *et al.* (1999) temperature pattern reconstructions more directly comparable in its latitudinal sampling to the Jones *et al.* series. The self-consistently estimated two standard error limits (shaded region) for the smoothed Mann *et al.* (1999) series are shown. The horizontal zero line denotes the 1961 to 1990 reference period mean temperature. All series were smoothed with a 40-year Hamming-weights lowpass filter, with boundary constraints imposed by padding the series with its mean values during the first and last 25 years.

Jones, 1993; Hughes and Diaz, 1994; Crowley and Lowery, 2000). Evidence from mountain glaciers does suggest increased glaciation in a number of widely spread regions outside Europe prior to the 20th century, including Alaska, New Zealand and Patagonia (Grove and Switsur, 1994). However, the timing of maximum glacial advances in these regions differs considerably, suggesting that they may represent largely independent regional climate changes, not a globally-synchronous increased glaciation (see Bradley, 1999). Thus current evidence does not support globally synchronous periods of anomalous cold or warmth over this timeframe, and the conventional terms of "Little Ice Age" and "Medieval Warm Period" appear to have limited utility in describing trends in hemispheric or global mean temperature changes in past centuries. With the more widespread proxy data and multi-proxy reconstructions of temperature change now available, the spatial and temporal character of these putative climate epochs can be reassessed.

Mann *et al.* (1998) and Jones *et al.* (1998) support the idea that the 15th to 19th centuries were the coldest of the millennium over the Northern Hemisphere overall. However, viewed hemispherically, the "Little Ice Age" can only be considered as a modest cooling of the Northern Hemisphere during this period of less than 1ºC relative to late 20th century levels (Bradley and Jones, 1993; Jones *et al.*, 1998; Mann *et al.*, 1998; 1999; Crowley and Lowery, 2000). Cold conditions appear, however, to have been considerably more pronounced in particular regions. Such regional variability can be understood in part as reflecting accompanying changes in atmospheric circulation. The "Little Ice Age" appears to have been most clearly expressed in the North Atlantic region as altered patterns of atmospheric circulation (O'Brien *et al.*, 1995). Unusually cold, dry winters in central Europe (e.g., 1 to 2°C below normal during the late 17th century) were very likely to have been associated with more frequent flows of continental air from the north-east (Wanner *et al.*, 1995; Pfister, 1999). Such conditions are consistent (Luterbacher *et al.*, 1999) with the negative or enhanced easterly wind phase of the NAO (Sections 2.2.2.3 and 2.6.5), which implies both warm and cold anomalies over different regions in the North Atlantic sector. Such strong influences on European temperature demonstrate the difficulty in extrapolating the sparse early information about European climate change to the hemispheric, let alone global, scale. While past changes in the NAO have likely had an influence in eastern North America, changes in the El Niño phenomenon (see also Section 2.6), are likely to have had a particularly significant influence on regional temperature patterns over North America.

The hemispherically averaged coldness of the 17th century largely reflected cold conditions in Eurasia, while cold hemispheric conditions in the 19th century were more associated with cold conditions in North America (Jones *et al.*, 1998; Mann *et al.*, 2000b). So, while the coldest decades of the 19th century appear to have been approximately 0.6 to 0.7°C colder than the latter decades of the 20th century in the hemispheric mean (Mann *et al.*, 1998), the coldest decades for the North American continent were closer to 1.5°C colder (Mann *et al.*, 2000b). In addition, the timing of peak coldness was often specific to particular seasons. In Switzerland, for example, the first particularly cold winters appear to have been in the 1560s, with cold springs beginning around 1568, and with 1573 the first unusually cold summer (Pfister, 1995).

The evidence for temperature changes in past centuries in the Southern Hemisphere is quite sparse. What evidence is available at the hemispheric scale for summer (Jones *et al.*, 1998) and annual mean conditions (Mann *et al.*, 2000b) suggests markedly different behaviour from the Northern Hemisphere. The only obvious similarity is the unprecedented warmth of the late 20th century. Speleothem evidence (isotopic evidence from calcite deposition in stalagmites and stalactites) from South Africa indicates anomalously cold conditions only prior to the 19th century, while speleothem (records derived from analysing stalagmites and stalagtites) and glacier evidence from the Southern Alps of New Zealand suggests cold conditions during the mid-17th and mid-19th centuries (Salinger, 1995). Dendroclimatic evidence from nearby Tasmania (Cook *et al.*, 2000) shows no evidence of unusual coldness at these times. Differences in the seasons most represented by this proxy information prevent a more direct comparison.

As with the "Little Ice Age", the posited "Medieval Warm Period" appears to have been less distinct, more moderate in amplitude, and somewhat different in timing at the hemispheric scale than is typically inferred for the conventionally-defined European epoch. The Northern Hemisphere mean temperature estimates of Jones *et al.* (1998), Mann *et al.* (1999), and Crowley and Lowery (2000) show temperatures from the 11th to 14th centuries to be about 0.2°C warmer than those from the 15th to 19th centuries, but rather below mid-20th century temperatures. The long-term hemispheric trend is best described as a modest and irregular cooling from AD 1000 to around 1850 to 1900, followed by an abrupt 20th century warming. Regional evidence is, however, quite variable. Crowley and Lowery (2000) show that western Greenland exhibited anomalous warmth locally only around AD 1000 (and to a lesser extent, around AD 1400), with quite cold conditions during the latter part of the 11th century, while Scandinavian summer temperatures appeared relatively warm only during the 11th and early 12th centuries. Crowley and Lowery (2000) find no evidence for warmth in the tropics. Regional evidence for medieval warmth elsewhere in the Northern Hemisphere is so variable that eastern, yet not western, China appears to have been warm by 20th century standards from the 9th to 13th centuries. The 12th and 14th centuries appear to have been mainly cold in China (Wang *et al.*, 1998a,b; Wang and Gong, 2000). The restricted evidence from the Southern Hemisphere, e.g., the Tasmanian tree-ring temperature reconstruction of Cook *et al.* (1999), shows no evidence for a distinct Medieval Warm Period.

Medieval warmth appears, in large part, to have been restricted to areas in and neighbouring the North Atlantic. This may implicate the role of ocean circulation-related climate variability. The Bermuda rise sediment record of Keigwin (1996) suggests warm medieval conditions and cold 17th to 19th century conditions in the Sargasso Sea of the tropical North Atlantic. A sediment record just south of Newfoundland (Keigwin and Pickart, 1999), in contrast, indicates cold medieval and warm 16th to 19th century upper ocean temperatures. Keigwin and Pickart (1999) suggest that these temperature contrasts were associated with changes in ocean currents in the North Atlantic. They argue that the "Little Ice Age" and "Medieval Warm Period" in the Atlantic region may in large measure reflect century-scale changes in the North Atlantic Oscillation (see Section 2.6). Such regional changes in oceanic and atmospheric processes, which are also relevant to the natural variability of the climate on millennial and longer time-scales (see Section 2.4.2), are greatly diminished or absent in their influence on hemispheric or global mean temperatures.

2.3.4 Volcanic and Solar Effects in the Recent Record

Recent studies comparing reconstructions of surface temperature and natural (solar and volcanic) radiative forcing (e.g., Lean *et al.*, 1995; Crowley and Kim, 1996, 1999; Overpeck *et al.*, 1997; Mann *et al.*, 1998; Damon and Peristykh, 1999; Free and Robock, 1999; Waple *et al.*, 2001) suggest that a combination of solar and volcanic influences have affected large-scale temperature in past centuries. The primary features of the Northern Hemisphere mean annual temperature histories of Mann *et al.* (1999a) and Crowley and Lowery (2000) from AD 1000 to 1900 have been largely reproduced based on experiments using an Energy Balance Model forced by estimates of these natural radiative forcings (Crowley, 2000; Mann, 2000) making the argument that the "Little Ice Age" and "Medieval Warm Period", at the hemispheric mean scale, are consistent with estimates of naturally-forced climate variability. Several studies indicate that the combined effect of these influences has contributed a small component to the warming of the 20th century. Most of these studies isolate greenhouse radiative forcing as being dominant during late 20th century warming (see Crowley, 2000). This argues against a close empirical relationship between certain sun-climate parameters and large-scale temperature that has been claimed for the 20th century (Hoyt and Schatten, 1997). The reader is referred to Chapter 6 for a detailed discussion of these radiative forcings, and to Chapter 12 for comparisons of observed and model simulations of recent climate change.

2.3.5 Summary

Since the SAR there have been considerable advances in our knowledge of temperature change over the last millennium. It is likely that temperatures were relatively warm in the Northern Hemisphere as a whole during the earlier centuries of the millennium, but it is much less likely that a globally-synchronous, well defined interval of "Medieval warmth" existed, comparable to the near global warmth of the late 20th century. Marked warmth seems to have been confined to Europe and regions neighbouring the North Atlantic. Relatively colder hemispheric or global-scale conditions did appear to set in after about AD 1400 and persist through the 19th century, but peak coldness is observed during substantially different epochs in different regions. By contrast, the warming of the 20th century has had a much more convincing global signature (see Figure 2.9). This is consistent with the palaeoclimate evidence that the rate and magnitude of global or hemispheric surface 20th century warming is likely to have been the largest of the millennium, with the 1990s and 1998 likely to have been the warmest decade and year, respectively, in the Northern Hemisphere. Independent estimates of hemispheric and global ground temperature trends over the past five centuries from sub-surface information contained in borehole data confirm the conclusion that late 20th century warmth is anomalous in a long-term context. Decreasing temporal resolution back in time of these estimates and potential complications in inferring surface air temperature trends from sub-surface ground temperature measurements precludes, however, a meaningful direct comparison of the borehole estimates with high-resolution temperature estimates based on other proxy climate data. Because less data are available, less is known about annual averages prior to 1,000 years before the present and for conditions prevailing in most of the Southern Hemisphere prior to 1861.

2.4 How Rapidly did Climate Change in the Distant Past?

2.4.1 Background

Only during the 1980s was the possibility of rapid climatic changes occurring at the time-scale of human life more or less fully recognised, largely due to the Greenland ice core drilled at Dye 3 in Southern Greenland (Dansgaard *et al.*, 1982, 1989). A possible link between such events and the mode of operation of the ocean was then subsequently suggested (Oeschger *et al.*, 1984; Broecker *et al.*, 1985; see Broecker, 1997, for a recent review). The SAR reviewed the evidence of such changes since the peak of the last inter-glacial period about 120 ky BP (thousands of years Before Present). It concluded that: (1) large and rapid climatic changes occurred during the last Ice Age and during the transition towards the present Holocene; (2) temperatures were far less variable during this latter period; and (3) suggestions that rapid changes may have also occurred during the last inter-glacial required confirmation.

These changes are now best documented from ice core, deep-sea sediment and continental records. Complementary and generally discontinuous information comes from coral and lake level data. The time-scale for the Pleistocene deep-sea core record is based on the orbitally tuned oxygen isotope record from marine sediments (Martinson *et al.*, 1987), constrained by two radiometrically dated horizons, the peak of the last inter-glacial (about 124 ky BP) and the Brunhes/Matuyama reversal of the Earth's magnetic field at about 780 ky BP. ^{14}C-dating is also used in the upper 50 ky BP; the result is a deep-sea core chronology believed to be accurate to within a few per cent for the last million years. ^{14}C-dating is also used for dating continental records as well as the counting of annual layers in tree rings and varved lake records, whereas ice-core chronologies are obtained by combining layer counting, glaciological models and comparison with other dated records. The use of globally representative records, such as changes in continental ice volume recorded in the isotopic composition of deep-sea sediments, or changes in atmospheric composition recorded in air bubbles trapped in ice cores, now allow such local records to be put into a global perspective. Studies still largely focus on the more recent glacial-interglacial cycle (the last 120 to 130 ky). Table 2.4 is a guide to terminology.

TAB 9

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Surface Temperature Reconstructions for the Last 2,000 Years

Committee on Surface Temperature Reconstructions for the Last 2,000 Years
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Division on Earth and Life Studies

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International Standard Book Number X-XXX-XXXXX-X (Book)

Additional copies of this report are available from the National Academies Press, 500 Fifth Street, N.W, Lockbox 285, Washington, D.C. 20055; (800) 624-6242 or (202)334-3313 (in the Washington metropolitan area); Internet, http://www.nap.edu.

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Foreword

Our understanding of climate and how it has varied over time are advancing rapidly as new data are acquired and new investigative instruments and methods are employed. Thus in 2005, I suggested to the U. S. Congress that the National Research Council (NRC) could help to answer questions about the data and methods that have been used in constructing records of Earth's surface temperatures from times when there were no scientific instruments, using proxy indicators. How has temperature varied over the last 2,000 years? How certain is the answer to this question?

Subsequently, this study was requested by Representative Sherwood Boehlert, Chairman of the Committee on Science, U.S. House of Representatives. Chairman Boehlert asked for a clear and concise report in a relatively short period of time, and the NRC agreed to undertake the study quickly. An *ad hoc* committee was formed, with the group carefully composed to include the breadth and depth of expertise and perspectives needed to analyze all aspects of how surface temperatures are estimated and interpreted, and to comment generally on climate science. The NRC asked the committee to summarize current scientific information on the temperature record for the past two millennia, describe the main areas of uncertainty and how significant they are, describe the principal methodologies used and any problems with these approaches, and explain how central is the debate over the paleoclimate temperature record to the state of scientific knowledge on global climate change.

The committee has prepared a report that, in my view, provides policy makers and the scientific community with a critical view of surface temperature reconstructions and how they are evolving over time, as well as a good sense of how important our understanding of the paleoclimate temperature record is within the overall state of scientific knowledge on global climate change. The report does not make policy recommendations.

I thank the members of the committee, who worked intensely to produce this careful report in a short period of time and contributed much personal time, insight, and energy. The NRC staff, and all those who contributed papers, data, graphics, and other information, as well as the independent experts who participated in the rigorous review process, were essential participants.

Ralph J. Cicerone, President
National Academy of Sciences
Chair, National Research Council

Preface

This committee was asked to describe and assess the state of scientific efforts to reconstruct surface temperature records for the Earth over approximately the last 2,000 years. (The full Statement of Task appears in Appendix A.) Normally, a technical issue such as surface temperature reconstructions might not generate widespread attention, but this case brings interesting lessons about how science works and how science, especially climate science, is communicated to policy makers and the public. The debate began in 1998 when a paper by Michael Mann, Raymond Bradley and Malcolm Hughes was published in the journal Nature. The authors used a new methodology to combine data from a number of sources to estimate temperatures in the Northern Hemisphere for the last six centuries, and later for the last 1,000 years. This research received wide attention, in part because it was illustrated with a simple graphic, the so-called hockey stick curve, that many interpreted as definitive evidence of anthropogenic causes of recent climate change. The research was given prominence in the 2001 report of the Intergovernmental Panel on Climate Change, and then picked up by many in the wider science community and by the popular media.

Science is a process of exploration of ideas—hypotheses are proposed and research is conducted to investigate. Other scientists work on the issue, producing supporting or negating evidence, and each hypothesis either survives for another round, evolves into other ideas, or is proven false and rejected. In the case of the hockey stick, the scientific process has proceeded for the last few years with many researchers testing and debating the results. Critics of the original papers have argued that the statistical methods were flawed, that the choice of data was biased, and that the data and procedures used were not shared so others could verify the work. This report is an opportunity to examine the strengths and limitations of surface temperature reconstructions and the role that they play in improving our understanding of climate. The reconstruction produced by Dr. Mann and his colleagues was just one step in a long process of research, and it is not (as sometimes presented) a clinching argument for anthropogenic global warming, but rather one of many independent lines of research on global climate change.

Using multiple types of proxy data to infer temperature time series over large geographic regions is a relatively new area of scientific research, although it builds upon the considerable progress that has been made in deducing past temperature variations at single sites and local regions. Surface temperature reconstructions often combine data from a number of specialized disciplines, and few individuals have expertise in all aspects of the work. The procedures for dealing with these data are evolving—there is no one "right" way to proceed. It is my opinion that this field is progressing in a healthy manner. As in all scientific endeavors, research reported in the scientific literature is often "work in progress" aimed at other investigators, not always to be taken as individual calls for action in the policy community.

With this as context, the committee considered the voluminous literature pertinent to its charge and received briefings and written contributions from more than two dozen people. We have organized our report knowing that we have at least two different audiences—the science community and the policy community. The principal conclusions of the committee are listed in the Summary and explained in the Overview using nontechnical language. More extensive technical justifications for the committee's conclusions, including references, are presented in the chapters that follow.

Finally, let me thank the members of the Committee on Surface Temperature Reconstructions for the Last 2,000 Years. The committee worked tirelessly over the last few months to assess the status of this field of research so that the public can see exactly what is involved, what we currently know about it, and what the prospects are for improving our understanding. We have tried to make clear how this piece of the climate puzzle fits into the broader discussions about global climate change.

Gerald R. North, *Chair*
Committee on Surface Temperature Reconstructions for the Last 2,000 Years

Acknowledgments

This report has been reviewed in draft form by individuals chosen for their diverse perspectives and technical expertise, in accordance with procedures approved by the NRC's Report Review Committee. The purpose of this independent review is to provide candid and critical comments that will assist the institution in making its published report as sound as possible and to ensure that the report meets institutional standards for objectivity, evidence, and responsiveness to the study charge. The review comments and draft manuscript remain confidential to protect the integrity of the deliberative process. We wish to thank the following individuals for their review of this report:

Peter Huybers, Woods Hole Oceanographic Institution
Carl Wunsch, Massachusetts Institute of Technology
Connie Woodhouse, National Oceanic and Atmospheric Administration
Julia Cole, University of Arizona
Lonnie Thompson, The Ohio State University
David Chapman, University of Utah
Ricardo Garcia-Herrera, Universidad Complutense de Madrid
David Brillinger, University of California, Berkeley
Robert Stine, University of Pennsylvania
Alexander Flax, Independent consultant
Claus Frohlich, PMOD Technologies
Richard Muller, Lawrence Berkeley Laboratory
Thomas Crowley, Duke University

Although the reviewers listed above have provided many constructive comments and suggestions, they were not asked to endorse the conclusions or recommendations nor did they see the final draft of the report before its release. The review of this report was overseen by Andrew R. Solow, Woods Hole Oceanographic Institution, and Louis J. Lanzerotti, New Jersey Institute of Technology. Appointed by the National Research Council, they were responsible for making certain that an independent examination of this report was carried out in accordance with institutional procedures and that all review comments were carefully considered. Responsibility for the final content of this report rests entirely with the authoring committee and the institution.

Table of Contents

SUMMARY 1

OVERVIEW 5

1 INTRODUCTION TO TECHNICAL CHAPTERS 25
Concepts and Definitions
Attribution of Global Warming to Human Influences
Report Structure

2 THE INSTRUMENTAL RECORD 29
Instrumental Data
Features of the Instrumental Record
Uncertainties and Errors Associated with the Instrumental Record
Spatial Sampling Issues

3 DOCUMENTARY AND HISTORICAL EVIDENCE 37
Types of Evidence
Limitations and Benefits of Historical and Documentary Sources
Systematic Climate Reconstructions Derived from Historical Archives
Consequences of Climate Change for Past Societies

4 TREE RINGS 44
Definition and Premises
Field and Laboratory Methods
Temperature Reconstructions

5 MARINE, LAKE, AND CAVE PROXIES 51
Corals
Marine Sediments
Lake and Peat Sediments
Speleothems
Summary

6 ICE ISOTOPES 62
Physical Basis for Deriving Climate Signals from Ice Isotopic Ratio Records
Calibration and Resolution
Results from Ice Isotopic Ratio Records

7 GLACIER LENGTH AND MASS BALANCE RECORDS 68
Reconstructing Temperature Records from Glacier Records

More Detailed Background on Glacier-Length-Based Reconstructions
Other Information Available from Glaciers

8 BOREHOLES 74
Boreholes in Rock and Permafrost
Limits on Borehole-Based Reconstructions
Boreholes in Glacial Ice

9 STATISTICAL BACKGROUND 79
Linear Regression and Proxy Reconstruction
Principal Component Regression
Validation and the Prediction Skill of the Proxy Reconstruction
Quantifying the Full Uncertainty of a Reconstruction

10 CLIMATE FORCINGS AND CLIMATE MODELS 94
Climate Forcings
Climate Model Simulations
Anthropogenic Forcing and Recent Climate Change

11 LARGE-SCALE MULTIPROXY RECONSTRUCTION TECHNIQUES 104
Evolution of Multiproxy Reconstruction Techniques
Strengths and Limitations of Large-Scale Surface Temperature Reconstructions
Overall Findings and Conclusions
What Comments Can Be Made on the Value of Exchanging Information and Data?
What Might Be Done to Improve Our Understanding of Climate Variations Over the Last 2,000 Years?

REFERENCES 114

APPENDIXES

A Statement of Task 135
B R Code for Figure 9-2 136
C Biographical Sketches of Committee Members 138

Summary

Because widespread, reliable instrumental records are available only for the last 150 years or so, scientists estimate climatic conditions in the more distant past by analyzing *proxy evidence* from sources such as tree rings, corals, ocean and lake sediments, cave deposits, ice cores, boreholes, glaciers, and documentary evidence. For example, records of Alpine glacier length, some of which are derived from paintings and other documentary sources, have been used to *reconstruct* the time series of surface temperature variations in south-central Europe for the last several centuries. Studying past climates can help us put the 20th century warming into a broader context, better understand the climate system, and improve projections of future climate.

Starting in the late 1990s, scientists began combining proxy evidence from many different locations in an effort to estimate surface temperature changes averaged over broad geographic regions during the last few hundred to few thousand years. These *large-scale surface temperature reconstructions* have enabled researchers to estimate past temperature variations over the Northern Hemisphere or even the entire globe, often with time resolution as fine as decades or even individual years. This research, and especially the first of these reconstructions published in 1998 and 1999 by Michael Mann, Raymond Bradley, and Malcolm Hughes, attracted considerable attention because the authors concluded that the Northern Hemisphere was warmer during the late 20th century than at any other time during the past millennium. Controversy arose because many people interpreted this result as definitive evidence of anthropogenic causes of recent climate change, while others criticized the methodologies and data that were used.

In response to a request from Congress, this committee was assembled by the National Research Council to describe and assess the state of scientific efforts to reconstruct surface temperature records for the Earth over approximately the last 2,000 years and the implications of these efforts for our understanding of global climate change.

Figure S-1 shows a compilation of large-scale surface temperature reconstructions from different research groups, each using its own methodology and selection of proxies, as well as the instrumental record (beginning in 1856) of global mean surface temperature.

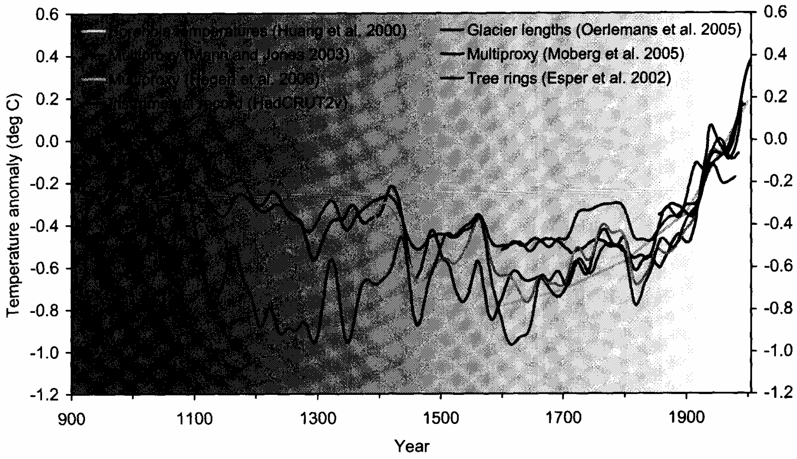


FIGURE S-1 Smoothed reconstructions of large-scale (Northern Hemisphere mean or global mean) surface temperature variations from six different research teams are shown along with the instrumental record of global mean surface temperature. Each curve portrays a somewhat different history of temperature variations, and is subject to a somewhat different set of uncertainties that generally increase going backward in time (as indicated by the gray shading). This set of reconstructions conveys a qualitatively consistent picture of temperature changes over the last 1,100 years, and especially the last 400. See Figure O-5 for details about each curve.

After considering all of the available evidence, including the curves shown in Figure S-1, the committee has reached the following conclusions:

- The instrumentally measured warming of about 0.6°C during the 20th century is also reflected in borehole temperature measurements, the retreat of glaciers, and other observational evidence, and can be simulated with climate models.
- Large-scale surface temperature reconstructions yield a generally consistent picture of temperature trends during the preceding millennium, including relatively warm conditions centered around A.D. 1000 (identified by some as the "Medieval Warm Period") and a relatively cold period (or "Little Ice Age") centered around 1700. The existence and extent of a Little Ice Age from roughly 1500 to 1850 is supported by a wide variety of evidence including ice cores, tree rings, borehole temperatures, glacier length records, and historical documents. Evidence for regional warmth during medieval times can be found in a diverse but more limited set of records including ice cores, tree rings, marine sediments, and historical sources from Europe and Asia, but the exact timing and duration of warm periods may have varied from region to region, and the magnitude and geographic extent of the warmth are uncertain.

- It can be said with a high level of confidence that global mean surface temperature was higher during the last few decades of the 20th century than during any comparable period during the preceding four centuries. This statement is justified by the consistency of the evidence from a wide variety of geographically diverse proxies.
- Less confidence can be placed in large-scale surface temperature reconstructions for the period from A.D. 900 to 1600. Presently available proxy evidence indicates that temperatures at many, but not all, individual locations were higher during the past 25 years than during any period of comparable length since A.D. 900. The uncertainties associated with reconstructing hemispheric mean or global mean temperatures from these data increase substantially backward in time through this period and are not yet fully quantified.
- Very little confidence can be assigned to statements concerning the hemispheric mean or global mean surface temperature prior to about A.D. 900 because of sparse data coverage and because the uncertainties associated with proxy data and the methods used to analyze and combine them are larger than during more recent time periods.

The main reason that our confidence in large-scale surface temperature reconstructions is lower before A.D. 1600 and especially before A.D. 900 is the relative scarcity of precisely dated proxy evidence. Other factors limiting our confidence in surface temperature reconstructions include the relatively short length of the instrumental record (which is used to calibrate and validate the reconstructions); the fact that all proxies are influenced by a variety of climate variables; the possibility that the relationship between proxy data and local surface temperatures may have varied over time; the lack of agreement as to which methods are most appropriate for calibrating and validating large-scale reconstructions and for selecting the proxy data to include; and the difficulties associated with constructing a global or hemispheric mean temperature estimate using data from a limited number of sites and with varying chronological precision. All of these considerations introduce uncertainties that are difficult to quantify.

Despite these limitations, the committee finds that efforts to reconstruct temperature histories for broad geographic regions using multiproxy methods are an important contribution to climate research and that these large-scale surface temperature reconstructions contain meaningful climatic signals. The individual proxy series used to create these reconstructions generally exhibit strong correlations with local environmental conditions, and in most cases there is a physical, chemical, or physiological reason why the proxy reflects local temperature variations. Our confidence in the results of these reconstructions becomes stronger when multiple independent lines of evidence point to the same general result, as in the case of the Little Ice Age cooling and the 20th century warming.

The basic conclusion of Mann et al. (1998, 1999) was that the late 20th century warmth in the Northern Hemisphere was unprecedented during at least the last 1,000 years. This conclusion has subsequently been supported by an array of evidence that includes both additional large-scale surface temperature reconstructions and pronounced changes in a variety of local proxy indicators, such as melting on icecaps and the retreat of glaciers around the world, which in many cases appear to be unprecedented during at least the last 2,000 years. Not all individual proxy records indicate that the recent warmth is unprecedented, although a larger fraction of geographically diverse sites experienced exceptional warmth during the late 20th century than during any other extended period from A.D. 900 onward.

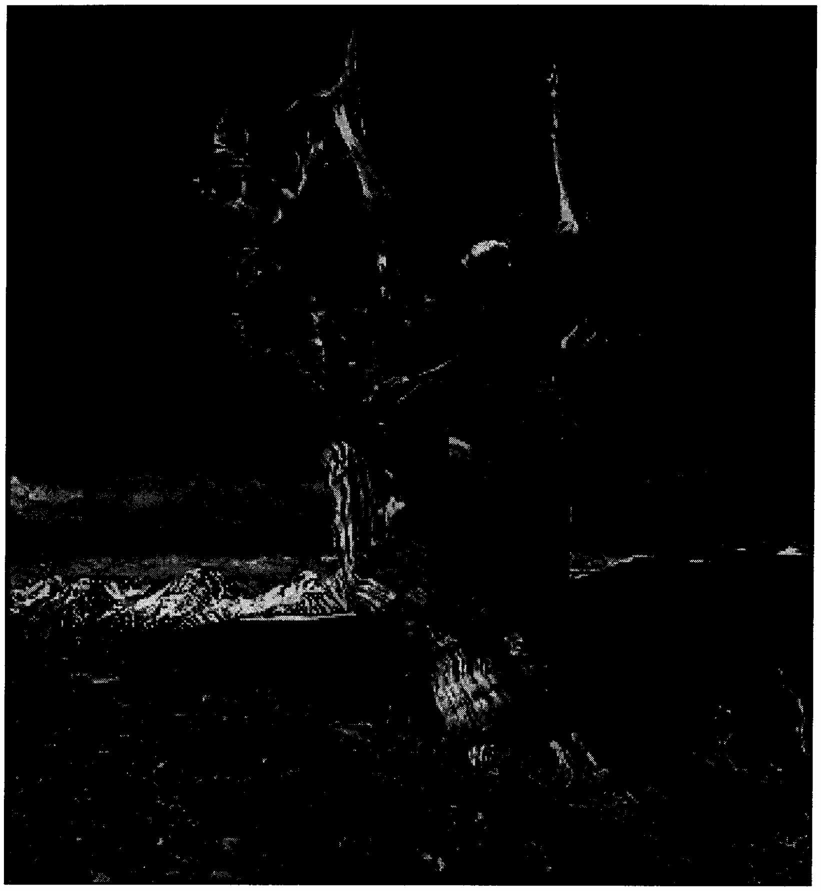
Based on the analyses presented in the original papers by Mann et al. and this newer supporting evidence, the committee finds it plausible that the Northern Hemisphere was warmer during the last few decades of the 20th century than during any comparable period over the preceding millennium. The substantial uncertainties currently present in the quantitative assessment of large-scale surface temperature changes prior to about A.D. 1600 lower our confidence in this conclusion compared to the high level of confidence we place in the Little Ice Age cooling and 20th century warming. Even less confidence can be placed in the original conclusions by Mann et al. (1999) that "the 1990s are likely the warmest decade, and 1998 the warmest year, in at least a millennium" because the uncertainties inherent in temperature reconstructions for individual years and decades are larger than those for longer time periods, and because not all of the available proxies record temperature information on such short timescales.

Surface temperature reconstructions for periods prior to the industrial era are only one of multiple lines of evidence supporting the conclusion that climatic warming is occurring in response to human activities, and they are not the primary evidence.

Surface temperature reconstructions also provide a useful source of information about the variability and sensitivity of the climate system. To within existing uncertainties, climate model simulations show that the estimated temperature variations during the two millennia prior to the Industrial Revolution can be explained plausibly by estimated variations in solar radiation and volcanic activity during the same period.

Large-scale surface temperature reconstructions have the potential to further improve our knowledge of temperature variations over the last 2,000 years, particularly if additional proxy evidence can be identified and obtained from areas where the coverage is relatively sparse and for time periods before A.D. 1600 and especially before A.D. 900. Furthermore, it would be helpful to update proxy records that were collected decades ago, in order to develop more reliable calibrations with the instrumental record. Improving access to data used in publications would also increase confidence in the results of large-scale surface temperature reconstructions both inside and outside the scientific community. New analytical methods, or more careful use of existing ones, may also help circumvent some of the existing limitations associated with surface temperature reconstructions based on multiple proxies. Finally, because some of the most important potential consequences of climate change are linked to changes in regional circulation patterns, hurricane activity, and the frequency and intensity of droughts and floods, regional and large-scale reconstructions of changes in other climatic variables, such as precipitation, over the last 2,000 years would provide a valuable complement to those made for temperature.

TAB 10



A REPORT TO CHAIRMAN BARTON, HOUSE COMMITTEE ON ENERGY AND COMMERCE AND TO CHAIRMAN WHITFIELD, HOUSE SUBCOMMITTEE ON OVERSIGHT AND INVESTIGATIONS: PALEOCLIMATE RECONSTRUCTION[1]

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[1] This image is a photograph of a now deceased Bristlecone Pine, a species that is prominent among the proxy series used for paleoclimate reconstruction.

AD HOC COMMITTEE REPORT ON THE 'HOCKEY STICK' GLOBAL CLIMATE RECONSTRUCTION[2]

EXECUTIVE SUMMARY

The Chairman of the Committee on Energy and Commerce as well as the Chairman of the Subcommittee on Oversight and Investigations have been interested in an independent verification of the critiques of Mann et al. (1998, 1999) [MBH98, MBH99] by McIntyre and McKitrick (2003, 2005a, 2005b) [MM03, MM05a, MM05b] as well as the related implications in the assessment. The conclusions from MBH98, MBH99 were featured in the Intergovernmental Panel on Climate Change report entitled *Climate Change 2001*[3]*: The Scientific Basis*. This report concerns the rise in global temperatures, specifically during the 1990s. The MBH98 and MBH99 papers are focused on paleoclimate temperature reconstruction and conclusions therein focus on what appear to be a rapid rise in global temperature during the 1990s when compared with temperatures of the previous millennium. These conclusions generated a highly polarized debate over the policy implications of MBH98, MBH99 for the nature of global climate change, and whether or not anthropogenic actions are the source. This committee, composed of Edward J. Wegman (George Mason University), David W. Scott (Rice University), and Yasmin H. Said (The Johns Hopkins University), has reviewed the work of both articles, as well as a network of journal articles that are related either by authors or subject matter, and has come to several conclusions and recommendations. This Ad Hoc Committee has worked pro bono, has received no compensation, and has no financial interest in the outcome of the report.

Global Climate Reconstruction Methodology

MBH98, MBH99 use several indicators to measure global climate change. Primarily, these include historical records, tree rings, ice cores, and coral reefs. The width and density of tree rings vary with climatic conditions (sunlight, precipitation, temperature, humidity, and carbon dioxide and nitrogen oxides availability), soil conditions, tree species, tree age, and stored carbohydrates in the trees. However, tree ring density is useful in paleoclimatic temperature reconstructions because in mature trees, tree rings vary approximately linearly with age. The width and density of tree rings are dependent on many confounding factors, making it difficult to isolate the climatic temperature signal. It is usually the case that width and density of tree rings are monitored in conjunction in order to more accurately use them as climate proxies.

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[2] This report was authored by Edward J. Wegman, George Mason University, David W. Scott, Rice University, and Yasmin H. Said, The Johns Hopkins University. We would also like to acknowledge the contributions of John T. Rigsby, III, Naval Surface Warfare Center, and Denise M. Reeves, MITRE Corporation.

[3] The IPCC report *Climate Change 2001: Third Assessment Report* consists of four sub-reports: 1) *Climate Change 2001: The Scientific Basis*, 2) *Climate Change 2001: Impacts, Adaptation, and Vulnerabilities*, 3) *Climate Change 2001: Mitigation*, and 4) *Climate Change 2001: Synthesis Report.*

Ice cores are the accumulation of snow and ice over many years that have recrystallized and have trapped air bubbles from previous time periods. The composition of these ice cores, especially the presence of hydrogen and oxygen isotopes, provides a picture of the climate at the time. Because isotopes of water vapor exhibit a lower vapor pressure, when the temperature falls, the heavier water molecules will condense faster than the normal water molecules. The relative concentrations of the heavier isotopes in the condensate indicate the temperature of condensation at the time, allowing for ice cores to be used in global temperature reconstruction. In addition to the isotope concentration, the air bubbles trapped in the ice cores allow for measurement of the atmospheric concentrations of trace gases, including greenhouse gases carbon dioxide, methane, and nitrous oxide. The air bubbles may also contain traces of aerosols, which are produced in great concentrations during volcanic eruptions.

Coral is similar to trees in that the growth and density of the coral is dependent upon temperature. X-rays of coral cross sections show the relative density and growth over time. High density layers of coral are produced during years of high ocean surface temperatures. Hence, corals can be calibrated to estimate sea surface temperatures.

Principal Component Analysis and the CFR and CPS Methodology

Principal component analysis is a method often used for reducing multidimensional datasets to lower dimensions for analysis. In this context, dimensions refer to the number of distinct variables. The time series proxy data involved are transformed into their principal components, where the first principal component is intended to explain most of the variation present in the variables. Each subsequent principal component explains less and less of the variation. In the methodology of MBH98/99, the first principal component is used in the temperature reconstruction, and also has the highest explained variance. This method is intended for dimension reduction. In most datasets, the first principal component should be the least smooth (because of the higher variance). However, in MBH98, MBH99, the proxy data are incorrectly centered, which inflates the variance of certain proxies and selectively chooses those decentered proxies as the temperature reconstruction.

There are several time series models that exist for the purpose of modeling series with dependence, including autoregressive, moving averages, autoregressive moving average models, and long memory processes. MBH98 and MBH99 focus on simple signal plus superimposed noise models for paleoclimate temperature reconstruction. Because of complex feedback mechanisms involved in climate dynamics, it is unlikely that the temperature records and the data derived from the proxies can be adequately modeled with a simple temperature signal with superimposed noise. We believe that there has not been a serious investigation to model the underlying process structures nor to model the present instrumented temperature record with sophisticated process models.

Two principal methods for temperature reconstructions have been used; CFR[4] (climate field construction) and CPS (climate-plus-scale). The CFR is essentially a principal component analysis and the CPS is a simple averaging of climate proxies, which are then scaled to actual temperature records. The controversy of Mann's methods lies in that the proxies are centered on the mean of the period 1902-1995, rather than on the whole time period. This mean is, thus, actually decentered low, which will cause it to exhibit a larger variance, giving it preference for being selected as the first principal component. The net effect of this decentering using the proxy data in MBH98 and MBH99 is to produce a "hockey stick" shape. Centering the mean is a critical factor in using the principal component methodology properly. It is not clear that Mann and associates realized the error in their methodology at the time of publication. Because of the lack of full documentation of their data and computer code, we have not been able to reproduce their research. We did, however, successfully recapture similar results to those of MM. This recreation supports the critique of the MBH98 methods, as the offset of the mean value creates an artificially large deviation from the desired mean value of zero.

Findings

In general, we found MBH98 and MBH99 to be somewhat obscure and incomplete and the criticisms of MM03/05a/05b to be valid and compelling. We also comment that they were attempting to draw attention to the discrepancies in MBH98 and MBH99, and not to do paleoclimatic temperature reconstruction. Normally, one would try to select a calibration dataset that is representative of the entire dataset. The 1902-1995 data is not fully appropriate for calibration and leads to a misuse in principal component analysis. However, the reasons for setting 1902-1995 as the calibration point presented in the narrative of MBH98 sounds reasonable, and the error may be easily overlooked by someone not trained in statistical methodology. We note that there is no evidence that Dr. Mann or any of the other authors in paleoclimatology studies have had significant interactions with mainstream statisticians.

In our further exploration of the social network of authorships in temperature reconstruction, we found that at least 43 authors have direct ties to Dr. Mann by virtue of coauthored papers with him. Our findings from this analysis suggest that authors in the area of paleoclimate studies are closely connected and thus 'independent studies' may not be as independent as they might appear on the surface. This committee does not believe that web logs are an appropriate forum for the scientific debate on this issue.

It is important to note the isolation of the paleoclimate community; even though they rely heavily on statistical methods they do not seem to be interacting with the statistical community. Additionally, we judge that the sharing of research materials, data and results was haphazardly and grudgingly done. In this case we judge that there was too much reliance on peer review, which was not necessarily independent. Moreover, the work has been sufficiently politicized that this community can hardly reassess their public positions without losing credibility. Overall, our committee believes that Mann's assessments that the decade of the 1990s was the hottest decade of the millennium and that 1998 was the hottest year of the millennium cannot be supported by his analysis.

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[4] The CFR methodology is essentially the methodology used in the MBH98/99 papers, but the terminology was not used until later.

Recommendations

Recommendation 1. Especially when massive amounts of public monies and human lives are at stake, academic work should have a more intense level of scrutiny and review. It is especially the case that authors of policy-related documents like the IPCC report, *Climate Change 2001: The Scientific Basis*, should not be the same people as those that constructed the academic papers.

Recommendation 2. We believe that federally funded research agencies should develop a more comprehensive and concise policy on disclosure. All of us writing this report have been federally funded. Our experience with funding agencies has been that they do not in general articulate clear guidelines to the investigators as to what must be disclosed. Federally funded work including code should be made available to other researchers upon reasonable request, especially if the intellectual property has no commercial value. Some consideration should be granted to data collectors to have exclusive use of their data for one or two years, prior to publication. But data collected under federal support should be made publicly available. (As federal agencies such as NASA do routinely.)

Recommendation 3. With clinical trials for drugs and devices to be approved for human use by the FDA, review and consultation with statisticians is expected. Indeed, it is standard practice to include statisticians in the application-for-approval process. We judge this to be a good policy when public health and also when substantial amounts of monies are involved, for example, when there are major policy decisions to be made based on statistical assessments. In such cases, evaluation by statisticians should be standard practice. This evaluation phase should be a mandatory part of all grant applications and funded accordingly.

Recommendation 4. Emphasis should be placed on the Federal funding of research related to fundamental understanding of the mechanisms of climate change. Funding should focus on interdisciplinary teams and avoid narrowly focused discipline research.

1. INTRODUCTION

Global warming is an issue that has gathered much public, legislative, national and international attention. Uncertainty about magnitude and consequences of global warming has caused considerable friction among governments and among their citizens. The Intergovernmental Panel on Climate Change (IPCC) report (Intergovernmental Panel on Climate Change, 2001) entitled, *Climate Change 2001: Third Assessment Report*, featured alarming statistics concerning the rapid rise in global temperatures during the decade of the 1990s and suggested that this rapid rise was due principally to anthropogenicly generated greenhouse gas emissions, specifically carbon dioxide. This document was taken to be a strong justification for the Kyoto Accord. Featured prominently in the IPCC report was the work of Dr. Michael Mann, Dr. Raymond Bradley, and Dr. Malcolm Hughes (Mann et al., 1998, 1999) [MBH98, MBH99]. These papers featured temperature reconstructions going back as far as 1000 years. The methodology found in Mann et al. employed a statistical technique known as principal components analysis (PCA). Challenges to the way in which PCA was used have arisen from McIntyre and McKitrick (2003, 2005a, 2005b) [MM03, MM05a, MM05b]. The challenges are based on rather subtle mathematical nuances. The discussion and evaluation of the use of PCA to some extent has degenerated in to the battle of competing web blogs: http://www.climateaudit.org, http://www.climate2003.org, and http://www.realclimate.org.

The Chairman of the House Committee on Energy and Commerce along with Chairman of the Subcommittee of Oversight and Investigations have been interested in discovering whether or not the criticisms of Mann et al. are valid and if so, what are the implications. To this end, Committee staff asked for advice as to the validity of the complaints of McIntyre and McKitrick [MM] and related implications. Dr. Wegman formed an ad hoc Committee (Drs. Edward J. Wegman – George Mason University, David W. Scott – Rice University, and Yasmin H. Said – The Johns Hopkins University). The Committee was organized with our own initiative as a pro bono committee.

We have attempted to address several broad issues. We have sought to reproduce the results of MM in order to determine whether their criticisms are valid and have merit. We will also comment on whether issues raised by those criticisms discussed in McIntyre and McKitrick (2005a, 2005b) raise broader questions concerning the assessment of Mann et al. (1998, 1999) in peer review and the IPCC and whether such science assessments involving work of a statistical nature require some type of strengthening to provide reliable guidance for policy makers.

Prior to the work of our committee and independently of our committee, Chairman Barton and Chairman Whitfield wrote letters to Drs. Michael Mann, Raymond Bradley, and Malcolm Hughes as well as to the Intergovernmental Panel on Climate Change and the National Science Foundation. All three of the authors responded, but as lead author Dr. Mann's responses were most extensive. Dr. Mann's responses had something of a confrontational tone. No member of our Committee participated in the design or structure of the questions to Dr. Mann. However, based on his responses and the extensive literature we have reviewed, we will also attempt to address some of our findings explicitly to issues raised by the questions to Dr. Mann and his responses. The specific questions of Chairman Barton and Chairman Whitfield are listed below.

Requests to Drs. Mann, Bradley and Hughes.

Provide:

1. Your *curriculum vitae*, including, but not limited to, a list of all studies relating to climate change research for which you were an author or co-author and the source of funding for these studies.

2. List of all financial support you have received related to your research, including, but not limited to, all private, state, and federal assistance, grants, contracts (including subgrants or subcontracts), or other financial awards or honoraria.

3. Regarding all such work involving federal grants or funding support under which you were a recipient of funding or principal investigator, provide all agreements relating to the underlying grants or funding, including, but not limited to, any provisions, adjustments, or exceptions made in the agreements relating to the dissemination and sharing of research results.

4. Provide the location of all data archives relating to each published study for which you were an author or co-author and indicate: (a) whether this information contains all the specific data you used and calculations you performed, including such supporting documentation as computer source code, validation information, and other ancillary information, necessary for full evaluation and application of the data, particularly for another party to replicate your research results; (b) when this information was available to researchers; (c) where and when you first identified the location of this information; (d) what modifications, if any, you have made to this information since publication of the respective study; and (e) if necessary information is not fully available, provide a detailed narrative description of the steps somebody must take to acquire the necessary information to replicate your study results or assess the quality of the proxy data you used.

5. According to the *Wall Street Journal*, you have declined to release the exact computer code you used to generate your results. (a) Is this correct? (b) What policy on sharing research and methods do you follow? (c) What is the source of this policy? (d) Provide this exact computer code used to generate your results.

6. Regarding study data and related information that is not publicly archived, what requests have you or your co-authors received for data relating to the climate change studies, what was your response, and why?

7. The authors McIntyre and McKitrick (*Energy & Environment*, Vol. 16, No. 1, 2005) report a number of errors and omissions in Mann *et al.*, 1998. Provide a

detailed narrative explanation of these alleged errors and how these may affect the underlying conclusions of the work, including, but not limited to answers to the following questions:

a. Did you run calculations without the bristlecone pine series referenced in the article and, if so, what was the result?
b. Did you or your co-authors calculate temperature reconstructions using the referenced "archived Gaspe tree ring data," and what were the results?
c. Did you calculate the R2 statistic for the temperature reconstruction, particularly for the 15th Century proxy record calculations and what were the results?
d. What validation statistics did you calculate for the reconstruction prior to 1820, and what were the results?
e. How did you choose particular proxies and proxy series?

8. Explain in detail your work for and on behalf of the Intergovernmental Panel on Climate Change, including, but not limited to: (a) your role in the Third Assessment Report [TAR]; (b) the process for review of studies and other information, including the dates of key meetings, upon which you worked during the TAR writing and review process; (c) the steps taken by you, reviewers, and lead authors to ensure the data underlying the studies forming a basis for key findings of the report were sound and accurate; (d) requests you received for revisions to your written contributions; and (e) the identity of the people who wrote and reviewed the historical temperature-record portions of the report, particularly Section 2.3, "Is the Recent Warming Unusual?"

2. BACKGROUND

2.1 Background on Paleoclimate Temperature Reconstruction

Paleoclimatology focuses on climate, principally temperature, prior to the era when instrumentation was available to measure climate artifacts. Many natural phenomena are climate dependent and, where records are still available, these phenomena may be used as proxies to extract a temperature signal. Of course the proxy signals are extremely noisy and thus temperature reconstruction becomes more problematic as one attempts reconstructions further back in time. Climate is not solely a matter of (global) temperature, although concerns with the effects of global warming focus primarily on temperature reconstructions. As just suggested, temperature reconstruction is based on proxy signals contained in the historical records of climate dependent natural phenomena. Table 1 based on Bradley (1999) illustrates the wide variety of these natural phenomena that may be used as proxies. Some proxies measure very low frequency (slowly varying) climatic variables and thus are not useful for measuring average annual temperature changes. Table 2 found in Bradley (1999), which was reproduced from Bradley and Eddy (1991) summarizes a variety of proxies and also indicates their minimum sampling interval as well as the range of years for which they could reasonably be used for temperature reconstruction. The high frequency proxies that could be used on an annual basis include tree rings, ice cores, and corals. In addition to serving as temperature proxies, these measurements are proxies for other climatic variables including, for example, precipitation, chemical composition of air and water, and solar activity.

1. **Glaciological (ice cores)**
    a. Geochemistry (ions and isotopes of oxygen and hydrogen)
    b. Gas content in air bubbles
    c. Trace element and microparticle concentrations
    d. Physical properties (e.g., ice fabric)
2. **Geological**
    a. Marine (ocean sediment cores)
        i. Biogenic sediments
            1. oxygen isotopic composition
            2. faunal and floral abundance
            3. morphological variations
            4. alkenones (from diatoms)
        ii. Inorganic sediments
            1. terrestrial (aeolian) dust and ice-rafted debris
            2. clay mineralogy
    b. Terrestrial
        i. Glacial deposits and features of glacial erosion
        ii. Preglacial features
        iii. Shorelines (Eustatic and glacio-eustatic features)
        iv. Aeolian deposits (loess and sand dunes)
        v. Lacustrine sediments and erosionial features (shorelines)
        vi. Pedological features
        vii. Speleothrems (age and stable isotope composition)
3. **Biological**
    a. Tree rings (width, density, stable isotope composition)
    b. Pollen (type, relative abundance, an/or absolute concentration)
    c. Plant macrofossils (age and distribution)
    d. Insects (assemblage characteristics)
    e. Corals (geochemistry)
    f. Diatoms, ostracods, and other biota in lake sediments (assemblages, abundance, and/or geochemistry)
    g. Modern population distribution (refugia and relict populations in plants and animals)
4. **Historical**
    a. Written records of environmental indicators (parameteorlogical phenomena)
    b. Phonological records

Table 1: Principal Sources of Proxy Data for Paleoclimatic Reconstructions

After Bradley (1999)

Archive	Minimum Sampling Interval	Temporal Range (order:yr)	Potential Information Derived
Historical records	day/hr	$\sim10^3$	T, P, B, V, M, L, S
Tree rings	yr/season	$\sim10^4$	T, P, B, V, M, S, C_A
Lake sediments	yr to 20 yr	$\sim10^4$-10^6	T, B, M, P, V, C_W
Corals	yr	$\sim10^4$	C_W, L, T, P
Ice cores	yr	$\sim5 \times 10^4$	T, P, C_A, B, V, M, S
Pollen	20 yr	$\sim10^5$	T, P, B
Speleothems	100 yr	$\sim5 \times 10^5$	C_W, T, P
Paleosols	100 yr	$\sim10^6$	T, P, B
Loess	100 yr	$\sim10^6$	P, B, M
Geomorphic features	100 yr	$\sim10^6$	T, P, V, L, P
Marine sediments	500 yr	$\sim10^7$	T, C_W, B, M, L, P

Table 2: Characteristics of Natural Archives

T = temperature
P = precipitation, humidity, water balance
C = chemical composition of air (C_A) or water (C_W)
B = information on biomass and vegetation patterns
V = volcanic eruptions
M = geomagnetic field variations
L = sea level
S = solar activity

After Bradley and Eddy (1991)

Tree Rings – A cross section of a temperate forest tree shows variation of lighter and darker bands that are usually continuous around the circumference of the tree. These bands are the so-called tree rings and are due to seasonal effects. Each tree ring is composed of large thin-walled cells called early wood and smaller more densely packed thick walled cells called late wood. The average width of a tree ring is a function of many variables including the tree species, tree age, stored carbohydrates in the tree, nutrients in the soil, and climatic factors including sunlight, precipitation, temperature, wind speed, humidity, and even carbon dioxide availability in the atmosphere. Obviously there are many confounding factors so the problem is to extract the temperature signal and to distinguish the temperature signal from the noise caused by the many confounding factors. Temperature information is usually derived from interannual variations in the ring width as well as interannual and intra-annual density variations. Density variations are valuable in paleoclimatic temperature reconstructions because they have a relatively simple growth function that, in mature trees, is approximately linear with age. The density variations have been shown empirically to contain a strong climatic temperature signal. Two values of density are measured within each growth ring: minimum density representing early wood and maximum density representing late wood. Maximum density values are strongly correlated with April to August mean temperatures in trees across the boreal forest from Alaska to Labrador, Schweingruber et al., (1993). Both tree ring width and density data are used in combination to extract the maximal climatic temperature signal.

Climate signal is strongest in trees that are under stress. Trees growing in sites where climate does not limit growth tend to produce rings that are uniform. Trees that are growing close to their extreme ecological range are greatly influenced by climate. Climate variations strongly influence annual growth increments. Two types of stress are commonly recognized, moisture stress and temperature stress. Trees growing in semiarid regions are limited by water availability and thus variations in ring width reflect this climatic moisture signal. Trees growing near to their ecological limits either in terms of latitude or altitude show growth limitations imposed by temperature and thus ring width variations in such trees contain a relatively strong temperature signal. However, the biological processes are extremely complex so that very different combinations of climatic conditions may cause similar ring width increments. Tree growth and carbohydrate production by a tree in one year will precondition the tree for strong growth in the subsequent year so that there is a strong autocorrelation in the ring width time series. Photosynthetic processes are accelerated with the increased availability of carbon dioxide in the atmosphere and, hence, it is conjectured that ring growth would also be correlated with atmospheric carbon dioxide; see Graybill and Idso (1993). In addition, oxides of nitrogen are formed in internal combustion engines that can be deposited as nitrates also contributing to fertilization of plant materials. It is clear that while there are temperature signals in the tree rings, the temperature signals are confounded with many other factors including fertilization effects due to use of fossil fuels.

Wider rings are frequently produced during the early life of a tree. Thus the tree rings frequently contain a low frequency signal that is unrelated to climate or, at least, confounded with climatic effects such as temperature. In order to use tree rings as a temperature signal successfully, this low frequency component must be removed. This is typically done by a nonlinear parametric trend fit using a polynomial or modified exponential curve. Because the early history of tree rings confounds climatic signal with low frequency specimen specific signal, tree rings are not usually effective for accurately determining low frequency, longer-term effects. Once there is reasonable confidence that the tree ring signal reflects a temperature signal, and then a calibration is performed using the derived tree ring data and instrumented temperature data. The assumption in this inference is that when tree ring structure observed during the instrumented period that is similar to tree ring structure observed in the past, both will have correspondingly similar temperature profiles. As pointed out earlier, many different sets of climatic conditions can and do yield similar tree ring profiles. Thus tree ring proxy data alone is not sufficient to determine past climate variables. See Bradley (1999) for a discussion of the fitting and calibration process for dendritic-based temperature reconstruction.

Ice Cores – The accumulated past snowfall in the polar caps and ice sheets provide a very useful record for paleoclimate reconstruction. We shall refer to ice cores in this section even though strictly speaking there is a combination of snow and ice. Somewhat compressed old snow is called a firn. The transition from snow to firn to ice occurs as the weight of overlying material causes the snow crystals to compress, deform and recrystalize in more compact form. As firn is buried beneath subsequent snowfalls, density is increased as air spaces are compressed due to mechanical packing as well as plastic deformation. Interconnected air passages may then be sealed and appear as individual air bubbles. At this point the firn becomes ice. Paleoclimatic information derived from ice cores is obtained from four principal mechanisms: 1) analysis of stable isotopes of water and atmospheric oxygen; 2) analysis of other gases in the air bubbles in the ice; 3) analysis of dissolved and particulate matter in the firn and ice; and 4) analysis of other physical properties such as thickness of the firn and ice.

The mechanism by which stable isotopes of oxygen and hydrogen carry a temperature signal is as follows. An oxygen atom can exist in three stable forms with atomic weights of 16, 17 or 18. Oxygen with an atomic weight of 16 makes up 99.76% of all oxygen atoms. Similarly, hydrogen can exist in two stable forms with atomic weights of one or two, the latter being called deuterium. Hydrogen with atomic weight of one comprises 99.984% of all hydrogen. Thus water molecules can exist in several heavy forms when compared with normal water, which is made up of two atomic-weight-1 hydrogen atoms and one atomic-weight-16 oxygen atom. The vapor pressure of normal water is higher than the heavier forms of water with evaporation resulting in a vapor that is relatively speaking poor in the heavier forms of water. Conversely, the remaining water will be enriched in water containing the heavier isotopes. When condensation occurs, the lower vapor pressure of water containing the heavier isotopes will cause that water to condense more rapidly than normal water. The greater the fall in temperature, the more condensation will occur; hence, the water vapor will exhibit less heavy isotope concentration when compared to the original (sea) water. Thus the relative isotope concentrations in the condensate will be a direct indicator of the temperature at which condensation occurred.

In addition to the relative heavy/light isotope ratios, the trapped bubbles in ice cores provide a record of atmospheric concentrations of trace gases including greenhouse gases such as carbon dioxide, methane and nitrous oxide. In addition the ice cores contain record of aerosols and dust content resulting from volcanic eruptions and other changes in particulate content in the atmosphere. The relative atmospheric concentrations of greenhouse gases as well as aerosol and particulate content coupled with other climate information gives insight into both the importance of these as drivers of temperature as well as how these drivers might couple in either a positive or negative feedback sense.

Corals – The term "coral" refers to the biological order *Scleractinia*, which have hard calcium-based skeletons supporting softer tissues. An important subgroup for paleoclimate studies is the reef-building corals in which the coral polyp lives symbiotically with single-celled algae. These algae produce carbohydrates by means of photosynthesis and are affected by water depth, water turbidity, and cloudiness. Much of the carbohydrates diffuse away from the algae providing food to the coral polyp, which in turn provide a protective environment for the algae. Reef-building corals are strongly affected by temperature and, as temperature drops, the rate of calcification drops with lower temperature potentially presaging the death of the colony. Coral growth rates vary over a year and can be sectioned and x-rayed to reveal high- and low-density bands. High density layers are produced during times of higher sea surface temperatures. Thus not unlike tree rings, data on corals also can be calibrated to estimate (sea) surface temperatures.

2.2 Background on Principal Components

Principal Components

Principal Component Analysis (PCA) is a method for reducing the dimension of a high dimensional data set while preserving most of the information in those data. Dimension is here taken to mean the number of distinct variables (proxies). In the context of paleoclimatology, the proxy variables are the high dimensional data set consisting of several time series that are intended to carry the temperature signal. The proxy data set in general will have a large number of interrelated or correlated variables. Principal component analysis tries to reduce the dimensionality of this data set while also trying to explain the variation present as much as possible. To achieve this, the original set of variables is transformed into a new set of variables, called the principal components (PC) that are uncorrelated and arranged in the order of decreasing "explained variance." It is hoped that the first several PCs explain most of the variation that was present in the many original variables. The idea is that if most of the variation is explained by the first several principal components, then the remaining principal components may be ignored for all practical purposes and the dimension of the data set is effectively reduced.

Noise, White and Red

In a statistical signal processing sense, noise is defined to be unknown external factors or fluctuations in signals. Noise is typically modeled as what are called random innovations, random meaning not predictable. Signals of interest may be actual audio signals, signals in the electromagnetic spectrum, or a known function to which simple statistical noise has been added. In the paleoclimatology context, noise is the unpredictable portion of the climate signal caused by random variations in the factors related to tree ring growth, ice core development, or coral growth. Noise is often thought of in terms of periodic variation. There are many types of noise with varying frequencies, each classified by a color. The color names for these different types of sounds are derived from an analogy between the spectrum of frequencies and the equivalent spectrum of light wave frequencies. That is, if the pattern of 'blue noise' were translated into light waves, the resulting light would be blue.

White noise, has equal power density across the entire frequency spectrum, that is, it has constant energy at all frequencies. When this is graphically represented, white noise has a flat power spectral density. In a practical example, white noise is what is used to refer to that steady, even soothing sound produced when tuning in to an unused radio or TV frequency. White noise has an equal amount of energy *per frequency band* in contrast to pink noise, which has an equal amount of energy per octave. Pink noise has a frequency spectrum that is flat in logarithmic space. The power density of pink noise, compared with white noise, decreases by 3 dB (decibels) per octave. It is said that pink noise is the most soothing sound to the human ear. Pink noise has the same frequency distribution as falling rain.

Red noise is similar to pink noise, but it has relatively more energy at lower frequencies than pink noise. Red noise has a power density that decreases 6 dB per octave as the frequency increases. Of course, red noise was named after a connection with red light, which is on the low end of the visible light spectrum. Mathematically speaking, integrating white noise produces red noise. Red noise in the paleoclimatology context comes from the fact that tree rings have correlation from year to year, that is, if a tree grows well in a given year, it will store carbohydrates and will tend to have a good year of growth the following year as well. Red noise in the paleoclimatology context is modeled by a first-order autoregressive model.

Autoregressive, Moving Average and ARMA Models

Autoregressive, moving averages, and ARMA models are statistical time series models. An autoregressive model of order p means that the present value of the time series depends only on the p most recent past values of the time series. The dependence is taken to be linear. If $p = 1$, then we say that the process is a first order autoregressive process as indicated for the red noise model above. A moving average process of order q is formed by taking a weighted average of q uncorrelated white noise terms, that is, zero mean constant variance terms. The moving average means that the next value of the moving average process drops off the oldest term from the average and adds a new term. Autoregressive moving average (ARMA) models, sometimes called Box-Jenkins models, are also used to model time series data. These models are used for understanding and predicting future values in the series. There are two parts to the ARMA model, an autoregressive (AR) part and a moving average (MA) part.

Gaussian Noise and Long Memory Processes

Although we have not specified the probability distribution of the random innovations, it is often the case that a normal or Gaussian probability distribution is appropriate to model noise or what we have called random innovations. The basic paleoclimatology model is taken to be a trend with superimposed white or red noise usually with a Gaussian distribution. The Gaussian distribution assumption is a convenient one mathematically. Random (or stochastic) processes whose autocorrelation function, decaying as a power law, sums to infinity are known as long range correlations or long range dependent processes. Because the decay is slow, as opposed to exponential decay, these processes are said to have long memory. Applications exhibiting long-range dependence include Ethernet traffic, financial time series, geophysical time series such as variation in temperature, and amplitude and frequency variation in EEG signals. Fractional Brownian motion is a self-similar Gaussian process with long memory. The Box-Jenkins ARMA models described in the previous section are all short-term memory processes.

In reality, temperature records and hence data derived from proxies are not modeled accurately by a trend with superimposed noise that is either red or white. There are complex feedback mechanisms and nonlinear effects that almost certainly cannot be modeled in any detail by a simple trend plus noise. These underlying process structures appear to have not been seriously investigated in the paleoclimate temperature reconstruction literature. Cohn and Lin (2005) make the case that much of natural time series, in their case hydrological time series, might be modeled more accurately by a long memory process. Long memory processes are stationary processes, but the corresponding time series often make extended sojourns away from the stationary mean value and, hence, mimic trends such as the perceived hockey stick phenomena.

One type of such long memory processes is a process driven by fractional Gaussian noise (fractional Brownian motion). An object with self-similarity is exactly or approximately similar to a part of itself. For example, many coastlines in the real world are self-similar since parts of them show the same properties at many scales. Self-similarity is a common property of many fractals, as is the case with fractional Brownian motion. A serious effort to model even the present instrumented temperature record with sophisticated process models does not appear to have taken place.

2.3 Background on Social Networks

Networks, Relations and Structure

A social network is a mathematical structure made of nodes, which are generally taken to represent individuals or organizations. A network graph illustrates how the nodes are connected. Social network analysis (also called *network theory*) has emerged as a key technique and a topic of study in modern sociology, anthropology, social psychology and organizational theory. Research has demonstrated that social networks, operating on many levels, from families up to the level of nations, play a critical role in determining the way problems are solved, organizations are run, and the degree to which individuals succeed in achieving their goals. The shape of the social network helps determine a network's usefulness to its individuals. Smaller, tighter networks can be less useful to their members than networks with lots of loose connections (weak ties) to individuals outside the main network. More "open" networks, with many weak ties and social connections, are more likely to introduce new ideas and opportunities to their members than closed networks with many redundant ties. In other words, a group of friends who only do things with each other already share the same knowledge and opportunities. Yet a group of individuals with connections to other social worlds is likely to have access to a wider range of information. It is better for individual success to have connections to a variety of networks rather than many connections within a single network. Similarly, individuals can exercise influence or act as brokers within their social networks by bridging two networks that are not directly linked (called filling social holes).

Networks operate anywhere that energy and information are exchanged: between neurons and cells, computers and people, genes and proteins, atoms and atoms, and people and people. Social theories are built on more than just metaphors. Social network analysis assumes that interpersonal ties matter, whether they exist among individuals, organizations or countries. Interpersonal connections matter because they are conduits for the transmission of information, goods, behavior and attitudes. Ties and connections form networks, which can be analyzed. The main goal of social network analysis is the detection and interpretation of patterns of social ties among people, nations, or organizations involved in social relationships.

There are several key concepts at the heart of network analysis. We outline these concepts next and then define a social network.

Actor: Social network analysis is concerned with understanding the linkages among social entities and the implications of these linkages. The social entities are referred to as actors. Actors do not necessarily have the desire or the ability to act. Most social network applications consider a collection of actors that are all of the same type. These are known as one-mode networks.

Relational Tie: Social ties link actors to one another. The range and type of social ties can be quite extensive. A tie establishes a linkage between a pair of actors. Examples of ties include the evaluation of one person by another (such as expressed friendship, liking, respect), transfer of material resources (such as business transactions, lending or borrowing things), association or affiliation (such as jointly attending the same social event or belonging to the same social club), behavioral interaction (talking together, sending messages), movement between places or statues (migration, social or physical mobility), physical connection (a road, river, bridge connecting two points), formal relations such as authority and biological relationships such as kinship or descent.

Dyad: A linkage or relationship establishes a tie at the most basic level between a pair of actors. The tie is an inherent property of the pair. Many kinds of network analysis are concerned with understanding ties among pairs and are based on the dyad as the unit of analysis.

Triad: The analysis of a subset of three actors (a triad) and the possible ties among them is motivated and informed by balance theory. Balance theory asks whether or not a triad is transitive or balanced. A transitive triad is characterized by transitive relations such as if actor *i likes* actor *j*, and actor *j likes* actor *k*, then actor *i* also *likes* actor *k*. A balanced triad means that if actors *i* and *j* like each other, then *i* and *j* should have similar evaluations of a third actor, whereas if they dislike each other then they are expected to differ in their evaluations.

Subgroup: Dyads are pairs of actors and associated ties, triads are triples of actors and associated ties. We can define a subgroup of actors as any subset among actors with associated ties. Locating and studying these subgroups using specific criteria is one of the primary objectives of social network analysis.

Group: Network analysis is not only concerned with collections of dyads, triads, or subgroups. Social network analysis has the ability to model the relationships among systems of actors. A group is a collection of actors on which ties are measured.

Relation: The collection of ties of a specific kind among members of a group is called a relation, for example, the set of friendships among pairs of children in a classroom or the set of formal diplomatic ties maintained by pairs of nations in the world. A relation refers to the collection of ties of a given kind measured on pairs of actors from a specified actor set.

Social Network: We are now in a position to define a social network. A social network consists of a finite set or sets of actors and the relation or relations defined on them. The presence of relational information is a significant feature of a social network.

Computational Facets of Social Network Analysis

The main goal of social network analysis is the detection and interpretation of patterns of social ties among actors. Social network analysis may be viewed as a broadening or generalization of standard data analytic techniques and applied statistics that focus on observational units and their characteristics. Complex network data sets may contain information about the characteristics of the actors (such as the gender of people in a group or the GNP of nations of the world) as well as structural variables. Network problems naturally give rise to graphs. The structural and compositional variables necessary for social network analysis often result in complicated data sets that must be modeled with sophisticated graph theoretic, algebraic and statistical methods. The underlying mathematical frameworks used to build social network models are called graphs. A graph is a discrete structure consisting of vertices (nodes) and edges (links), where the vertices correspond to the objects, and the edges to the relations of the structure to be modeled.

A network consists of a graph and additional information on the vertices or lines of the graphs. Names of people or businesses or countries represent additional information on vertices. Line values are numbers for arcs and edges that indicate the strength of relationships between actors. This flexible definition allows a wide variety of empirical phenomena to be modeled as networks.

Properties of vertices are used to find and interpret patterns of ties in a network. Social networks are often complicated and may be large. Partitions are used to reduce a network so that different facets can be studied.

- Partitions – A partition of a network is a classification or clustering of the vertices in the network so that each vertex is assigned to exactly one class or cluster. Partitions may specify some property that depends on attributes of the vertices. Partitions divide the vertices of a network into a number of mutually exclusive subsets. That is, a partition splits a network into parts. We can produce a local view defined by a selected class of vertices that consists of all of the structural ties between nodes in the selected class of vertices. Partitions are also sometimes called blocks or blockmodels. These are essentially a way to cluster actors together in groups that behave in a similar way.

- Allegiance – Allegiance measures the support that an actor provides for the structure of his block. An actor supports his block by having internal block edges. A measure of this is the total number of edges that an actor has internal to his block. An actor supports his block by not having external edges from the block to other actors or blocks. A measure of this is the total number of possible external edges minus the total number of existing external edges. The allegiance for a block is a weighted sum of a measure of internal allegiance and a measure of external allegiance. The overall allegiance for a social network is the sum of the allegiances for the individual blocks. If the overall allegiance is positive then a good partition was made. The partitioning continues recursively until a new partition no longer contributes to a positive allegiance.

- Global View – We may want a global view of a network that allows us to study relationships among classes.

- Cohesion – Solidarity, shared norms, identity, collective behavior, and social cohesion are considered to emerge from social relations. The first concern of social analysis is to investigate who is related and who is not. The general hypothesis assumes that people who match on social characteristics will interact more often and people who interact regularly will foster a common attitude or identity. Social networks usually contain dense pockets of people who stick together. They are called cohesive subgroups and usually more than interaction joins the people involved. People who interact intensively are likely to consider themselves as a social group. This phenomenon is known as homophily: "birds of a feather flock together". There are several techniques that detect cohesive subgroups in social networks. All of these techniques are based on the ways in which the vertices are interconnected. These techniques are used to investigate whether a cohesive group represents an emergent or established social group. Social cohesion is used to describe structural concepts of density and connectedness. Density refers to the number of links between vertices. A network is strongly connected if it contains paths between all of its vertices and is weakly connected when semi-paths connect all of its vertices. Connected networks and networks with high average degree are thought to be more cohesive. There are several techniques to detect cohesive subgroups based on density and connectedness.

- Affiliations – Membership in an organization or participation in an event is a source of social ties. An affiliation is a relationship between people and an organization. Affiliations are often institutional or structural and tend to be less personal as they result from private choices to a lesser degree than sentiments and friendship.

- Brokerage – Social relations can be considered to be channels that transport information, services, or goods between people or organizations. From a bird's eye view, social structure helps to explain how information, goods or even attitudes and behavior diffuses within a social system. Network analysis reveals social structure and helps to trace the routes that goods and information may follow. Some social structures permit rapid diffusion of information, whereas others contain sections that are difficult to reach. We can also focus on the position of specific people or organizations within the network. In general, being well connected is advantageous. Contacts are necessary to have access to information and help. The number and intensity of a person's ties are called his or her sociability or social capital. Social capital is known to correlate positively to age and education in Western societies. Some people occupy central or strategic positions within the system of channels and are crucial for the transmission process. Some positions may exert pressure on their occupants, but they also yield power and profit. The direction of ties is not very important in social network structures that capture the exchange of information.

- Centrality – This is one of the oldest concepts in network analysis. Most social networks contain people or organizations that are central. Because of their position, they have better access to information, and better opportunity to spread information. This is known as the ego-centered-approach to centrality. The network is centralized from socio-centered perspective. The notion of centrality refers to the positions of individual vertices within the network, while centralization is used to characterize an entire network. A network is highly centralized if there is a clear boundary between the center and the periphery. In a highly centralized network, information spreads easily, but the center is indispensable for the transmission of information.

There are several ways to measure the centrality of vertices and the centralization of networks. The concepts of vertex centrality and network centralization are best understood by considering undirected communication networks. If social relations are channels that transmit information between people, central people are those people who have access to information circulating in the network or who may control the circulation of information.

The accessibility of information is linked to the concept of distance. If you are closer to the other people in the network, the paths that information has to follow to reach you are shorter, so it is easier for you to acquire information. If we take into account direct neighbors only, the number of neighbors (the degree of a vertex in a simple undirected network) is a simple measure of centrality. If we also want to consider other indirect contacts, we use closeness centrality, which measures our distance to all other vertices in the network. The closeness centrality of a vertex is higher if the total distance to all other vertices is shorter.

The importance of a vertex to the circulation of information is captured by the concept of betweenness centrality. From this perspective, a person is central if he or she is a link in more information chains between other people in the network. High betweenness centrality indicates that a person is an important intermediary in the communication network. Information chains are represented by geodesics and the betweenness centrality of a vertex is simply the proportion of geodesics between other pairs of vertices that include the vertex. The centralization of a network is higher if it contains very central vertices as well as very peripheral vertices.

3. LITERATURE REVIEW OF GLOBAL CLIMATE CHANGE RESEARCH

Michael Mann's Dissertation and Related Work

In his 1998 dissertation, Michael Mann used instrumental data and multi-proxy datasets to observe climate variability over the past few centuries. He also used a simplified coupled ocean-atmosphere model to describe mechanisms that may contribute to the climate variability. In his dissertation, Dr. Mann described a 70 to 100 year oscillation in the climate signal formed by the proxy and instrumental data. He notes that this century-scale variation in the climate involves a combination of meridional overturning (the circulation of cold water in the ocean) and gyre-scale circulation.

After being awarded his doctorate, Dr. Mann, together with his colleagues Dr. Bradley and Dr. Hughes, continued multi-proxy reconstruction research with his 1998 paper, *Global-Scale Temperature Patterns and Climate Forcing over the Past Six Centuries*, [MBH98]. In this paper, he attempts to use PCA to find an eigenbasis (a new coordinate system where the axes are the eigenvectors, or principal components that represent the significant relationships in the data) for the multi-proxy data series for the period 1610-1995. He also uses a multivariate regression method to observe possible forcing agents, or contributors to warming. Dr. Mann uses linear relationships between possible forcing agents (greenhouse gases, solar irradiance and volcanic aerosols) and climate in previous studies by R.S. Bradley and T.J. Crowley as a basis for regression analysis. He reports that the results are a large spike in greenhouse gas forcing in the 20th century. Additionally, he notes that 1995 and 1997 were likely the hottest years since 1400 AD within a 99.7% level of certainty.

In 1999, Dr. Mann and colleagues supplemented MBH98 by a new paper, *Northern Hemisphere Temperatures during the Past Millennium: Inferences, Uncertainties, and Limitations* MBH99. In this work they used similar methods to reconstruct temperatures further back to the beginning of the millennium. Although uncertainties are magnified with each previous year of reconstruction, their results suggested that 20th century warming counters a millennial-scale cooling trend and that the 1990s was likely the hottest decade in the millennium, with moderate certainty.

McIntyre and McKitrick

After MBH99, Stephen McIntyre and Ross McKitrick [MM03] published their critique of the 1998 paper, citing calculation errors, unjustified truncation or extrapolation of source data, obsolete data, geographical location errors and incorrect calculation of principal components. They also claimed that using the MBH98 methodology and the Northern Hemisphere average temperature index for the period 1400-1980 shows that temperatures in the 15th century exceeded those of the late 20th century. In particular, they claim that MBH98's incorrect usage of PCA alone resulted in the well-known "hockey stick" shape.

In a 2004 corrigendum, Mann et al. replied to these criticisms, contending that McIntyre and McKitrick's finding resulted from an elimination of over 70% of the 15th century proxy data used by MBH98. They also assert that MM03's results fail independent cross-validation tests. In subsequent response papers, MM05a and MM05b noted that the data eliminated in their earlier critique of MBH98 was restricted to two proxy series, the Gaspe cedar ring-width series, and the first principal component from the North American tree ring network in MBH98. In the case of the first principal component, McIntyre and McKitrick stated that the mean was not centered correctly over the period of analysis, 1400-1980. Instead of subtracting the mean of each data series between the years 1400 and 1980, they subtracted the mean of 1902-1980. McIntyre and McKitrick state that this decentering of the mean causes the explained variance of certain proxies to be inflated, namely the proxy series that causes the hockey stick shape. Subsequently, that particular proxy series is chosen as the principal component, indicating it is the most significant correlation in the data. With regard to the Gaspe cedar tree ring series, McIntyre and McKitrick state that Mann 1998 did not use archived data, but rather made an extrapolation in which they misrepresented the start date of the series. They also state that this extrapolation depresses the early 15th century results. Lastly, they note that the underlying dataset up to 1421 is based only on one tree, and only on two trees up to 1447.

Mann and Rutherford's 2005 paper in turn responded to these new criticisms, stating that McIntyre and McKitrick's misunderstanding of their methodology and use of an incorrect version of the proxy dataset is the source of the discrepancy in their results. They argue that the Mann et al. 1998 implementation calculates PC series of proxy networks over progressively longer intervals, which allows for the use of the maximum amount of data. For example, if there were 50 proxy series, and only 10 date back to AD 1400, then calculating one set of PC would eliminate 40 of the 50 series available back to AD 1600. By using two different intervals, 1400-1980 and 1600-1980 in this example, all proxy series can be utilized. Mann et al. contend that this misunderstanding is what led to the elimination of data prior to 1500 AD and is also what gives rise to the warmth in the 15th century of McIntyre and McKitrick's version of MBH98.

To address the extrapolation critique, Mann et al. terminated the 1971 calibration period in which they filled in missing proxy values in the multi-proxy PC network between 1971 and 1980. They also approached the reconstruction using a different method, regularized expectation maximization (REGEM), and yielded the same results. They then conclude that their reconstruction is robust and reproducible, based on their use of an independent Climate Field Reconstruction method (the REGEM method) and their use of individual proxies instead of the multi-proxy PC representation used in Mann et al.1998.

Other Notable Works

While Mann et al. have focused much of their work on high frequency proxies, or those proxies that provide data on climate variability on a decadal or even yearly scale, Jan Esper and colleagues have investigated the effect of using low-frequency proxies that preserve data on climate variability on a centennial scale in their paper *Low-Frequency Signals in Long Tree-Ring Chronologies for Reconstructing Past Temperature Variability*. Esper et al. contend that preserving multi-centennial variability in tree-ring records is critical in comparing the temperatures of the Medieval Warming Period

(MWP) and those of the 20th century. By carefully selecting tree-ring chronologies from fourteen sites in the Northern Hemisphere (NH) extratropics, Esper et al. produced a reconstruction that preserved the multi-centennial variation, as well as supported the large-scale occurrence of the MWP over the NH extratropics. Using the regional curve standardization (RCS) method for their chronologies, Esper et al. found that there were significant multi-centennial differences between Mann et al. 1998 and their reconstruction. These differences may be explained by the fact that Mann et al.'s analysis includes data from the tropical and subtropical Northern Hemisphere whereas Esper's analysis includes only the extra tropic region.

In their 2005 paper *Highly Variable Northern Hemisphere temperatures reconstructed from low- and high-resolution proxy data*, Moberg et al. further studied the use of low-resolution proxy data in order to preserve multi-centennial variability in climate reconstructions. Specifically, they focused on lake and ocean sediment cores, which tend to have a lower time resolution, but provide climate information at multi-centennial timescales that may not be captured by tree-ring data. Moberg et al. reconstructed Northern Hemisphere temperatures for the past 2,000 years by combining low-resolution proxies with tree-ring data. Their reconstruction shows a larger multi-centennial variability than most previous multi-proxy reconstructions. Furthermore, their reconstruction depicted high temperatures that are comparable to the 20th century temperatures in the period AD 1000-1100. Their results also suggest a natural trend in multi-centennial variability that is likely to continue.

Following in this same trend, Hans von Storch et al., in their 2004 paper *Reconstructing Past Climate from Noisy Data*, used a coupled atmosphere-ocean model simulation of the past 1000 years to test the skill of past empirical reconstructions, specifically those of Mann et al. 1998, 1999. They found that while previous millennium based multi-proxy records find small amplitude variations followed by a clear warming trend in the past two centuries, the centennial variability of the Northern Hemisphere temperature is underestimated by these regression based methods. Their results also suggest that actual variability may have been at least twice as large as the variability predicted in these past studies. The authors surmise that this conclusion probably applies to most regression-based methods of analysis and that other methods that estimate past temperatures with physical (instead of statistical) methods or regression methods that address retention of low-frequency variability in proxies may be free from this critique.

Another 2005 paper *Are Multiproxy Climate Reconstructions Robust?* by Gerd Burger and Ulrich Cubasch questions whether these methods are statistically significant enough to be able to make robust conclusions. Burger and Ulrich describe sixty-four climate reconstructions, based on regression of temperature fields on multi-proxy networks, which are each distinguished by at least one of six standard criteria of this method. By combining these criteria Burger and Ulrich define numerous variants on millennial histories. No one criterion can account for the number of variations and no particular variant is more valid than another. Even the variant with the best reduction of error statistic is the furthest variation from the climate history of Mann et al. 1998. Burger and

Cubasch conclude that the regression model is not valid when applied in an extrapolative manner, as in climate reconstruction.

In a 2006 paper, *The Spatial Extent of 20th Century Warmth in the Context of the Past 1200 Years*, Timothy Osborn and Keith Briffa examine the most prominent anomalies in proxy records since AD 1200. They state that the most significant anomaly is the geographical extent of warmth in the middle to late 20th century. They also detail anomalies during AD 890 to 1170 and AD 1580 to 1850 as being consistent with the concepts of a Medieval Warming Period (MWP) and Little Ice Age (LIA), respectively. However, they found that when comparing these anomalies with instrumental temperatures of the 20th century, the spatial or geographical extent of this warm anomaly is far greater than that of the MWP or LIA. Their study consisted of fourteen regional temperature-related proxy records. Since it is not possible to conduct a direct comparison between proxy records and instrumental temperatures, the proxy data analysis was conducted with each series normalized over the 1856 to 1995 period, or the period where proxy and instrumental data overlap. Relative to a decadal time scale, Osborn and Briffa found supporting evidence for the MWP and LIA, but their geographical reach appeared restricted since these anomalies were sensitive to specific proxy records.

Analysis

While the work of Michael Mann and colleagues presents what appears to be compelling evidence of global temperature change, the criticisms of McIntyre and McKitrick, as well as those of other authors mentioned are indeed valid. Because the error and uncertainty involved in climate reconstructions is magnified with each preceding year, the ability to make certain conclusions about the climate at the beginning of the millennium is not very robust. This is even less robust considering the inability to actually calculate an accurate uncertainty for these reconstructions. Additionally, the work of Esper, Von Storch and Moberg make valid arguments for the inclusion of low-frequency proxies as well as the inability of PCA to effectively measure variations on a multi-centennial scale. This pitfall of PCA is further complicated by its tendency for misuse during the calibration process, specifically the decentering of the mean that McIntyre and McKitrick mention.

The papers of Mann et al. in themselves are written in a confusing manner, making it difficult for the reader to discern the actual methodology and what uncertainty is actually associated with these reconstructions. Vague terms such as "moderate certainty" (Mann et al. 1999) give no guidance to the reader as to how such conclusions should be weighed. While the works do have supplementary websites, they rely heavily on the reader's ability to piece together the work and methodology from raw data. This is especially unsettling when the findings of these works are said to have global impact, yet only a small population could truly understand them. Thus, it is no surprise that Mann et al. claim a misunderstanding of their work by McIntyre and McKitrick.

In their works, Mann et al. describe the possible causes of global climate change in terms of atmospheric forcings, such as anthropogenic, volcanic, or solar forcings. Another questionable aspect of these works is that linear relationships are assumed in all forcing-climate relationships. This is a significantly simplified model for something as complex as the earth's climate, which most likely has complicated nonlinear cyclical processes on a multi-centennial scale that we do not yet understand. Mann et al. also infer that since there is a partial positive correlation between global mean temperatures in the 20th century and CO_2 concentration, greenhouse-gas forcing is the dominant external forcing of the climate system. Osborn and Briffa make a similar statement, where they casually note that evidence for warming also occurs at a period where CO_2 concentrations are high. A common phrase among statisticians is *correlation does not imply causation.* The variables affecting earth's climate and atmosphere are most likely to be numerous and confounding. Making conclusive statements without specific findings with regard to atmospheric forcings suggests a lack of scientific rigor and possibly an agenda.

It is also interesting to note that Mann's dissertation focused on 70 to 100 year climate signal variability, yet his future work does not have a similar component. His subsequent papers focus heavily on tree ring measurements, which provide data on a decadal or yearly scale. In later work, he also makes no mention of the ocean circulation variables, which he describes in his thesis as being integral to the variation in climate. If this type of forcing is a natural variable, it makes the conclusions about atmospheric forcings seem incomplete.

The work initiated by Mann and his colleagues is still in its infancy, and as such further study, the use of wider proxy networks and the development of more sophisticated climate models will all be necessary future steps in propagating this research. It is not expected or likely that after preliminary research, definitive conclusions can be made about the earth's climate over the past millennium.

4. RECONSTRUCTIONS AND EXPLORATION OF PRINCIPAL COMPONENT METHODOLOGIES

Mann et al. (2005) identify two major methods of climate reconstruction, which they describe respectively as climate field reconstruction (CFR) methods[5] and what they describe as *simple* climate-plus-scale (CPS) methods. CFR methods are claimed to "assimilate proxy records into a reconstruction of underlying patterns of past climate change" and among papers identified as using these methods are MBH 98, Evans et al. (2002), Luterbacher et al. (2002), Rutherford et al. (2005) and Zhang et al. (2004). In contrast CPS methods are said to "composite a number of proxy series and scales the resulting composite against a target (e.g. Northern Hemisphere temperature) instrumental series." Examples of papers using the CPS methods include Jones et al. (1998), Crowley and Lowery (2000), Briffa et al. (2001), Esper et al. (2002), Mann and Jones (2003) and Crowley et al. (2003). Although the language describing both of these methods seems somewhat obscure, it would appear that CFR methods are just principal component methods as describe earlier and in the appendix and that CPS methods are just simple averaging of climate proxies and then scaling them to actual temperature records.

The key issue in dispute is the CFR methodology as used in MBH98 and MBH99. The description of the work in MBH98 is both somewhat obscure and as others have noted incomplete. The essence of the discussion is as follows. Principal component methods are normally structured so that each of the data time series (proxy data series) are centered on their respective means and appropriately scaled. The first principal component attempts to discover the composite series that explains the maximum amount of variance. The second principal component is another composite series that is uncorrelated with the first and that seeks to explain as much of the remaining variance as possible. The third, fourth and so on follow in a similar way. In MBH98/99 the authors make a simple seemingly innocuous and somewhat obscure calibration assumption. Because the instrumental temperature records are only available for a limited window, they use instrumental temperature data from 1902-1995 to calibrate the proxy data set. This would seem reasonable except for the fact that temperatures were rising during this period. So that centering on this period has the effect of making the mean value for any proxy series exhibiting the same increasing trend to be decentered low. Because the proxy series exhibiting the rising trend are decentered, their calculated variance will be larger than their normal variance when calculated based on centered data, and hence they will tend to be selected preferentially as the first principal component. (In fact the effect of this can clearly be seen RPC no. 1 in Figure 5 in MBH98.). Thus, in effect, any proxy series that exhibits a rising trend in the calibration period will be preferentially added to the first principal component.

The centering of the proxy series is a critical factor in using principal components methodology properly. It is not clear that Dr. Mann and his associates even realized that their methodology was faulty at the time of writing the MBH paper. The net effect of the decentering is to preferentially choose the so-called hockey stick shapes. While this error would have been less critical had the paper been overlooked like many academic papers are, the fact that their paper fit some policy agendas has greatly enhanced their paper's visibility. Specifically, global warming and its potentially negative consequences have been central concerns of both governments and individuals. The 'hockey stick' reconstruction of temperature graphic dramatically illustrated the global warming issue and was adopted by the IPCC and many governments as the poster graphic. The graphics' prominence together with the fact that it is based on incorrect use of PCA puts Dr. Mann and his co-authors in a difficult face-saving position. We have been to Michael Mann's University of Virginia website and downloaded the materials there. Unfortunately, we did not find adequate material to reproduce the MBH98 materials.

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[5] CFR methods are essentially the methodology that was used in MBH98 and MBH99. However, the methods in MBH98 and MBH99 were not formally called CFR methods, the climate field reconstruction phrase being coined only later.

We have been able to reproduce the results of McIntyre and McKitrick (2005b). While at first the McIntyre code was specific to the file structure of his computer, with his assistance we were able to run the code on our own machines and reproduce and extend some of his results. In Figure 4.1, the top panel displays PC1 simulated using the MBH98 methodology from stationary trendless red noise. The bottom panel displays the MBH98 Northern Hemisphere temperature index reconstruction.

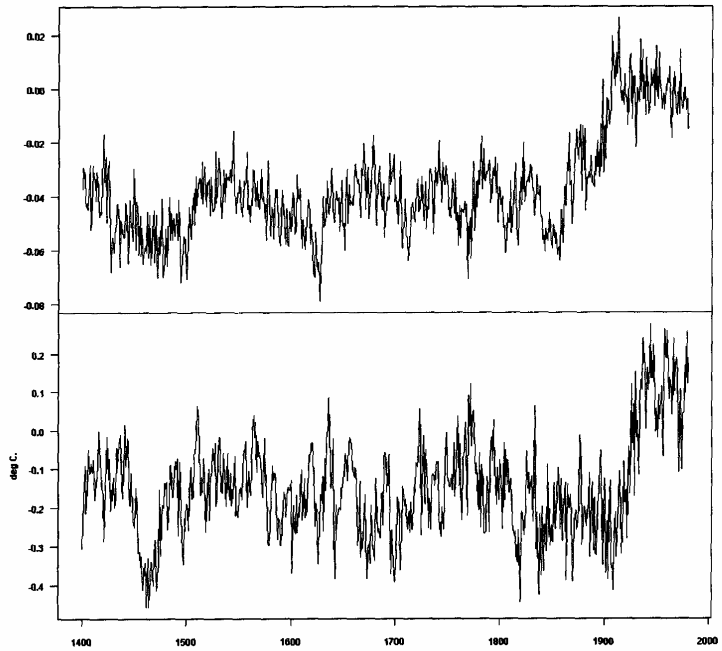

Figure 4.1: Reproduced version of Figure 1 in McIntyre and McKitrick (2005b). Top panel is PC1 simulated using MBH 98 methodology from stationary trendless red noise. Bottom panel is the MBH98 Northern Hemisphere temperature index reconstruction.

Discussion: The similarity in shapes is obvious. As mentioned earlier, red noise exhibits a correlation structure, which, although it is a stationary process, to will depart from the zero mean for minor sojourns. However, the top panel clearly exhibits the hockey stick behavior induced by the MBH98 methodology.

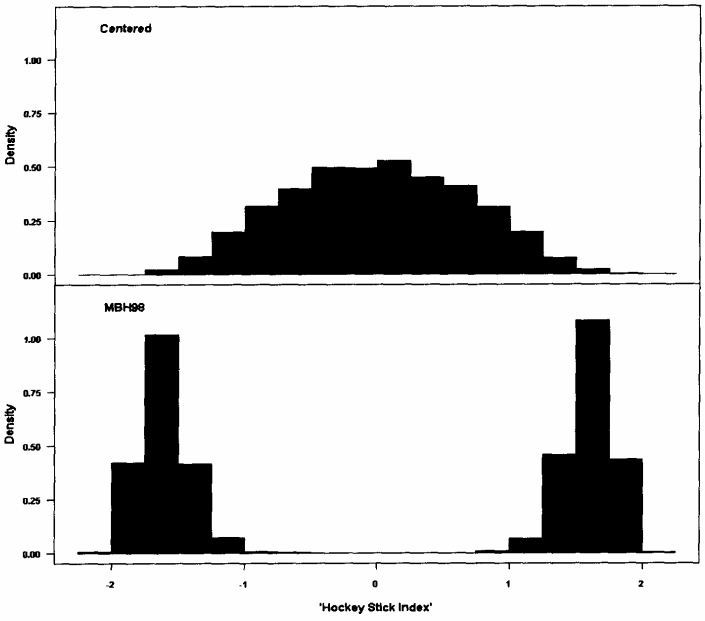

Figure 4.2: This is our recomputation of the Figure 2 in McIntyre and McKitrick (2005b). The lower panel is what MM05b call the 'Hockey Stick Index' for PC1s. For 10,000 simulated PC1s, the histogram shows the distribution of the difference between the 1902-1980 mean and the 1400-1980 mean divided by the 1400-1980 standard deviation using the MBH98 data transformation. The top histogram is based on the centered PCA computation.

Discussion: Figure 4.2 is our recomputation of the Figure 2 in McIntyre and McKitrick (2005b). The lower panel is what MM05b call the 'Hockey Stick Index' for PC1s. For 10,000 simulated PC1s, the histogram shows the distribution of the difference between the 1902-1980 mean and the 1400-1980 mean divided by the 1400-1980 standard deviation using the MBH98 data transformation. The top histogram is based on the centered PCA computation. Although our result is not identical to Figure 2 in MM05b, it reproduces the essential features of MM05b. In particular, the MBH98 methodology (and follow-on studies that use the MBH98 methodology) show a marked preference for 'hockey stick" shapes. The negative values between –2 and –1 indicate the 1902-1980 mean is lower hence the blade of the hockey stick is turned down, while the positive values between 1 and 2 in the bottom panel indicate the hockey stick blade is turned up.

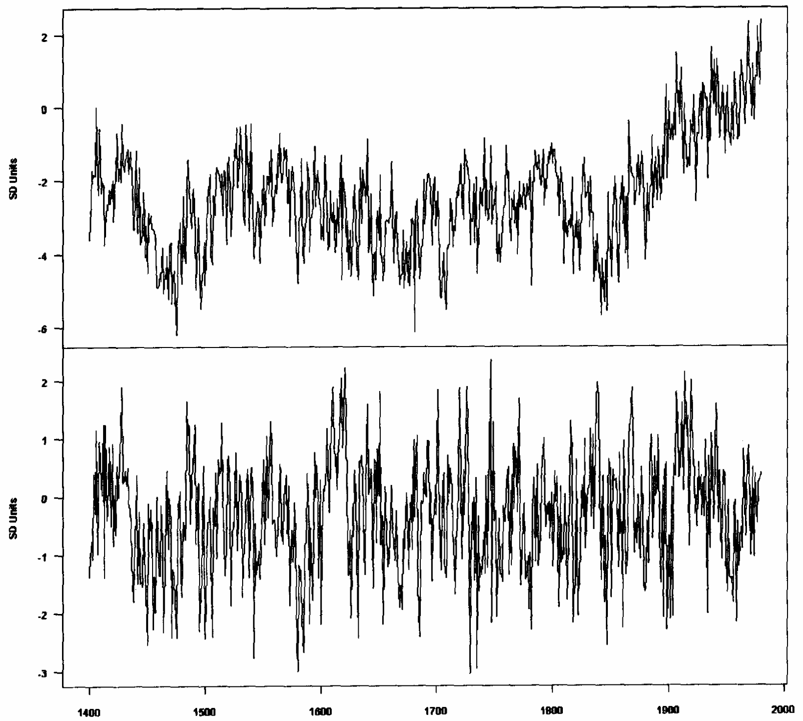

Figure 4.3: This is a recomputation of Figure 3 from MM05b. The North American Tree Network PC1 is a proxy featured prominently in MBH98. It is a PCA reconstruction of a series of tree ring proxies using the MBH98 methodology. The upper panel is the PCA reconstruction using the MBH98 data transformation. The lower panel is the reconstruction using the centered PCA methodology.

Discussion: In addition to the hockey stick shape of the upper panel it is worth noting that the lower panel exhibits considerably more variability. As mentioned in earlier discussions, PCA seeks to identify the largest contributor to the variance. It is not inherently a smoothing mechanism. The MBH98 offset of the mean value creates an 'artificially large deviation' from the desired mean value of zero.

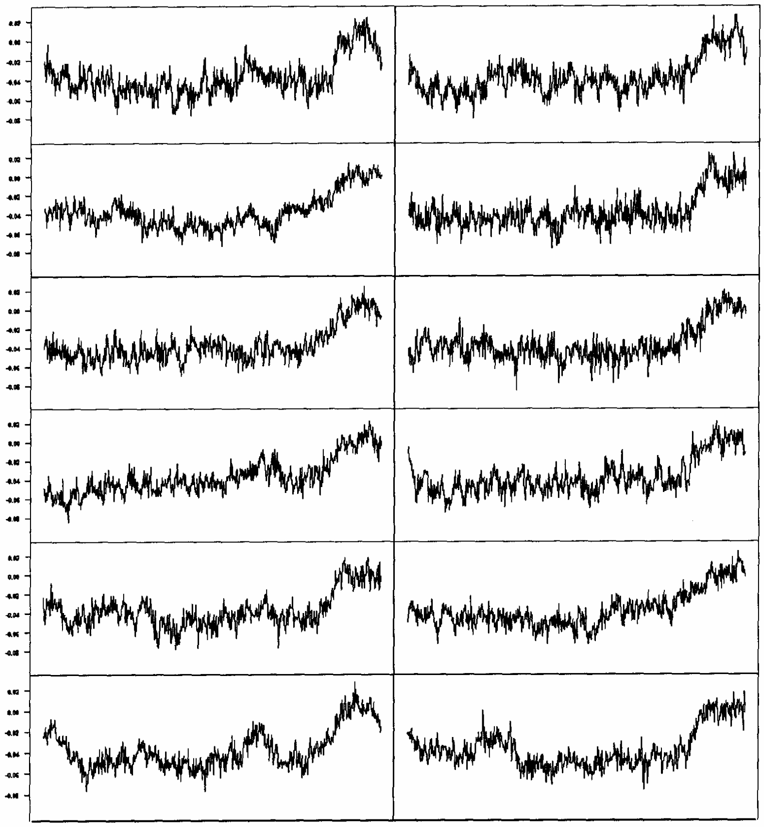

Figure 4.4: One of the most compelling illustrations that McIntyre and McKitrick have produced is created by feeding red noise [AR(1) with parameter = 0.2] into the MBH algorithm. The AR(1) process is a stationary process meaning that it should not exhibit any long-term trend. The MBH98 algorithm found 'hockey stick' trend in each of the independent replications.

Discussion: Because the red noise time series have a correlation of 0.2, some of these time series will turn upwards [or downwards] during the 'calibration' period[6] and the MBH98 methodology will selectively emphasize these upturning [or downturning] time series.

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[6] 1902-1980

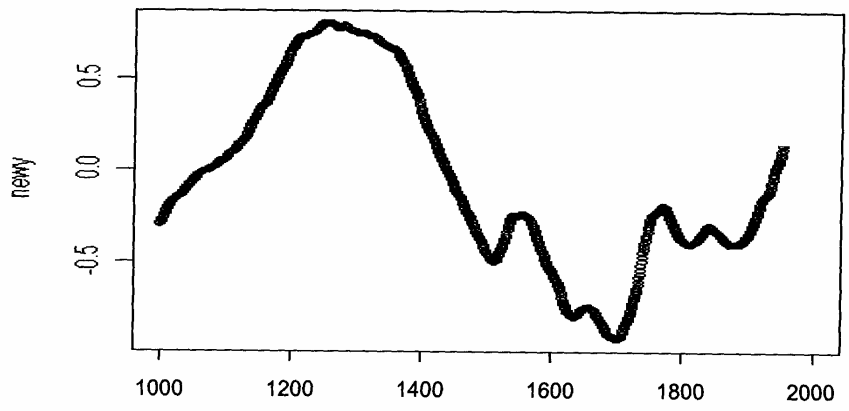

Figure 4.5: Here we have digitized the temperature profile as presented in the IPCC Assessment Report 1990. The early period between 1100 to about 1400 of above average temperatures is known as the Medieval Warm Period and the period from about 1500 to 1900 is known as the Little Ice Age.

Discussion: In Figure 4.5, we have digitized the temperature profile as presented in the IPCC Assessment Report 1990. The early period between 1100 to about 1400 of above average temperatures is known as the Medieval Warm Period and the period from about 1500 to 1900 is known as the Little Ice Age. The 1990 report was not predicated on a global warming scenario. It is clear that at least in 1990, the Medieval Warm Period was thought to have temperatures considerably warmer than the present era.

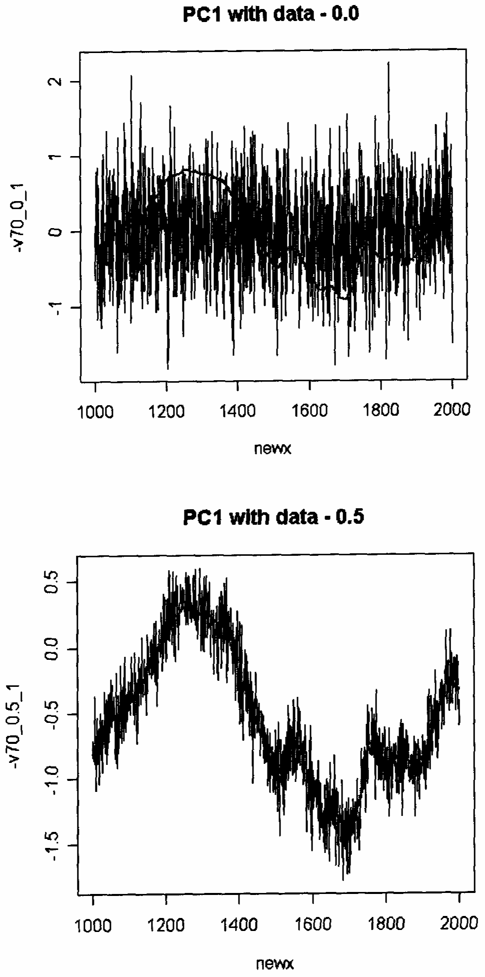

Figure 4.6: We created a pseudo-proxy scenario by creating 69 white noise process simulations, i.e. 69 white noise proxies and adding the profile in Figure 4.5 as the 70th proxy. All 70 pseudo-proxies were standardized to have a variance of one. In the top panel we applied the properly centered version of PCA. The black curve is the PC1. Offsetting the IPCC 1990 profile from zero mean by .5, and using it as the 70th proxy, we applied the MBH98 (CFR) algorithm to the proxies to obtain a 'reconstruction' of the Figure 4.5 profile in the bottom panel.

Discussion: Although there has been much discussion about the 'hockey stick' shape, the point of Figure 4.6 is that the offset procedure used in MBH98 (CFR) algorithm will reproduce any desired shape depending on what shape exists in the proxies. Recall in Figure 4.6 only one proxy of 70 has the shape, but the offset version of the PCA clearly picks out the shape.

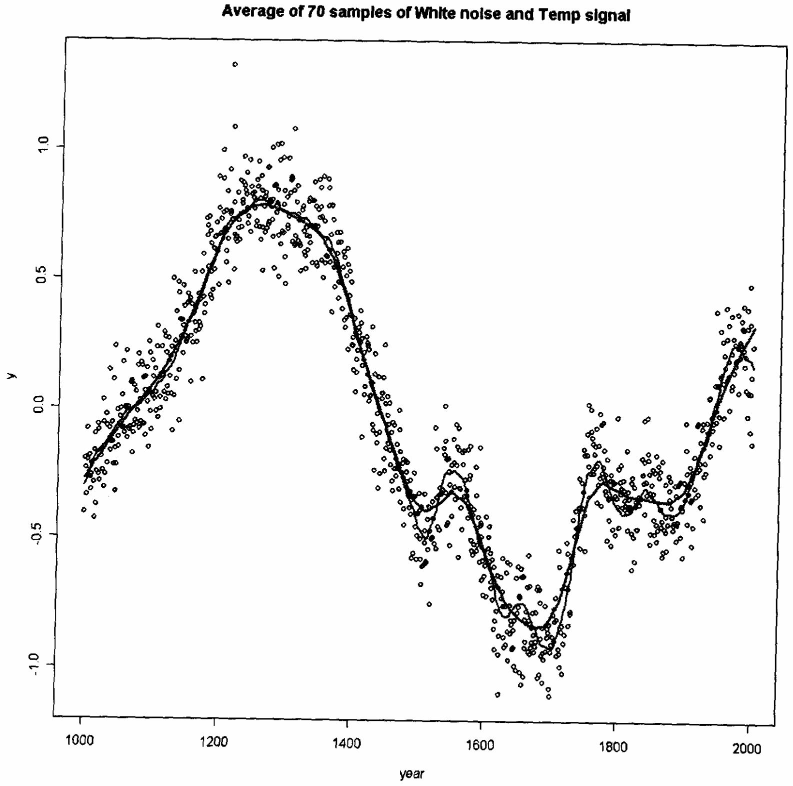

Figure 4.7: In this illustration, we created a different pseudo-proxy scenario by taking 70 copies of the Figure 4.5 profile and adding independent white noise processes to it. We then applied the CPS methodology to these pseudo-proxies to obtain another 'reconstruction' of the Figure 4.5 profile.

Discussion: In Figure 4.7, the blue curve is the original profile, the black dots are the result of applying the CPS methodology, i.e. a simple average, and the red curve is the result of applying a smoothing procedure known in the statistical literature as LOESS.

The point being made with Figures 4.6 and 4.7 is that if there are hockey sticks in the underlying data and if they are decentered, then the CFR methodology will selectively emphasize them. Similarly, if there are 'hockey sticks' in the data series and the remainder of the data are uncorrelated noise, then the CPS method will also emphasize the 'hockey stick' shape. However, if the data contain other shapes and these methods are applied to data containing these other shapes, then these methods will selectively pick out those shapes. In Figure 4.6, by decentering the 1990 profile, we inflate its effective variance so that PCA will preferentially pick it as the first principal component. In Figure 4.7, the independent white noise will be incoherent[7] and thus tend to cancel out while the 'signal' is the same in every proxy and will thus tend to be additive. The point here is that if each (or even a large percentage) of the proxies is selected with the hockey stick shape, then the incoherent noise will cancel and the coherent 'hockey stick' shape will emerge. Thus even discussions of 'independent replications' of the hockey stick results by different methods may not be what they superficially appear to be.

Remark: Technically speaking, the MBH98 algorithm is not principal components. Principal components are obtained theoretically through an eigenanalysis of the covariance matrix (which uses the centered data). Now there is an equivalent and numerically preferred method of obtaining the principal components by finding the so-called singular-value decomposition (SVD) of the original data matrix. The PC1 is the first of the right singular vectors in the SVD. However, this is only the case if the data matrix columns have been centered. Since the MBH98 algorithm does not center the data matrix, the SVD is actually returning a different vector than PC1. One may investigate this vector but it is incorrect to identify it as the principal component. Appendix A provides the mathematical details of this remark.

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[7] Incoherent is a technical term meaning that the time series have no common form and that they are statistically independent. If each time series contains the same or similar form (shape), then this is referred to as a coherent part of the time series.

5. SOCIAL NETWORK ANALYSIS OF AUTHORSHIPS IN TEMPERATURE RECONSTRUCTIONS

One of the interesting questions associated with the 'hockey stick controversy' are the relationships among the authors and consequently how confident one can be in the peer review process. In particular, if there is a tight relationship among the authors and there are not a large number of individuals engaged in a particular topic area, then one may suspect that the peer review process does not fully vet papers before they are published. Indeed, a common practice among associate editors for scholarly journals is to look in the list of references for a submitted paper to see who else is writing in a given area and thus who might legitimately be called on to provide knowledgeable peer review. Of course, if a given discipline area is small and the authors in the area are tightly coupled, then this process is likely to turn up very sympathetic referees. These referees may have co-authored other papers with a given author. They may believe they know that author's other writings well enough that errors can continue to propagate and indeed be reinforced.

In order to answer such questions about the relationships among authors in the area of temperature reconstructions, we developed two datasets. The first specifically focusing on Dr. Mann was developed by first considering all of his co-authors and then examining the abstracts produced by the co-authors. We focus on Dr. Mann because he is the lead author of MBH98/99 and because he is extremely influential in this area as can be seen by his high degree of centrality. Drs. Bradley and Hughes also appear in the social network, but do not exhibit the centrality that Dr. Mann exhibits. We used the Engineering Compendex database, which is available on the web, to develop the abstract database of his coauthors. Based on the expanded database we examined the co-authors of his co-authors. This first database is Dr. Mann centric with the idea of understanding the relationships among his reasonably close associates. This first database consisted of 43 individuals all of whom have close ties to Dr. Mann. The second database was developed by looking for abstracts in the Engineering Compendex that treated aspects of temperature reconstruction. This second more expanded database contained more authors. In our analysis, we considered only the top 50 and top 75 in terms of numbers of papers published. There were more authors who wrote only one paper in the area and are thus not consistent researchers in the area. We report here the analysis with the top 75 authors, i.e. the 75 most frequently published authors.

Figures 5.1 through 5.4 deal with the first dataset of the closest associates of Dr. Mann. Figures 5.5 through 5.7 deal with the 75 most frequently published authors.

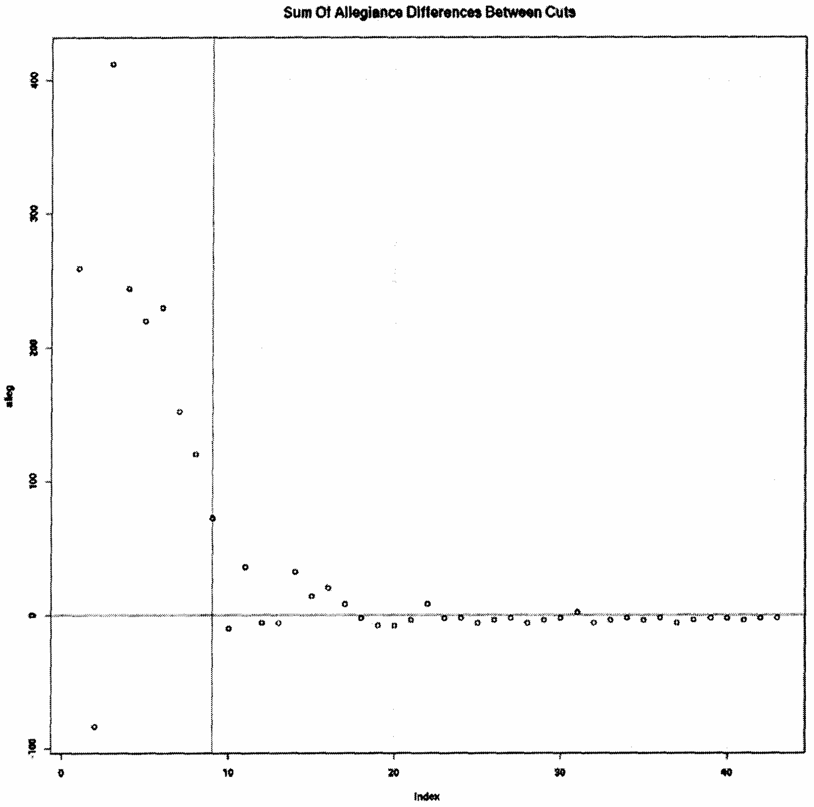

Figure 5.1: The allegiance plot for the Mann co-authors

Discussion: As mentioned in Section 2.3, allegiance is a concept/method useful for determining the clusters and cluster sizes for a social network. In some sense this determines second-order substructure within a social network. Recall that the allegiance for a block or cluster depends on having a large number of internal connections within the block and relatively few connections external to the block. The allegiance criteria are applied recursively until the allegiance criteria drop essentially to zero. This happens after nine blocks or clusters. Michael Mann is treated as a separate block because he has connections with every one of the other 42 researchers. Thus the second partition has an allegiance that temporarily drops below zero.

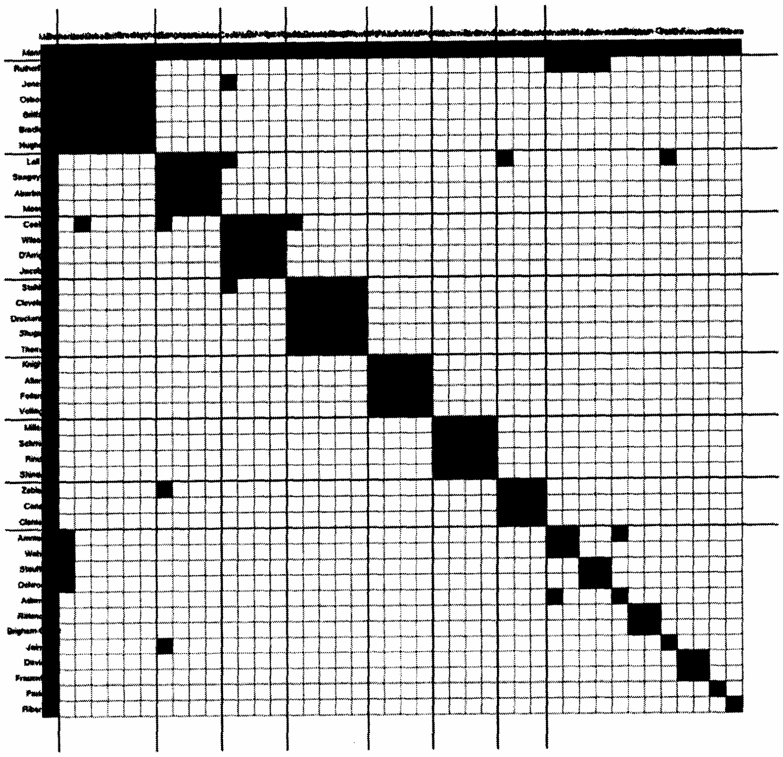

Figure 5.2: This is a matrix indicating the nine blocks of the 43 investigators. The black squares indicate that there is a co-author relationship.

Discussion: The block (cluster) structure is very clear. Michael Mann is a co-author with every one of the other 42. The black squares on the diagonal indicate that the investigators work closely within their group, but not so extensively outside of their group. The occasional off diagonal boxes indicate that some investigators have joint papers with investigators outside of their immediate group. The order of the authors on the vertical and horizontal axes is the same. Unfortunately, there is overprinting on the horizontal so that individual authors are not readable. However, it is immediately clear that the Mann, Rutherford, Jones, Osborn, Briffa, Bradley and Hughes form a clique, each interacting with all of the others. A clique is a fully connected subgraph, meaning everyone in the clique interacts with every one else in the clique.

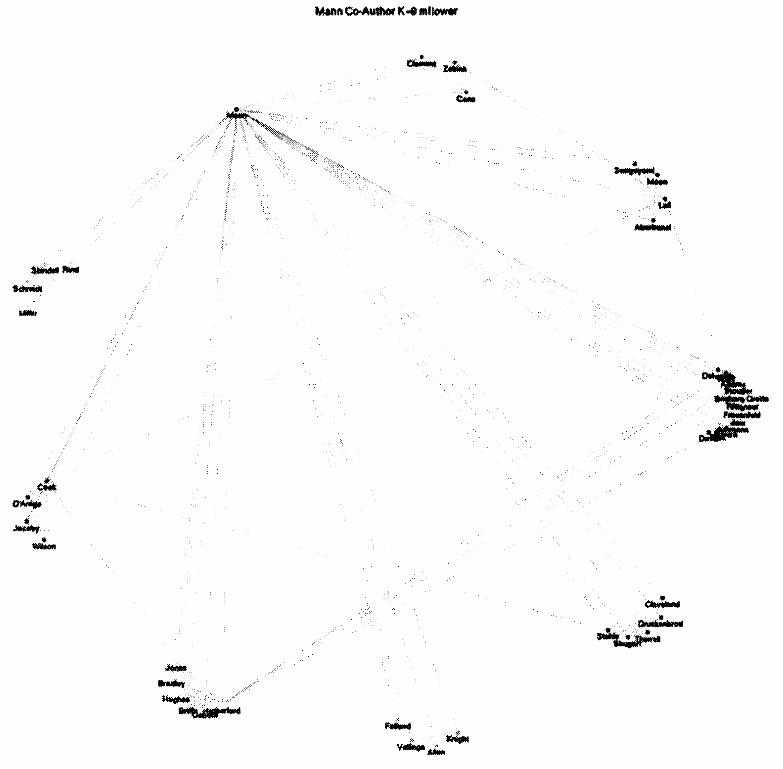

Figure 5.3: The classic social network view of the Mann co-authors. Each block or subcluster is represented along an arc.

Discussion: As mentioned before, Michael Mann is his own group since he is a co-author with each of the other 42. The cliques are very clear in this layout. In addition to the Mann-Rutherford-Jones-Osborn-Briffa-Bradley-Hughes clique there are several others that are readily apparent. They are Rind-Shindell-Schmidt-Miller, Cook-D'Arrigo-Jacoby-Wilson, Folland-Vellinga-Allan-Knight, Stahle-Shugart-Therrell-Druckenbrod-Cleveland, Sangoyomi-Moon-Lall-Abarbanel, and Clement-Zebiak-Cane. The last cluster is somewhat of the miscellaneous cluster of people who had published with Michael Mann, but not much if at all with each other.

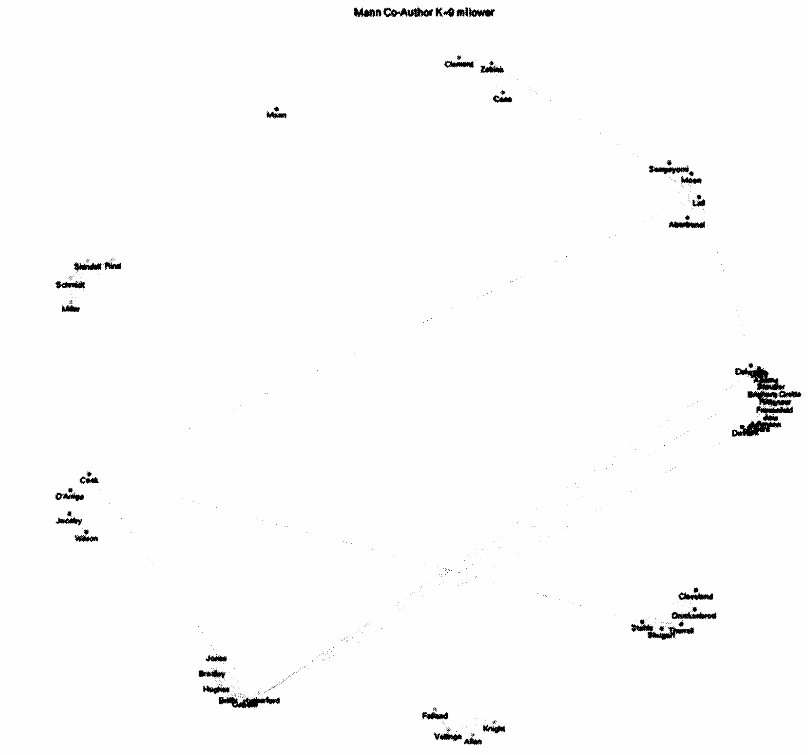

Figure 5.4: The Figure 5.3 with the linkages to Dr. Michael Mann removed.

Discussion: The dominance of Dr. Mann in terms of centrality is very clear comparing Figure 5.3 with Figure 5.4. Other authors of the remaining 42 that have some degree of centrality are Cook, Rutherford, and Lall.

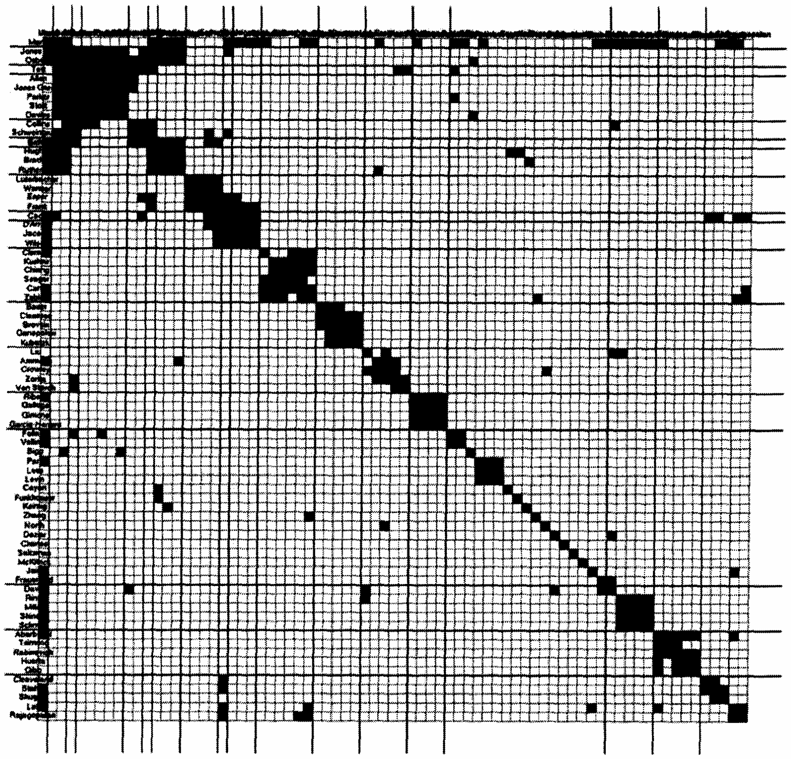

Figure 5.5: Listed here are the 75 most frequently published authors in the area of climate reconstruction. Here there are 18 blocks.

Discussion: The structure among these most frequently published authors is somewhat less than the earlier group of 43. However, the block diagonal structure still remains strong so that we do find clusters interacting with each other. The Mann-Briffa-Hughes-Bradley-Rutherford clique still is readily apparent from just the allegiance computation perspective without any forced joining.

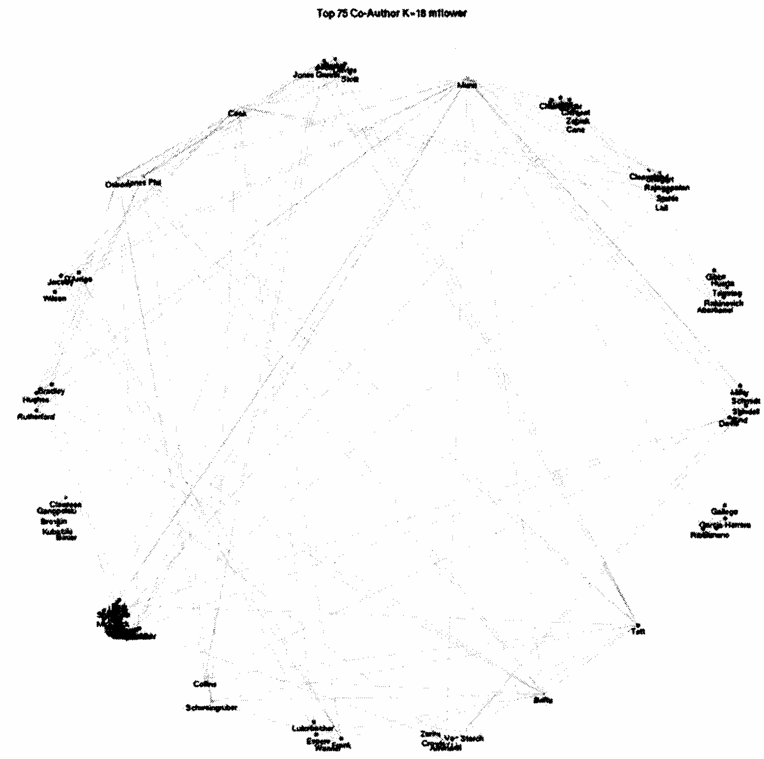

Figure 5.6: The social network of the 75 most frequently published authors in the area of climate reconstruction.

Discussion: There are some interesting features. Although Michael Mann remains an author with high centrality, Tett, Briffa and Cook emerge as belonging to their own cluster and they also exhibit high centrality. Schweingruber and Collins also appear to have relatively high centrality. One interesting observation is that although Tett is fairly central, he has no direct linkage to Mann. Similarly the Gareth Jones-Allen-Parker-Davies-Stott clique also has no direct linkage to Mann. There are two Joneses Gareth Jones is not the same person as the person previously labeled as Jones.

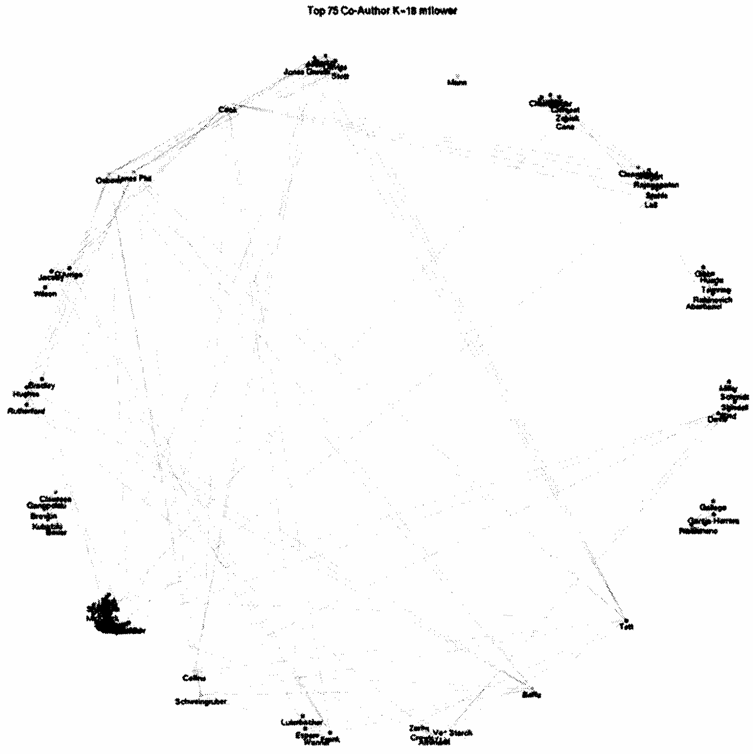

Figure 5.7: Figure 5.6 with linkages to Michael Mann removed.

Discussion: Several other interesting details emerge. The clique Claussen-Ganopolski-Brovkin-Kubatzki-Bauer is completely isolated from the other researchers in the area. Similarly, the Ribe-Gimeno-Garcia-Herrera-Gallego clique and the Arbarbanel-Rabinovich-Tsimning-Huerta-Gibb clique are nearly isolated with only linkages to Mann in the first case and linkages to Mann and Lall in the second case.

ID	BJ03	MBH 98-99	Jones 98	CL00 CL02	Briffa 00	Esper 02	Mann Jones 03	BHD 03	Jones-Mann 04	Moberg 05	OB 06	DWJ 06
Polar Urals	●	●	●	●	●	●	●	●	●	●	●	●
Tornetrask	●	●	●	●	●	●	●	●	●	●	●	●
Jacoby Mongolia		●			●	●	●	●	●		●	●
Jacobytreeline	●	●	●	●	●				●			●
Bristlecone/foxtails (PC1)		●		●		●	●	●	●	●	●	
Dunde/Yang Composite		●		●			●	●	●	●	●	
Greenland dO18		●	●	●			●	●	●		●	
Jasper			●	●	●	●			●		●	●
Taimyr					●	●	●	●		●	●	●
European docs	●	●	●						●		●	
England docs	●	●	●	●					●			
Chesapeake Mg/Ca							●		●	●	●	
Briffa WUSA	●	●	●						●			
Tasmania_Cook	●	●	●				●	●	●			
Quelccaya		●					●	●	●			
Boninsegna Rio Alerce	●	●	●					●				
Yakutia		●			●							●
Tirol						●					●	●
Svalbard melt	●	●	●									
Kameda melt	●	●	●									
New Caledonia		●	●						●			
Law Dome			●				●		●			
Great Barrier Reef		●	●						●			
Galapagos		●	●						●			
E China docs	●							●	●			
Sajama								●	●			
New Zealnd Norton	●	●										
Lenca			●				●					
Huascaran								●	●			
Cook- Oroko							●	●				
Bonisegna araucaria	●	●										
Sargasso Sea dO18				●						●		
N China doc	●								●			
Mangazeja						●					●	
Jaemtland						●						●
Greenland Borehole							●			●		
Fritts USA	●	●										
France, Morocco tree rings		●		●								
Dasuopu								●	●			
Conroy Lake sediments				●						●		
Bonif, Quebec						●					●	
Beijing stalagmite								●		●		
Agazziz melt	●									●		

Figure 5.8: Relationships of major temperature reconstruction papers and the proxies that they used.

Discussion: The social network analysis of authors' relations suggests that the "independent reconstructions" are not as independent as one might guess. Indeed, the matrix outlined in Figure 5.8 illustrates the proxies that are used more than one time in twelve major temperature reconstruction papers. The black boxes indicate that the proxy was used in a given paper. It is clear that many of the proxies are re-used in most of the papers. It is not surprising that the papers would obtain similar results and so cannot really claim to be independent verifications. As a graphical comparison of a number of the reconstructions, see Figure 5.9 below taken from D'Arrigo et al. (2006).

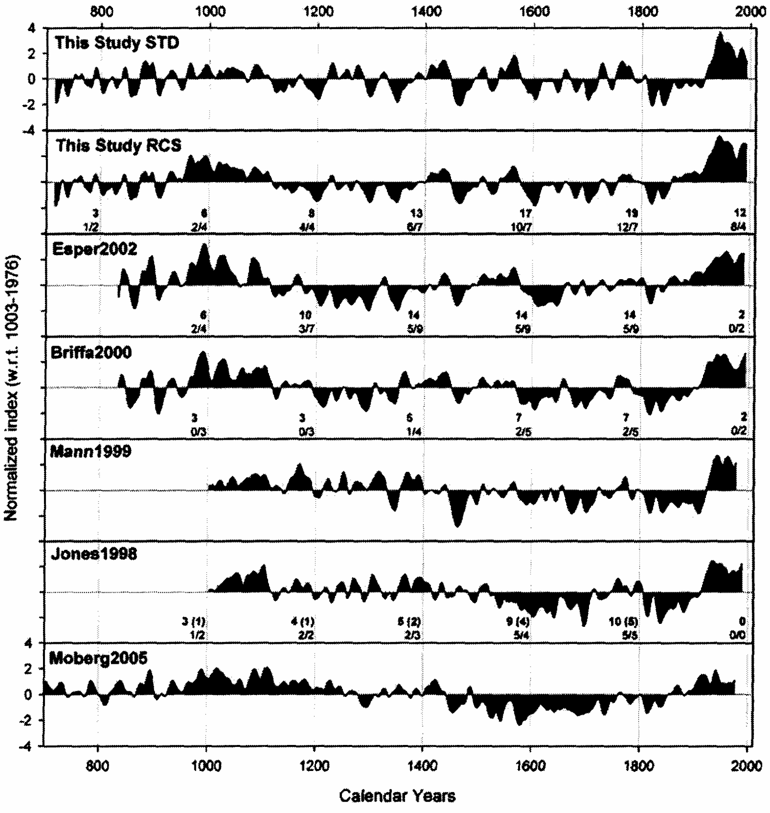

Figure 5.9. A comparison of several different reconstructions. From D'Arrigo et al. (2006)

Discussion: There are variations in the temperature reconstruction indicating the fundamental uncertainty in the reconstruction process. Essentially all agree that there was a medieval warm period centered on AD 1000 and a little ice age from at least 1600 to 1850. There is consensus in these reconstructions that the global average temperature has risen over the last 400 years. However, what must be added is that temperatures were below average in AD 1600. Both Esper et al. (2002) and Moberg et al. (2005) indicate that current global temperatures are not warmer that the medieval warm period.

6. FINDINGS

Some of the issues have been addressed in the text of the description of the methodologies in the earlier part of our discussion. For completeness and clarity we will summarize again here.

1. In general we found the writing of MBH98 somewhat obscure and incomplete. The fact that MBH98 issued a further clarification in the form of a corrigendum published in *Nature* (Mann et al. 2004) suggests that these authors made errors and incomplete disclosures in the original version of the paper. This also suggests that the refereeing process was not as thorough as it could have been.

2. In general, we find the criticisms by MM03, MM05a and MM05b to be valid and their arguments to be compelling. We were able to reproduce their results and offer both theoretical explanations (Appendix A) and simulations to verify that their observations were correct. We comment that they were attempting to draw attention to the deficiencies of the MBH98-type methodologies and were not trying to do paleoclimatic temperature reconstructions[8].

3. As we mentioned earlier, it is common for data analysis and pattern recognition experts to use a training sample for calibration. Normally one would seek to have the training data to be representative of what one might expect from the entire dataset. Because the temperature profile in the 1902-1995 is not similar, because of increasing trend, to the millennium temperature profile, it is not fully appropriate for the calibration and, in fact, leads to the misuse of the principal components analysis. However, the narrative in MBH98 on the surface sounds entirely reasonable on this calibration point, and could easily be missed by someone who is not extensively trained in statistical methodology. Dr. Mann has close ties to both Yale University and Pennsylvania State University. We note in passing that both Yale University and Pennsylvania State University have Departments of Statistics with excellent reputations[9]. Even though their work has a very significant statistical component, based on their literature citations, there is no evidence that Dr. Mann or any of the other authors in paleoclimatology studies have significant interactions with mainstream statisticians.

4. In response to the letter from Chairman Barton and Chairman Whitfield, Dr. Mann did release several websites with extensive materials, including data and code. The material is not organized or documented in such a way that makes it practical for an outsider to replicate the MBH98/99 results. For example, the directory and file structure Dr. Mann used are embedded in the code. It would

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[8] MM05a was critiqued by Wahl and Ammann (2006) and the Wahl et al. (2006) based on the lack of statistical skill of their paleoclimate temperature reconstruction. Thus these critiques of the MM05a and MM05b work are not to the point.

[9] The Penn State and Yale Departments were ranked 19 and 20 respectively in the National Research Council publication, *Research-Doctorate Programs in the United States*, NRC (1995).

take extensive restructuring of the code to make it compatible with a local machine. Moreover, the cryptic nature of some of the MBH98/99 narratives means that outsiders would have to make guesses at the precise nature of the procedures being used.

5. As mentioned in our introduction, much of the discussion on the 'hockey stick' issue has taken place on competing web blogs. Our committee believes that web blogs are not an appropriate way to conduct science and thus the blogs give credence to the fact that these global warming issues are have migrated from the realm of rational scientific discourse. Unfortunately, the factions involved have become highly and passionately polarized.

6. Generally speaking, the paleoclimatology community has not recognized the validity of the MM05 papers and has tended dismiss their results as being developed by biased amateurs. The paleoclimatology community seems to be tightly coupled as indicated by our social network analysis, has rallied around the MBH98/99 position, and has issued an extensive series of alternative assessments most of which appear to support the conclusions of MBH98/99.

7. Our committee believes that the assessments that the decade of the 1990s was the hottest decade in a millennium and that 1998 was the hottest year in a millennium cannot be supported by the MBH98/99 analysis. As mentioned earlier in our background section, tree ring proxies are typically calibrated to remove low frequency variations. The cycle of Medieval Warm Period and Little Ice Age that was widely recognized in 1990 has disappeared from the MBH98/99 analyses, thus making possible the hottest decade/hottest year claim. However, the methodology of MBH98/99 suppresses this low frequency information. The paucity of data in the more remote past makes the hottest-in-a-millennium claims essentially unverifiable.

8. Although we have not addressed the Bristlecone Pines issue extensively in this report except as one element of the proxy data, there is one point worth mentioning. Graybill and Idso (1993) specifically sought to show that Bristlecone Pines were CO_2 fertilized. Bondi et al. (1999) suggest [Bristlecones] "are not a reliable temperature proxy for the last 150 years as it shows an increasing trend in about 1850 that has been attributed to atmospheric CO_2 fertilization." It is not surprising therefore that this important proxy in MBH98/99 yields a temperature curve that is highly correlated with atmospheric CO_2. We also note that IPCC 1996 stated that "the possible confounding effects of carbon dioxide fertilization need to be taken into account when calibrating tree ring data against climate variations." In addition, as use of fossil fuels has risen, so does the release of oxides of nitrogen into the atmosphere, some of which are deposited as nitrates, that are fertilizer for biota. Thus tree ring growth would be correlated with the deposition of nitrates, which, in turn, would be correlated with carbon dioxide release. There are clearly confounding factors for using tree rings as temperature signals.

9. Based on discussion in Mann et al. (2005) and Dr. Mann's response to the letters from the Chairman Barton and Chairman Whitfield, there seems to be at least some confusion on the meaning of R^2. R^2 is usually called the *coefficient of determination* and in standard analysis of variance; it is computed as 1 – (SSE/SST). SSE is the sum of squared errors due to lack of fit (of the regression or paleoclimate reconstruction) while SST is the total sum of squares about the mean. If the fit is perfect the SSE would be zero and R^2 would be one. Conversely, if the fit of the reconstruction is no better than taking the mean value, then SSE/SST is one and the R^2 is 0. On the other hand, the Pearson product moment correlation, r, measures association rather than lack of fit. In the case of simple linear regression, $R^2 = r^2$. However, in the climate reconstruction scenario, they are not the same thing. In fact, what is called β in MBH98 is very close what we have called R^2.

10. We note here that we are statisticians/mathematicians who were asked to comment on the correctness of the methodology found in MBH98/99. In this report we have focused on answering this question and not on whether or not the global climate is changing. We have discussed paleoclimatology only to the extent that it was necessary to make our discussion of the statistical issues clear. The instrumented temperature record makes it clear that global temperatures have risen since 1850 CE. How this present era compares to previous epochs is not clear because the uncertainties in the proxies. However, it is clear that average global temperature increases are not the real focus. It is the temperature increases at the poles that matter and average global or Northern Hemisphere increases do not address the issue. We note that according to experts at NASA's JPL, the average ocean height is increasing by approximately 1 millimeter per year, half of which is due to melting of polar ice and the other half due to thermal expansion. The latter fact implies that the oceans are absorbing tremendous amounts of heat, which is much more alarming because of the coupling of ocean circulation to the atmosphere. (See Wunsch 2002, 2006).

7. CONCLUSIONS AND RECOMMENDATIONS

Conclusion 1. The politicization of academic scholarly work leads to confusing public debates. Scholarly papers published in peer reviewed journals are considered the archival record of research. There is usually no requirement to archive supplemental material such as code and data. Consequently, the supplementary material for academic work is often poorly documented and archived and is not sufficiently robust to withstand intense public debate. In the present example there was too much reliance on peer review, which seemed not to be sufficiently independent.

Recommendation 1. Especially when massive amounts of public monies and human lives are at stake, academic work should have a more intense level of scrutiny and review. It is especially the case that authors of policy-related documents like the IPCC report, *Climate Change 2001: The Scientific Basis*, should not be the same people as those that constructed the academic papers.

Conclusion 2. Sharing of research materials, data, and results is haphazard and often grudgingly done. We were especially struck by Dr. Mann's insistence that the code he developed was his intellectual property and that he could legally hold it personally without disclosing it to peers. When code and data are not shared and methodology is not fully disclosed, peers do not have the ability to replicate the work and thus independent verification is impossible.

Recommendation 2. We believe that federally funded research agencies should develop a more comprehensive and concise policy on disclosure. All of us writing this report have been federally funded. Our experience with funding agencies has been that they do not in general articulate clear guidelines to the investigators as to what must be disclosed. Federally funded work including code should be made available to other researchers upon reasonable request, especially if the intellectual property has no commercial value. Some consideration should be granted to data collectors to have exclusive use of their data for one or two years, prior to publication. But data collected under federal support should be made publicly available. (As federal agencies such as NASA do routinely.)

Conclusion 3. As statisticians, we were struck by the isolation of communities such as the paleoclimate community that rely heavily on statistical methods, yet do not seem to be interacting with the mainstream statistical community. The public policy implications of this debate are financially staggering and yet apparently no independent statistical expertise was sought or used.

Recommendation 3. With clinical trials for drugs and devices to be approved for human use by the FDA, review and consultation with statisticians is expected. Indeed, it is standard practice to include statisticians in the application-for-approval process. We judge this to be a good policy when public health and also when substantial amounts of monies are involved, for example, when there are major policy decisions to be made based on statistical assessments. In such cases, evaluation by statisticians should be standard practice. This evaluation phase should be a mandatory part of all grant applications and funded accordingly.

Conclusion 4. While the paleoclimate reconstruction has gathered much publicity because it reinforces a policy agenda, it does not provide insight and understanding of the physical mechanisms of climate change except to the extent that tree ring, ice cores and such give physical evidence such as the prevalence of green-house gases. What is needed is deeper understanding of the physical mechanisms of climate change.

Recommendation 4. Emphasis should be placed on the Federal funding of research related to fundamental understanding of the mechanisms of climate change. Funding should focus on interdisciplinary teams and avoid narrowly focused discipline research.

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APPENDIX

Appendix A. Mathematical Underpinnings of PCA

A.1 Eigenvectors and Eigenvalues

Consider a general square $d \times d$ matrix $\boldsymbol{M}$ and a vector $\boldsymbol{v}$. The vector $\boldsymbol{v}$ is said to be an eigenvector of the matrix $\boldsymbol{M}$ if $\boldsymbol{Mv} = s\boldsymbol{v}$ where s is a scalar. The way to think about this situation geometrically is that a matrix such as $\boldsymbol{M}$ could have three effects. It could rotate a vector, it could mirror image a vector, and it could rescale a vector. In general it cannot translate a vector. If it rotates a vector or mirror images a vector into a scaled version of itself, then the vector is an eigenvector of the matrix.

As a simple example, consider $\boldsymbol{M} = \begin{pmatrix} 8 & 10 \\ 3 & -5 \end{pmatrix}$ and let $\boldsymbol{v} = \begin{pmatrix} 5 \\ 1 \end{pmatrix}$. Then $\boldsymbol{Mv} = \begin{pmatrix} 8 & 10 \\ 3 & -5 \end{pmatrix}\begin{pmatrix} 5 \\ 1 \end{pmatrix} = \begin{pmatrix} 50 \\ 10 \end{pmatrix} = 10\begin{pmatrix} 5 \\ 1 \end{pmatrix} = 10\boldsymbol{v}$. Thus $\begin{pmatrix} 5 \\ 1 \end{pmatrix}$ is an eigenvector of $\boldsymbol{M}$ and the scaler s is said to be the eigenvalue corresponding to the eigenvector.

Eigenvectors can only be found for square matrices and not all matrices may have eigenvectors. If a $d \times d$ symmetric matrix does have an eigenvector, it will have d of them. The d eigenvectors will be orthogonal (perpendicular) to each other. It is also clear that if we scale a matrix by a scalar amount, say c, then $(c\boldsymbol{M})\boldsymbol{v} = c(\boldsymbol{Mv}) = c(s\boldsymbol{v}) = (cs)\boldsymbol{v}$. Thus if we scale the matrix by an amount c, we scale the corresponding eigenvalue by the same amount. It is convenient to scale the largest eigenvector to length 1. In this case the rescaled eigenvectors become an orthonormal basis for the d-dimensional vector space. In the example above the length of the eigenvector is $\sqrt{5^2 + 1^2} = \sqrt{26}$. Thus $\begin{pmatrix} \frac{5}{\sqrt{26}} \\ \frac{1}{\sqrt{26}} \end{pmatrix}$ is the unit eigenvector whose length is 1.

A.2 Principal Components

The orthogonality of eigenvectors is the attractive feature that we would like to use for multivariate data analysis. We may start out with a series of data vectors, say $\boldsymbol{x}_1, \boldsymbol{x}_2, \ldots, \boldsymbol{x}_d$, which may, in fact, be highly correlated. We would like to create another set of data vectors that are orthogonal. We assume that the vectors, $\boldsymbol{x}_1, \boldsymbol{x}_2, \ldots, \boldsymbol{x}_d$, are column vectors of length n. We may form $\overline{x}_1, \overline{x}_2, \ldots, \overline{x}_d$, the sample means of each column vector. Then $\boldsymbol{x}_1 - \overline{x}_1, \boldsymbol{x}_2 - \overline{x}_2, \ldots, \boldsymbol{x}_d - \overline{x}_d$ are the column vectors of centered data, which can be organized into an $n \times d$ data matrix $\boldsymbol{X}$. Then $\widehat{\boldsymbol{\Sigma}} = \frac{1}{n}\boldsymbol{X}^{\mathrm{T}}\boldsymbol{X}$ is the $d \times d$ covariance matrix corresponding to the data matrix $\boldsymbol{X}$. Here the $\boldsymbol{X}^{\mathrm{T}}$ indicates $\boldsymbol{X}$ transpose. Because $\widehat{\boldsymbol{\Sigma}}$ is a square matrix, the eigenvectors associated with $\widehat{\boldsymbol{\Sigma}}$ are $\boldsymbol{v}_{x1}, \boldsymbol{v}_{x2}, \ldots, \boldsymbol{v}_{xd}$. Let the corresponding eigenvalues be $\alpha_{x1} \geq \alpha_{x2} \geq \cdots \geq \alpha_{xd}$ so that the eigenvalues are arranged in decreasing order of magnitude. The geometric interpretation of the first eigenvector is that it points in the direction that has the largest variance. The second eigenvector points in a direction orthogonal to the first eigenvector and in a direction that has the second largest variance, and so on for the remaining eigenvectors. The eigenvectors are each $d \times 1$ column vectors. The fact that these vectors are unit vectors (i.e. have magnitude 1) is captured by $\boldsymbol{v}_{xi}^{\mathrm{T}}\boldsymbol{v}_{xi} = 1, i = 1, 2, \ldots, d$ and that they are orthogonal is captured by $\boldsymbol{v}_{xi}^{\mathrm{T}}\boldsymbol{v}_{xj} = 0,\ i \neq j,\ i, j = 1, 2, \ldots, d.$ If $\boldsymbol{v}_{xj} = (v_{xj1}, v_{xj2}, \ldots, v_{xjd})^{\mathrm{T}}$ is the column vector for the *j*th eigenvector, then

$$y_{kj} = \sum_{i=1}^{d}(x_{ki} - \overline{x}_k)v_{xji}$$

is the *k*th entry of the *j*th principal component. In matrix form $\boldsymbol{y}_j = \boldsymbol{X}\boldsymbol{v}_j$ is the *j*th principal component. In the notation of Mann et al. (1998), $\boldsymbol{y}_1$ is PC_1 and this corresponds to the first reconstruction. Because $\boldsymbol{X}$ is a $n \times d$ matrix and $\boldsymbol{v}_j$ is a $d \times 1$ column vector, $\boldsymbol{y}_j$ is a $n \times 1$ column vector and in the case of Mann et al. (1998), it will represent the *j*th principal component time series. If we assemble the eigenvectors into a $d \times d$ matrix $\boldsymbol{V} = (\boldsymbol{v}_{x1}, \boldsymbol{v}_{x2}, \ldots, \boldsymbol{v}_{xd})$, then $\boldsymbol{Y} = \boldsymbol{X}\boldsymbol{V}$ would be the matrix of principal components. Principal component methodology is typically used for dimension reduction counting on the fact that for some j_0, $\alpha_j \approx 0$ for $j \geq j_0$. Thus for $j \geq j_0$ the principal components may be treated as negligible and thus ignored. In the temperature reconstruction model, the PC_1 (first principal component) is being used to reconstruct a time series that is capturing the most variability in the data.

A.3 Numerical Methods and Bias Illustration

With large datasets, a preferred numerical method for finding the eigenvalues is to use a singular value decomposition of the data matrix $\boldsymbol{X}$. The singular value decomposition (SVD) is given by

$$\boldsymbol{X} = \boldsymbol{U}\boldsymbol{D}\boldsymbol{V}^{\mathrm{T}},$$

where $\boldsymbol{D}$ is a diagonal matrix of d singular values and the matrices $\boldsymbol{U}$ $(n \times d)$ and $\boldsymbol{V}$ $(d \times d)$ satisfy $\boldsymbol{U}^{\mathrm{T}}\boldsymbol{U} = \boldsymbol{I}_d$ and $\boldsymbol{V}^{\mathrm{T}}\boldsymbol{V} = \boldsymbol{V}\boldsymbol{V}^{\mathrm{T}} = \boldsymbol{I}_d$. In this case

$$\widehat{\Sigma} = \tfrac{1}{n}(\boldsymbol{U}\boldsymbol{D}\boldsymbol{V}^{\mathrm{T}})^{\mathrm{T}}(\boldsymbol{U}\boldsymbol{D}\boldsymbol{V}^{\mathrm{T}}) = \tfrac{1}{n}\boldsymbol{V}\boldsymbol{D}\boldsymbol{U}^{\mathrm{T}}\boldsymbol{U}\boldsymbol{D}\boldsymbol{V}^{\mathrm{T}} = \tfrac{1}{n}\boldsymbol{V}\boldsymbol{D}^2\boldsymbol{V}^{\mathrm{T}}.$$

The eigenvectors of $\widehat{\Sigma}$ are the right singular vectors of $\boldsymbol{X}$ given by $\boldsymbol{V}$.

In Mann et al. (1998), the study period is partitioned into a reconstruction period 1400-1995 and a training period 1902-1980 in which all the proxy variables are available. The data matrix is centered using the training data rather than the overall means. Because the training period has higher temperatures, this biases the overall data lower for the period 1400-1995, thus inflating the variance. In this case the right singular vectors, $\boldsymbol{V}$, are no longer the eigenvectors.

In general,

$$\widehat{\Sigma} = \frac{1}{n}\boldsymbol{X}^{\mathrm{T}}\boldsymbol{X} - \bar{\boldsymbol{x}}\bar{\boldsymbol{x}}^{\mathrm{T}}$$

where $\bar{\boldsymbol{x}}$ is the $p \times 1$ vector of variable means in $\boldsymbol{X}$ after partial centering. For this $\boldsymbol{X}$ we let the column vectors of the right singular matrix $\boldsymbol{V}$ be $\boldsymbol{v}_1, \boldsymbol{v}_2, \dots, \boldsymbol{v}_d$. Because these form an orthonormal basis, we can find constants, β_i, that satisfy

$$\bar{\boldsymbol{x}} = \sum_{i=1}^{d} \beta_i \boldsymbol{v}_i \text{ so that } \bar{\boldsymbol{x}}\bar{\boldsymbol{x}}^{\mathrm{T}} = \sum_{i=1}^{d} \sum_{j=1}^{d} \beta_i \beta_j \boldsymbol{v}_i \boldsymbol{v}_j^{\mathrm{T}}$$

To see how close the singular values, $\boldsymbol{v}_k$, are to the eigenvectors of $\widehat{\Sigma}$ let us substitute

$$\begin{aligned}
\widehat{\Sigma}\boldsymbol{v}_k &= \left(\frac{1}{n}\boldsymbol{X}^{\mathrm{T}}\boldsymbol{X} - \bar{\boldsymbol{x}}\bar{\boldsymbol{x}}^{\mathrm{T}}\right)\boldsymbol{v}_k \\
&= \frac{1}{n}\boldsymbol{X}^{\mathrm{T}}\boldsymbol{X}\boldsymbol{v}_k - \sum_{i=1}^{d}\sum_{j=1}^{d} \beta_i\beta_j \boldsymbol{v}_i \boldsymbol{v}_j^{\mathrm{T}} \boldsymbol{v}_k \\
&= \frac{1}{n}\boldsymbol{X}^{\mathrm{T}}\boldsymbol{X}\boldsymbol{v}_k - \sum_{i=1}^{d} \beta_i\beta_k \boldsymbol{v}_i \\
&= \frac{1}{n}\boldsymbol{X}^{\mathrm{T}}\boldsymbol{X}\boldsymbol{v}_k - \beta_k \bar{\boldsymbol{x}}.
\end{aligned}$$

Unless $\bar{\boldsymbol{x}}$ happens to be an eigenvector so that $\boldsymbol{v}_k = \bar{\boldsymbol{x}}$ for some k, the singular vectors are not the eigenvector directions. They are biased toward the unsubtracted mean vector $\bar{\boldsymbol{x}}$. Note that if one data vector (proxy variable) $\boldsymbol{x}_k$ happens to have a much larger variance than the others, then the principal components method will attempt to fit that variable causing the eigenvector to align with variable. Similarly the largest component of the mean vector will be the contribution from variable $\boldsymbol{x}_k$ so that the eigenvector and the mean vector will be such that the directions of $\boldsymbol{v}_k$ and $\bar{\boldsymbol{x}}$ tend to align. If $\boldsymbol{v}_k = \bar{\boldsymbol{x}}$, then the associate eigenvalue for $\frac{1}{n}\boldsymbol{X}^{\mathrm{T}}\boldsymbol{X}$ is biased upwards by the amount $\bar{\boldsymbol{x}}^{\mathrm{T}}\bar{\boldsymbol{x}}$. This may be substantial. Thus the first eigenvector of $\boldsymbol{X}^{\mathrm{T}}\boldsymbol{X}$ will be biased towards $\bar{\boldsymbol{x}}$ and its importance overstated by $\bar{\boldsymbol{x}}^{\mathrm{T}}\bar{\boldsymbol{x}}$.

APPENDIX B. Request from the House Committee on Science/Chairman Boehlert

In addition to the effort undertaken by the House Committee on Energy and Commerce, the House Committee on Science also sought to clarify these issues. To this end the House Committee on Science undertook to charge the National Research Council (NRC) of the National Academy of Science (NAS).

House Committee on Science Charge to NAS

The group should, in a clear and concise report issued in a relatively short period of time, answer the following questions:

1. What is the current scientific consensus on the temperature record of the last 1,000 to 2,000 years? What are the main areas of uncertainty and how significant are they?

2. What is the current scientific consensus on the conclusions reached by Drs. Mann, Bradley and Hughes? What are the principal scientific criticisms of their work and how significant are they? Has the information needed to replicate their work been available? Have other scientists been able to replicate their work?

3. How central is the debate over the paleoclimate temperature record to the overall scientific consensus on global climate change (as reflected in previous reports from the Academy)? How central is the work of Drs. Mann, Bradley, and Hughes to the consensus on the temperature record?

NAS/NRC Internal Translation of the Charge to NAS/Atmospheric Science Board

The committee will describe and assess the state of scientific efforts to reconstruct surface temperature records for the Earth over approximately the past 2,000 years. The committee will summarize current scientific information on the temperature record for the past two millennia, describe the main areas of uncertainty and how significant they are, describe the principal methodologies used and any problems with these approaches, and explain how central the debate over the paleoclimate temperature record is to the state of scientific knowledge on global climate change. As part of this effort, the committee will address tasks such as:

1. Describe the proxy records that have been used to estimate surface temperatures for the pre-instrumental period (e.g., tree rings, sediment cores, isotopes in water and ice, biological indicators, indicators from coral formations, geological boreholes, and historical accounts) and evaluate their limitations.

2. Discuss how proxy data can be used to reconstruct surface temperature over different geographical regions and time periods.

3. Assess the various methods employed to combine multiple proxy data to develop large-scale surface temperature reconstructions, the major assumptions associated with each approach, and the uncertainties associated with these methodologies.

4. Comment on the overall accuracy and precision of such reconstructions, relevant data quality and access issues, and future research challenges.

Note: statement of task revised on 03/30/2006. The National Academies is sponsoring this study. The approximate starting date for the project is 01/19/2006. The committee will issue a Final Report in approximately four months.

Note: Although the House Committee on Science initiated the Academy study, the Academy decided not to address the specific questions of the House Committee on Science and decided to focus of the Academy study away from the specific questions and address broader issues.

We attempt here to give answers to the House Committee on Science questions.

- What is the current scientific consensus on the temperature record of the last 1,000 to 2,000 years?

  Ans: There is strong evidence from the instrumented temperature record that temperatures are rising since 1850 and that global warming is a fact. How accurate the reconstructions over the past millennium are is a matter of debate and we do not believe there is a consensus on this issue.

- What are the main areas of uncertainty and how significant are they?

  Ans: The proxy data have many factors encoded in them, one of which is temperature. However, the temperature proxy is confounded with many other factors that have not been teased out including carbon dioxide fertilization effects. The high level of variability in the proxy data as well as the lack of low frequency effects make the reconstructions more problematic than the advocates of these methods would have us believe. In addition the lack of a really substantial stationary, instrumented temperature record handicaps the calibration.

- What is the current scientific consensus on the conclusions reached by Drs. Mann, Bradley and Hughes?

  Ans: Based on the literature we have reviewed, there is no overarching consensus on MBH98/99. As analyzed in our social network, there is a tightly knit group of individuals who passionately believe in their thesis. However, our perception is that this group has a self-reinforcing feedback mechanism and, moreover, the work has been sufficiently politicized that they can hardly reassess their public positions without losing credibility.

- What are the principal scientific criticisms of their work and how significant are they?

  Ans: Our perception is that principal components (statistical) analysis was used incorrectly and, based on this, unsupportable inferences were drawn about the current magnitude of global warming relative to the historical past. We hasten to repeat that the Earth is getting warmer. What does not appear to be true is that the process mechanism is as well understood as some scholars would have us believe. In addition the use of some proxies does not appear to be as carefully managed as we might like.

- Has the information needed to replicate their work been available?

  Ans: In our opinion, the answer is no. As mentioned earlier, there were gaps in MBH98.

- Have other scientists been able to replicate their work?

  Ans: von Storch, Zorita and González-Rouco in their presentation to the NAS Committee report that Bürger et al. (2006) have reproduced the MBH98 results. We have not verified this independently.

- How central is the debate over the paleoclimate temperature record to the overall scientific consensus on global climate change (as reflected in previous reports from the Academy)?

  Ans: In a real sense the paleoclimate results of MBH98/99 are essentially irrelevant to the consensus on climate change. The instrumented temperature record since 1850 clearly indicates an increase in temperature. Whether this is unprecedented in the last millennium seems less clear and to what extent the natural planetary processes can mitigate the excess green-house gas release is unknown. What is more important in our view is real insight into and understanding of the processes of global warming.

- How central is the work of Drs. Mann, Bradley, and Hughes to the consensus on the temperature record?

  Ans: MBH98/99 has been politicized by the IPCC and other public forums and has generated an unfortunate level of consensus in the public and political sectors and has been accepted to a large extent as truth. Within the scholarly community and in certain conservative sectors of the popular press, there is at least some level of skepticism.

APPENDIX C. Summaries of Important Papers Discussed In the Report

Summary of Michael E. Mann's Ph.D. Dissertation, *A Study of Ocean-Atmosphere Interaction and Low-Frequency Variability of the Climate System* (1998)

Dr. Mann attempts to clarify low frequency climate variations so that the effects of other factors (anthropogenic forcing, etc.) on climate can be defined. Dr. Mann's method includes a simplified theoretical model to provide a description of the effects of ocean circulation on climate as well as application of multivariate statistical methodology in the reconstruction of oscillatory low-frequency signals, using proxy and instrumental data. While Dr. Mann states that there is scant robust evidence for periodic climate signals other than the yearly seasonal variations, he notes there does seem to be an oscillatory character in many climatic processes. Dr. Mann attributes this "quasi-oscillatory" climate signal to linear and non-linear feedback mechanisms. Additionally, these signals are marked by defined, finite, decadal to centennial scale variations. Dr. Mann concludes, from the investigation of proxy data, that the most likely source of the climate variations is the coupled ocean-atmosphere processes.

In his analysis, Dr. Mann determines that the traditional approach of climate signal detection, univariate spectral analysis, underestimates multi-decadal variations. He opts for a multivariate method, principal component analysis combined with spectral analysis, for climate signal detection. However, this method also presents with problems, as the distinct principal components present different climate signals and varying decomposition of the associated noise. Dr. Mann attributes this to two consecutive statistical operations with confounding optimization properties. Dr. Mann goes on to examine several different methods of principal component analysis that mitigate these negative effects, but eventually settles on multitaper frequency-domain singular value decomposition, or "MTM-SVD."

MTM-SVD isolates statistically significant oscillations that are correlated with the independent time series. This method is useful in describing spatially-correlated oscillatory signals that have arbitrary spatial relationships in amplitude and phase. Subsequently, this approach can detect standing and traveling patterns in a spatial-temporal dataset as well as account for periodic or aperiodic oscillatory patterns. Dr. Mann contends that this method allows for an accurate climate reconstruction of spatiotemporal patterns immersed in noise.

Using these methods, Dr. Mann found a long-term global warming trend and anomalous atmospheric circulation patterns. These patterns show similarity to a modeled response of climate to increased greenhouse gases. Additionally, Dr. Mann found significant internal 50-100 year oscillations with similar features occurring over several centuries. Similar oscillatory signals have been attributed to variability in the thermohaline circulation and coupled ocean-atmosphere processes in other model simulations. Dr. Mann also found a distinct 10-15 year oscillation in the instrumental data. This evidence of several inter-annual climate signals makes the interpretation of data more complicated in terms of a simple linear dynamical mechanism.

Next, Dr. Mann proposes an alternative method for modeling the ocean circulation variations with respect to climate. He takes the equations governing ocean circulation and subdivides them into two sections. Dr. Mann studies these two dominant modes of circulation, the gyre and mean meridional overturning circulations, separately as well as dynamically coupled. He notes that this type of modeling is only relevant when the non-uniform effects for each section are properly taken into account. After deriving these two components, Dr. Mann introduces some approximations and simplifications to allow for their coupling. From this, Dr. Mann creates what he believes to be a reasonably faithful description of large-scale ocean circulation, temperature and salinity fields of the world's oceans. However, he notes one caveat, that this model is very sensitive to dynamics created when the gyre circulation is not taken into account. When these gyre-scale processes are absent from the model, a 200-300 year mode of ocean variability is clearly defined, taken by Dr. Mann to be the linear mode of the variability in the meridional overturning circulation. Dr. Mann estimates the effects of the ocean circulation on the atmosphere by parameterizing the modeled response of the atmosphere to sea surface temperature variations. When gyre-scale processes are accounted for, a 70-to-100 year instability is present. Dr. Mann interprets this variation as an oceanic delayed oscillator mechanism caused by changes in the meridional overturning, which subsequently causes changes in the near-surface salinity and heat advection. These effects in turn dampen the meridional overturning circulation before it can become large-scale. Dr. Mann contends that the results of the model study underscore possible interactions between these two major circulation processes and the nature of decadal to century scale variability.

Dr. Mann contends that his work shows strong evidence for the existence of 50-100 year scale oscillations centered in the North Atlantic, persistent over several centuries. This is suggestive (but not conclusive) of damped oscillations in the climate system. Dr. Mann also contends that an atmospheric response to both of these major circulation processes is identified, corresponding closely to sea level pressure variations. Lastly, Dr. Mann defends the robustness of this simplified model, stating that the observed climate variability is consistent with many complex climatic mechanisms not included in this study. However, with more long-term proxy data and more large-scale climate reconstructions, the application of the signal detection methods described here will provide further insight into the nature of these decadal to century scale climate signals.

Summary of *Global-scale temperature Patterns and Climate Forcing Over the Past Six Centuries* by Michael Mann, Raymond Bradley, and Malcolm Hughes (1998)

In an attempt to understand long-term global climate variation, Mann et al. use a widely distributed set of proxy and instrumental climate indicators to reconstruct global temperature patterns over the past 500 years. Using this data, they attempt to estimate the relationship between global temperature changes and variations in volcanic aerosols, solar irradiance, and greenhouse gas concentrations.

The data consisted of a multiproxy network. In this case proxy is a time series constructed using data from various sources, such as tree ring measurements, ice cores, ice melts, and historical records. Overall the network includes 112 proxies, and each series has been formatted into annual mean anomalies relative to the reference period used for this data, 1902-1980. Certain tree-ring datasets have been represented by a small number of leading principal components. The dendroclimatic data has also been carefully reviewed to ensure standardization and sizeable segment lengths. Although the data network covers large portions of the globe, there is only enough reliable information to conduct a spatial analysis of the Northern Hemisphere.

Because of the heterogeneity of the information available, Mann et al. calibrated the datasets by first decomposing the 20th century instrumental data into its dominant patterns of variability using principal component analysis, and subsequently calibrating the individual climate proxy indicators against the time histories of these distinct patterns during their mutual interval of overlap. Included in this calibration approach are three assumptions: 1) the indicators in our network are linearly related to one or more of the instrumental training patterns, 2) a relatively sparse, but widely distributed sampling of long proxy and instrumental records may measure the small number of degrees of freedom in climate patterns at interannual and longer timescales, and 3) patterns of variability captured by the multiproxy network have analogs in the patterns they find in the shorter instrumental data. In their principal component analysis (PCA), Mann et al. isolated a small number of dominant patterns of variability, otherwise labeled 'empirical eigenvectors'. Each of these patterns or eigenvectors has a characteristic spatial pattern and a pattern evolving over time (also referred to as the 'principal component'). These eigenvectors are ranked according to the percentage of variance they describe. The first five eigenvectors describe 93% of the total variance. Each of the indicators in this study was calibrated using these five eigenvectors.

The temperature reconstructions derived using all indicators and the most optimal eigenvector subsets show long term trends including pronounced cold periods during the mid-seventeenth and mid-nineteenth centuries and warmer intervals during the mid-sixteenth and late eighteenth centuries. Based on their methods, almost all of the years before the twentieth century exhibit temperatures well below the twentieth century mean temperature. Taking into account the uncertainties in their reconstruction, they find that the years 1990, 1995, and 1997 each show anomalies that are greater than any other year back to 1400, with roughly a 99.7% level of certainty.

Other general circulation and energy-balance model experiments, including some statistical comparisons of twentieth century global temperatures with forcing series, suggest that although both solar and greenhouse gas forcings play some role in explaining twentieth century climate trends, greenhouse gases appear to play an increasingly dominant role during this century. Additionally, it is hoped that as larger numbers of high quality proxy reconstructions become available it may be possible to create a more globally representative multiproxy data network for further study.

Summary of *Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations* Mann et al. (1999)

Estimates of climate variability during past centuries rely upon indirect "proxy" indicators – natural archives that record past climate variations: tree rings, sediments, ice cores and corals. MBH98 used these temperature proxies to reconstruct yearly global surface temperature patterns back to CE 1400. In this article, Mann et al. attempt to reconstruct global surface temperature patterns prior to 1400 because it is surmised that temperatures were warmer even before the period reconstructed in MBH98. However, in order to recreate these temperature patterns, the same methodology employed in MBH98 was applied to an even sparser proxy data network available prior to CE 1400. Only 12 viable indicators are available for this time period. Because only a small number of indicators are available in regions where the primary pattern of hemispheric mean temperature variation has significant amplitude, these indicators have a particularly important role. Just as in MBH98, the calibration procedure for these 12 indicators invokes two assumptions: first, that a linear relationship exists between proxy climate indicators and some combination of large-scale temperature patterns and second, that patterns of surface temperature in the past can be suitably described in terms of some linear combinations of the dominant present-day surface temperature patterns. The calibration/verification statistics for reconstructions based on the 12 indicators are somewhat degraded compared to those for the post CE 1400 period. The explained variance in the MBH98 data (post-1400 AD) was between 42% and 51%, whereas the explained variance among these 12 indicators is between 34% and 39%. Furthermore, the first principal component of the ITRDB (International Tree Ring Data Bank) data in this analysis is the only one of these series that exhibits a significant correlation with time history of the dominant temperature pattern of the 1902-1980-calibration period. If this indicator is omitted, positive calibration/variance scores cannot be obtained for the Northern Hemisphere (NH) series. Thus, ITRDB PC1 is the most meaningful component in resolving hemispheric temperature trends. The assumption that this relationship is consistent with time requires closer study and as such a more widespread network of proxy indicators will be required for more confident inferences.

The reconstructed NH series indicates a cooling period prior to industrialization, possibly driven by astronomical forcing[1], which is thought to have driven long term temperatures down since the mid-Holocene period. In addition, significant long-term climate variability may be associated with solar irradiance variations. Our reconstruction supports the notion that warmer hemispheric conditions took place early in the millennium followed by a long period of cooling beginning in the 14th century, which can be viewed as the initial onset of the Little Ice Age. However, even the warmer intervals in our reconstruction pale in comparison with modern (mid-to-late 20th century) temperatures. The data still upholds the conclusion that the 1990s was likely the hottest decade and that 1998 was likely the hottest year of the millennium. However, without more widespread high-resolution data, further conclusions cannot be drawn in regard to the spatial and temporal details of climate change in the past millennium and beyond.

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[1] Astronomical forcing refers to the effect on climate of changes in the tilt and the shape of the orbit of the Earth.

Summary of *Global Temperature Patterns in Past Centuries: An Interactive Presentation* by Michael Mann, Ed Gille, Raymond Bradley et al. (2000)

This paper expands on earlier work by the same authors (*Global-scale temperature Patterns and Climate Forcing Over the Past Six Centuries* by Mann et al.), which used multiproxy networks of indirect climate variability indicators, such as tree-ring measurements, ice cores, coral growth, etc., to reconstruct climate variability and temperatures over the past centuries. In subsequent papers, Mann et al. expanded the reconstruction to Northern Hemisphere (NH) temperature variations over the past millennium, to examine ENSO-scale patterns of climate variability during past centuries, to compare observed patterns of climate variability in the Atlantic, and to assess the relationship between global patterns of climate variation and particular regional patterns. Most recently, Mann et al. have made available for the first time seasonally resolved versions of global temperature surface patterns in an interactive format, allowing users to select specific spatial regions or time periods of interest. Details of the data and methods involved are discussed in Mann et al. (1998) and (1999).

The data consisted of a multiproxy network. In this case proxy is a time series constructed using data from various sources, such as tree ring measurements, ice cores, ice melts, and historical records. Overall the network includes 112 proxies, and each series has been formatted into annual mean anomalies relative to the reference period used for this data, 1902-1980. Certain tree-ring datasets have been represented by a small number of leading principal components. The dendroclimatic data has also been carefully reviewed to ensure standardization and sizeable segment lengths. Although the data network covers large portions of the globe, there is only enough reliable information to conduct a spatial analysis of the Northern Hemisphere.

Because of the heterogeneity of the information available, Mann et al. calibrated the datasets by first decomposing the 20th century instrumental data into its dominant patterns of variability using principal component analysis, and subsequently calibrating the individual climate proxy indicators against the time histories of these distinct patterns during their mutual interval of overlap. Included in this calibration approach are three assumptions: 1) the indicators in our network are linearly related to one or more of the instrumental training patterns, 2) a relatively sparse but widely distributed sampling of long proxy and instrumental records may measure the small number of degrees of freedom in climate patterns at interannual and longer timescales., and 3) patterns of variability captured by the multiproxy network have analogues in the patterns we resolve in the shorter instrumental data. In their principal component analysis (PCA), Mann et al. isolated a small number of dominant patterns of variability, otherwise labeled 'empirical eigenvectors'. Each of these patterns or eigenvectors has a characteristic spatial pattern and a pattern evolving over time (also referred to as the 'principal component'). These eigenvectors are ranked according to the percentage of variance they describe. The first five eigenvectors describe 93% of the total variance. Each of the indicators in this study was calibrated using these five eigenvectors.

The most recent temperature reconstructions indicate that 1998 (as opposed to 1990, 1995 and 1997 as previously proposed in Mann et al. 1998, 1999) was most likely the warmest year of at least the past millennium. There are also distinct temperature trends for the Northern and Southern hemispheres. While both hemispheres have similar trends, the coldness of the 19th century appears to be somewhat more pronounced in the Northern hemisphere. Additionally, evidence suggests that the post-1850 warming was more dramatic at higher latitudes relative to lower latitudes due to larger positive feedbacks at high latitudes. The annual mean temperature trends at higher latitudes are seen to be greater than the hemispheric trends themselves. In contrast, the tropical band shows less change than the entire Northern Hemisphere series.

Mann et al. also provide yearly global temperature maps for annual mean, boreal cold season, and warm season for the reconstructed temperature fields from 1730 to 1980, the raw temperature data from 1902-1993 (used for calibration) and the sparse raw "verification" data from 1854 to 1901 (used for cross-validation). Users can investigate spatial patterns and time histories of this global temperature data at http://www.ngdc.noaa.gov/cgi-bin/paleo/mannplot2.pl.

The statistical relationship between variations in the NH mean temperature and estimates of the histories of solar, greenhouse gas and volcanic forcings suggest that while the natural forcings play a role, only greenhouse gas forcing alone can explain the unusual warmth of the past few decades. Mann et al. also examined the sensitivity surrounding these forcings and found that when physically reasonable lags are incorporated into the attribution analysis there is evidence of even greater statistical relationships with particular forcings. At the physical lag of one year, the relationship between temperature variations and volcanic forcing is slightly more consistent. At the physical lag of 10-15 years the relationship between greenhouse gas increases and increasing temperatures is considerably more significant, while the relationship with solar irradiation is less significant. Thus, there is significant evidence that recent anthropogenic activities are contributing to the recent warming.

It is clear that the primary limitations of large-scale proxy-based reconstruction in past centuries, both temporally and spatially, reside in the increasingly sparse nature of available proxy networks available to provide reliable, past climate information. Arduous efforts are needed to extend such networks in space and time to the point where significant improvements will be possible in order to gain a more empirical understanding of climate variations during the past millennium.

Summary of *Ocean Observations and the Climate Forecast Problem* by Carl Wunsch (2002)

Due to the recent importance of studying climate change, it has become apparent that there are significant problems in observing the ocean and its climate. Much of the problem is technical, but there is also the matter of culture and misapprehension. Many in the field of meteorology continue to have an antiquated and misleading perception of the ocean circulation. In his article, Wunsch outlines the reasons for many of the problems in observing the ocean.

Since the opacity of the ocean has made it difficult to observe until recent technological innovations, and the cost of supporting oceanographic ships is prohibitive, time series of oceanic variables were almost nonexistent. The only variables relatively easy to measure and interpret were properties such as temperature, salinity and oxygen. Since these properties are particularly stable, the static picture of ocean circulation became the predominant view. However, with the advent of modern electronics, obtaining time series of oceanographic data became easier. After years of literature and data on the subject, it became clear that the ocean is actually quite turbulent under the surface and that few, if any, elements of ocean circulation are truly steady.

There exists a large-scale oceanic circulation that appears to be steady over decades, but is thought to be slowly changing everywhere in ways not yet known. However, the current understanding of how oceanic circulation will affect the climate is actually very narrow. Additionally, the problem is further compounded by the fact that models have become so sophisticated and interesting, it is tempting to assume they must be skillful. Most papers written on the subject of oceanographic models give little or no guidance to the reader as to the actual expected skill of the model. This type of research begs the question, is it really plausible that a 4° or 1° ocean model can be integrated with skill for 1000 years? The magnitude of the error in these models is enormous when integrated over such a long time period. The evidence for the skillfulness of similar models is scant.

The assumption that the oceanic system is much simpler than it actually is leads to a corruption of the entire literature. Readers of paleoclimate papers will notice that extraordinarily complicated and far-reaching changes in the climate system are often reduced to simple assertions about how the "global conveyor" changed. One might also be suspicious of "concrete" evidence of atmospheric modeling because atmospheric modeling must be equally if not more difficult than modeling the ocean. In order to begin to make any kind of model, years of observation with oceanographic satellites are needed. Most of these satellites are not currently regarded as operational. Of primary concern is to insure that everyone understands the problem and to recognize the great influence past assumptions exercise over future necessity.

Summary of *Corrections to the Mann et al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series* by Stephen McIntyre and Ross McKitrick (MM03) (2003)

In their paper, *Corrections to the Mann et al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series,* (hereafter referred to as MM03), McIntyre and McKitrick assess the methodology and results of the widely referenced Mann, Bradley, and Hughes paper, *Global Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries* (hereafter referred to as MBH98). In MBH98 the authors attempted to reconstruct a temperature history of the Northern Hemisphere for the period 1400-1980. Their result was a "hockey stick" shaped graph, from which they concluded that the temperatures of the late 20th century were unprecedented and that 1990-2000 was likely the hottest decade in the millennium, and 1998 was likely the hottest year in the millennium. These findings have been prominent in the discussion on global climate change and in subsequent policy discussions. MM03 attempts to recreate the research in MBH98 in order to prove or disprove their findings.

In the course of the research reproduction, McIntyre and McKitrick found errors in the statistical methodology of MBH98. Primarily, MM03 found that the creation of the proxy database itself held serious errors. In this context proxy denotes one of the 112 physical measurements used that can serve as an indicator of climatic conditions, including temperature. Examples of proxies include tree measurements, ice cores, and coral calcification rates. The time series created from these measurements form the basis of the MBH98 study.

MM03 claimed the following errors in the MBH98 proxy database:

1. unjustified truncation of three time series
2. copying 1980 values from one series onto another
3. displacement of 18 series to one year earlier than apparently intended
4. Statistically unjustified extrapolations or interpolations to cover missing entries in 19 series
5. geographical mislocations and missing identifiers of location
6. inconsistent use of seasonal temperature data where annual data is available
7. obsolete data in at least 24 series, some of which may have been obsolete at the time of the MBH98 study
8. listing of unused proxies
9. incorrect calculation on all 28 tree ring principal components.

Having accounted for the major errors, MM03 reconstructed the temperature history. Using the MBH98 methodology, they were able to accurately reproduce the "hockey stick" shaped graph in the MBH98 findings. Still using the same basic methodology, MM03 prepared the data with improved quality control, including using the most recent data and collating it correctly. The result was a northern hemisphere temperature reconstruction that takes on a different shape in which the temperature index peaks at around 1450 AD, near the earliest measured point on the graph. MM03 concluded that the errors in MBH98 make the data unreliable and obsolete such that it does not support their end conclusions.

Summary of *Global Surface Temperature over the Past Two Millennia* by Michael Mann and Philip Jones (2003)

Mann and Jones present their reconstructions of Northern and Southern Hemisphere mean surface temperature over the past two millennia based on high-resolution (annually or decadally scaled) proxies. For the Northern Hemisphere, they use previous temperature reconstructions from eight distinct regions based on 23 individual proxy records and for the Southern Hemisphere, they use temperature reconstructions from five distinct regions. Composites were performed separately for each hemisphere, based on available regional temperature records. Each regional temperature record was standardized by removal of the long-term mean and division by the standard deviation, after decadal smoothing. The composites were weighted combinations of the standardized proxy series, weighted by size of region and estimated reliability of the climate signal in the proxy. Proxy records exhibiting negative or approximately zero local correlations were eliminated from the study. Each composite was also standardized to have the same mean and decadal standard deviation as the target instrumental series over the period of common overlap.

The Northern Hemisphere reconstruction was observed to be largely insensitive to elimination of shorter proxy records or to the weighting of the proxy series, suggesting a significant robustness. The reconstruction is consistent with previous Mann reconstructions, in that the warmth in the late 20th century is unprecedented. Larger uncertainties in the Southern Hemisphere reconstruction preclude a similar conclusion for this. Increased quality and quantity of Southern Hemisphere proxy records are needed to decrease the current uncertainties surrounding the reconstruction and definitively make conclusions about the climate variability.

Summary of *Reconstructing Past Climate from Noisy Data* by Hans von Storch et al. (2004)

While attempting to measure anthropogenic effects on the earth's climate, it is necessary to create a reconstruction of past climate variations. Most studies have identified varying warm values in the 11th and 12th centuries followed by secular cooling periods in the mid-16th, 17th and early 19th centuries. These cooler intervals were followed by warming that is still experienced today. The amplitude of these preindustrial variations is debated, although the most notable study on the subject and the most quoted, Mann et al. 1998 (MBH98), as well as the Intergovernmental Panel on Climate Change (IPCC), report that these variations were of small amplitude. However, recent studies have suggested that centennial variations may have been larger than previously thought. This study uses a coupled atmosphere-ocean model simulation of the past millennia as a surrogate climate to test the reconstruction method of MBH98.

Using this model as a virtual world to determine the skill of regression-based reconstruction models like MBH98, von Storch et al. found that the model is reasonably skilled at reproducing short-term variations but substantial underestimation occurs in the long-term estimations. On an inter-annual scale, the reconstruction has a calibration reduction-of-error statistic of .7 for perfect pseudo-proxies and .3 for pseudo-proxies with a higher degree of noise. However, only 20% of the 100-year variability is recovered when the noise level is approximately 50%. Similar results were obtained using the third Hadley Centre coupled model (HadCM3), indicating the results are not dependent on the model used.

Von Storch et al. also tested a number of other hypotheses. They found that including more instrumental data in the proxies does not improve results, expanding the proxy set in sparse areas improved results marginally, and that expanding the range of temperature variability present in the pseudo-proxies greatly improves the results. Additionally, von Storch et al. questioned the validity of linear regression models in general in estimating climate. Using pseudo-proxies to estimate local temperatures which were then spatially averaged to derive a Northern Hemisphere temperature, they found similar problems that occur in MBH98: underestimation of low-frequency variability for a given amount of noise. The authors conclude that climate simulations of the past millennium are burdened by model limitations and uncertainties in external forcing and therefore the output must be considered with care. Additionally, the linear regression methods as used in MBH98, suffer from marked losses of centennial and multidecadal variations.

Summary of *The M&M Critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications* by Stephen McIntyre and Ross McKitrick (MM05a) (2005a)

In an extension of their 2003 paper (*Corrections to the Mann et. Al. (1998) Proxy Database and Northern Hemispheric Average Temperature Series),* McIntyre and McKitrick further detail their critique of Mann et. al. (1998) and respond to its subsequent update Mann et. al. (1999). In response to McIntyre and McKitrick (2003), Mann et. al. published new information regarding their original research that MM03 attempted to replicate. While the new information did not include the source code used to generate the original results, it did include an extensive archive of data and supplementary information on the methods at the University of Virginia FTP site.

In their article, M&M indicate that the individual data series (proxies) used to reconstruct the temperature index are important, and that errors within these series do not get washed out in a multi-proxy study. Specifically, MM05a found that the differences in MBH98 and MM03 can be almost fully reconciled through the variations in handling of two distinct series, the Gaspe "northern treeline" series and the first principal component (PC1) from the North American proxy roster (NOAMER). In MBH98, the first four years of both of these series were extrapolated. The extrapolation has the effect of depressing early 15th century results, and was not disclosed by Mann et al. until a later paper, Mann et al. (2004). The underlying dataset that was subject to extrapolation also fails to meet the data quality standards described by Mann et al. elsewhere in the paper.

In the MBH98 methodology, they used a principal component analysis, which they reported to be conventional or centered. However, in further disclosure of information on the UVA FTP site, it has been determined that the principal component analysis was not actually centered. In fact the mean used in their calculations is the 1902-1980 mean, but it was applied to the period 1400-1980. The effect of de-centering the mean is a persistent "hockey stick" shaped PC1, even when layered with persistent red noise. It follows from this shape that the climate of the late 20th century was unprecedented. Because the original code is in FORTRAN, which takes much more programming to run statistical processes than modern software such as R, it is very possible that this is due to a programming error, although Mann et al. have not admitted to any such error.

In the MBH98 de-centered principal component calculation, a group of twenty primarily bristlecone pine sites govern the first principal component. Fourteen of these chronologies account for over 93% variance in the PC1 and 38% of the total variance. The effect is that it omits the influence of the other 56 proxies in the network. In a centered version of the data, the influence of the bristlecone pine drops to the fourth principal component, where it accounts for 8% of the total variance. The MM03 results are obtained if the first two NOAMER principal components are used. The MBH98 results can be obtained if the NOAMER network is expanded to five principal components. Subsequently, their conclusion about the climate of the late 20th century is contingent upon including low-order principal components that only account for 8% of the variance of one proxy roster. Furthermore, the MM03 results occur even in a decentered PC calculation, regardless of the presence of PC4, if the bristlecone pine sites are excluded.

In the Gaspe "northern treeline" series, MM05a found that the MBH98 results occur under three conditions: 1) the series must be used as an individual proxy; 2) the series must contain the portion of the series that relies only on one or two trees for data; and 3) it must contain the ad-hoc extrapolation of the first four years of the chronology. Under all other conditions, including using an archived version of the series without extrapolation, MM03 type results occur.

MM05a also addresses the MBH98 claims of robustness in their findings. The sensitivity of the 15th century results to slight variations in the data and method of two individual series show a fundamental instability of the results that flatly contradicts the language used in MBH98 and in Mann et al. (2000) where it states "...whether we use all data, exclude tree rings, or base a reconstruction only on tree rings, has no significant effect on the form of the reconstruction for the period in question..." Additionally, MM05a notes much of the specialist literature raises questions about these indicators and at the least these questions should be resolved before using these two series as temperature proxies, much less as uniquely accurate stenographs of the world's temperature history.

In response to MM03, Mann et al. wrote several critiques that appeared in *Nature* magazine as letters and as separate articles. The Mann et al. (2004) paper argued that the MM03 use of centered principal components calculations amounted to an "effective omission" of the 70 sites of the North American network. However, the methodology used omits only one of the 22 series. A calculation like this should be robust enough that it is relatively insensitive to the removal of one series. Also, "effective omission" is more descriptive of the MBH98 de-centering method, which uses 14 bristlecone sites to account for over 99% of explained variance.

In another response, Mann et al. claim that the PC series are linear combinations of the proxies and as such cannot produce a trend that is not already present in the underlying data. However, the effect of de-centering the mean in PC analysis is that it preferentially selects series with hockey-stick shapes and it is this over weighting that yields a pattern not representative of the underlying data. Additionally, Mann et al. responded to the MM03 critique of the bristlecone pine, which pointed out that the bristlecone pine had no established linear response to temperature and as such was not a reliable temperature indicator. Mann et al. responded by stating that their indicators were linearly related to one or more instrumental training patterns, not local temperatures. Thus, the use of the bristlecone pine series as a temperature indicator may not be valid.

The authors of MM05 concluded that the various errors and adverse calculations that were not disclosed exhibit the limitations of the peer review process. They also note the limited due diligence of paleoclimate journal peer review and that it would have been prudent to have checked the MBH98 data and methods against original data before accepting the findings as the main endorsement of the Intergovernmental Panel on Climate Change.

Summary of *Hockey sticks, principal components, and spurious significance* by Stephen McIntyre and Ross McKitrick (2005b)

In their critique of *Global-scale temperature Patterns and Climate Forcing Over the Past Six Centuries* (MBH98) by Mann et al., McIntyre and McKitrick (M&M) note several errors in the methodology and subsequent conclusions made by Mann et al. First, M&M discuss the incorrect usage of principal component analysis (PCA) in MBH98. A conventional PC algorithm centers the data by subtracting the column means of the underlying series. For the 1400 to 1450 data series, the FORTRAN code contains an unusual data transformation prior to the PC calculation, which was never reported in print. Each tree ring series was transformed by subtracting the 1902-1980 mean and then dividing by the 1902-1980 standard deviation and dividing again by the standard deviation of the residuals from fitting a linear trend in the 1902-1980 period. For PCA, if the 1902-1980 mean is close to the 1400-1980 mean, then there will be very little impact from this linear transformation. However, if the means differ, then the explained series variance is inflated. Since PCA gives more weight to series that have more explained variance, the effect is preference for the 'hockey stick' shape seen in Mann et al.. This 'hockey stick' shape supports the conclusions that climatic conditions in the late twentieth century are anomalies.

M&M also ran a Monte Carlo Simulation on 70 of the stationary proxy data series. When applying the linear transformation described above that was found in MBH98, nearly every simulation yielded first principal components (PC1) with a 'hockey stick' shape. Without this transformation, the 'hockey stick' shape appeared in the PC1 only 15.3% of the time. Additionally, the MBH98 method creates a PC1 that is dominated by bristlecone pine and foxtail pine tree ring series (both closely related species). Out of the 70 sites in the network, 93% of the variance in the MBH98 PC1 is accounted for by only 15 bristlecone and foxtail pine sites, all with data collected by one man, Donald Graybill. Without the transformation, these sites have an explained variance of less than 8%. The substantially reduced share of explained variance coupled with the omission of virtually every species other than bristlecone and foxtail pine, argues strongly against interpreting it as the dominant component of variance in the North American network. There is also evidence present in other articles calling the reliability of bristlecone pines as an effective temperature proxy into question.

M&M also evaluated the MBH98 usage of the Reduction of Error statistic in place of the more reliable and widely used Monte Carlo Model to establish significant benchmarks. By using the Monte Carlo Model, M&M found that a more accurate significance level for the MBH98 procedures is .59, as opposed to the level of 0.0 reported in the original study. A guard against spurious RE significance is to examine other statistics, such as the R^2 and CE statistics. However, MBH98 did not report any additional statistics for the controversial 15th century period. The M&M calculations indicate that these values for the 15th century section of the temperature reconstruction are not significant, thereby refuting the conclusions made by MBH98.

Summary of *Highly Variable Northern Hemisphere Temperatures Reconstructed from Low- and High-Resolution Proxy Data* by Anders Moberg et al. (2005)

In their study, Moberg et al. reconstruct a climate history for the past 2,000 years using low resolution proxies (proxies that provide climate information at multi-centennial timescales, such as ocean sediment cores) and high resolution proxies (proxies that provide climate information on a decadal scale, such as tree rings). Due to the high profile of high-resolution proxies in reconstructions, mostly from Mann et al. 1998, views have been expressed that only tree ring and other high resolution data are useful for quantitative large scale temperature reconstructions. However, tree ring data has a well documented unreliability in reproducing multi-centennial temperature variability. By using low-resolution data for multi-centennial information combined with high-resolution data for decadal information, the most unreliable timescales for each proxy can be avoided.

The dataset used for this study was limited since proxies were required that dated back 2,000 years. Seven tree-ring series and eleven low-resolution proxy series were used. To obtain a reconstruction covering the complete range of timescales Moberg et al. created a wavelet transform to ensure tree-ring records contribute only to timescales less than 80 years and all low-resolution proxies contribute only to longer timescales. To calibrate the reconstruction, its mean value and variance were adjusted to agree with the instrumental record of Northern Hemisphere annual mean temperatures in the overlapping period 1856-1979.

The reconstruction indicates two warm peaks around A.D. 1000 and 1100 and pronounced cold periods in the 16th and 17th centuries. The peaks in medieval times are comparable to those of the 20th century, although warmth seen in post-1990 seems to be unprecedented. Reconstructions of the temporal evolution of warming variables (volcanic aerosols, solar irradiance and greenhouse gases) have been used to drive simple energy balance climate models as well as fully coupled atmosphere-ocean general circulation models. Moberg et al. note that the Northern Hemispheric temperature series obtained from such an experiment with the coupled model ECHO-G bears a strong qualitative similarity to their reconstruction. This supports the case of a pronounced hemispheric low-frequency temperature variability resulting from the climate's response to natural changes in radioactive forcing.

There are notable differences in the Moberg et al. reconstruction and that of Mann et al. 1998. While there is a large amount of data in common between the two reconstructions, Mann et al. combined tree-ring data with decadally resolved proxies without any separate treatment at different timescales. Additionally, this study's dataset contains centennially resolved data from the oceans while Mann et al. used only annually or decadally resolved data from continents or locations near the coast. Mann et al. also used a different calibration method (regression versus variance scaling as in this study).

Further study in the process of weighting different timescales and spatial representation of the data should be conducted to see which method most accurately depicts past climate variability. This study finds no evidence for any earlier periods in the past two millennia with warmer conditions than the post-1990 period. However, natural multi-centennial climate variability, especially as a response to solar irradiance, may be larger than previously thought. This does not imply that global warming has been caused by natural factors alone, but that there is a need to improve scenarios for future climate change by also including forced natural variability.

Summary of *Testing the Fidelity of Methods Used in Proxy-Based Reconstructions of Past Climate* by Michael Mann, Scott Rutherford, Eugene Wahl and Caspar Ammann (2005)

In this article Mann et al. examine two prominent methods in historical climate reconstruction, Climate Field Reconstruction (CFR) and the Composite-Plus Scale (CPS). The former combines several different proxy[2] records in order to reconstruct underlying patterns of past climate change. The latter combines many different proxy series (such as tree ring series, ice core series, etc.) and scales the resulting composite against a target series (i.e. the Northern Hemisphere) that is measured instrumentally. In order to assess both methods, Mann et al. used climate simulation to create a known climate record. They then layered the model with the typical noise associated with real-world uncertainties found in actual proxies. Thus, Mann et al. created pseudo proxies that they could use to test the two methods of climate reconstruction. They constructed three distinct networks of pseudo proxies, each with attributes similar to actual proxy networks used in past CFR and CPS studies.

Following the standard CPS procedure, each pseudo proxy was smoothed by decade and standardized. The weighted composite of these proxies was then scaled to have the same mean and standard deviation as the actual Northern Hemisphere series. Using different levels of the signal-to-noise ratio (SNR) (relative amplitudes of noise variance), Mann et al. evaluated the effectiveness of each method. In CPS experiments, the results most closely resembled those obtained from actual proxies for SNR=1.0. The lower the SNR level (.25 and .5 were also measured), the lower the skill of reconstruction. Additionally, when SNR=1.0, the CPS method was found to be relatively insensitive to the length of the calibration interval. Mann et al. found that in general, CPS or regression based methods employing a short calibration period are likely to underestimate long-term variability.

Mann et al.'s implementation of the CFR approach makes use of the regularized expectation maximization (RegEM), which is similar to Principal Component Analysis (PCA), but it employs estimates of data covariances in iterations. Mann et al. tested three types of this method: the straight application of RegEM, a "hybrid frequency-domain calibration" approach and a stepwise version of RegEM. All three of these methods yielded similar results in the study. Similar to CPS, Mann et al. found that when SNR=1.0, this method yielded a similar resolved variance and it was relatively insensitive to the calibration period. However, this method yielded a moderately more skillful reconstruction with a long calibration period. Additionally, the CFR method appears to systematically underestimate the amplitude of the larger volcanic cooling events, most likely because of the small number of volcanic events present in the calibration interval.

In general, Mann et al. found no evidence that real-world proxy-based temperature reconstructions are likely to suffer from any systematic underestimate of low-frequency variability. Their findings also suggest that both of these standard methods, CPS and

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[2] In this case, proxy refers to a time series of indicators such as tree rings, ice cores, and coral.

CFR, are likely to provide a faithful estimate of actual long-term hemispheric temperature histories, within estimated uncertainties.

Summary of *Low-Frequency Signals in Long Tree-Ring Chronologies for Reconstructing Past Temperature Variability* by Jan Esper, Edward Cook, and Fritz Schweingruber (2005)

In this article Esper et al. address the debate revolving around the reliability of tree-ring records as substantial basis of temperature reconstruction before the 17th century. The authors' present analysis of centuries-long ring-width trends in 1205 radial tree-ring series from 14 high-elevation and middle-to-high latitude sites distributed over a large part of the Northern Hemisphere extratropics. Esper et al. looked at growth trends in tree ring proxies by analyzing individual raw ring-width measurements using Regional Curve Standardization (RCS) methods. Successful use of the RCS method usually requires a large number of ring-width series because the method of detrending is not based on any explicit curve fitting to the individual series, but rather over series of a similar region. However, the series are further broken down into two groups, those that age linearly and those with age trends that are non-linear.

In each of these groups, the smoothed regional curves were estimated from the averaged biological age-aligned data. The resulting tree ring indices were then averaged into linear and non-linear mean value functions to produce two nearly independent tree-ring chronologies covering the years 800-1990. Each of these chronologies showed evidence of above average temperatures during the Medieval Warming Period (900-1300), below average temperatures during the Little Ice Age (1200-1850), and large-scale warming after 1850, consistent with instrumental temperature records. Overall, these results demonstrate that properly selected and processed tree-ring records can preserve such long time-scale climate variability.

Additionally, using RCS methods, climate variability of the Medieval Warming Period (MWP) can be reconstructed, and it approaches the magnitude of 20th-century warming in the Northern Hemisphere up to 1990. Consistent with other analyses of the MWP, it appears to be more temporally variable than the warming trend of the past century. Analysis also supports that the warmest period of the MWP may have begun in the early 900s, with the warmest interval being from 950 to 1045 AD. This finding suggests that past comparisons of the MWP with the 20th-century warming may not have included all of the MWP, especially its warmest period.

Summary of *Are Multiproxy Climate Reconstructions Robust?* By Gerd Bürger and Ulrich Cubasch (2005)

Bürger and Cubasch review the robustness of multiproxy climate reconstructions, especially with regard to Mann et al. 1998 (MBH98), a prominent and widely discussed paper on the topic. The MBH98 reconstruction applies an inverse regression between a set of multiproxies on one hand and the dominant temperature principal components (PCs) on the other. The sparse availability of proxies prior to 1450 is accounted for by estimating the regression for seven successive time periods. Bürger and Cubasch skip this last step in their approximation of the MBH98 settings. In this study, they use the proxies available for the period 1400-1450 (which includes 18 tree ring and ice core proxies). Using the 1902-1980 as the calibration period, an empirical model is fitted and applied to the full proxy record. Before estimating the regression model, the proxies undergo a PC transformation, a measure against collinearity, which can inflate the model error.

Bürger and Cubasch found 64 variants of reconstructed millennial Northern Hemisphere temperatures. The spread about the MBH98 is immense, especially around the years 1450, 1650, and 1850. There is no evidence that one variation should be chosen over the others and even the variant with the best reduction of error statistic (79%) is the variant that most strongly deviates from MBH98. When the setting was moved to AD 1600 instead of 1400, the spread is still quite large in the early part of the reconstruction, even though more proxies are available.

Fundamental to all proxy inferences is the assumption that the variations in the proxy are related to the temperature and somewhat uniformly. However, the results of this study do not give such a relationship, at least not one that is robust. Bürger and Cubasch could not find one criterion solely responsible for the spread of variants, but it is possible that a significant source of uncertainty could be the scale mismatch between the full millennial and the calibrating proxy variations. In that case, the regression model leaves its general domain of validity and is applied in an extrapolative manner. The further the estimated regression laws are extrapolated the less robust the method is. This error growth is especially critical for parameter-insensitive, multi-proxy climate field reconstructions of the MBH98 type. In order to salvage such a method, there must be a mathematical derivation of the model error and more sophisticated regularization schemes that can minimize the error.

Summary of *Proxy-Based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Method, Predictor Network, Target Season and Target Domain* by Rutherford et al. (2005)

Rutherford et al. discuss the necessity of climate reconstruction with multi-proxy networks as empirical evidence in documenting past climate variability. In this case, they note the advantage of using high-resolution proxies (annually or seasonally resolved proxies, such as tree rings, corals and ice cores) because they overlap with instrumental data of the past century, allowing analysis of their climate signal and reliability. These proxies have been used to reconstruct spatial climate fields which not only provide a climate variability record but which also retain information of the mechanisms or forcing underlying the variability. Annually resolved proxy networks have also been used to directly reconstruct indices of climate variability, but these methods are somewhat flawed in that they assume a direct relationship between the recorded proxy variables and temperature and precipitation but large-scale climate influences may change over time. Rutherford et al. focus specifically on recent constructions of this type of Northern Hemisphere temperatures and the reasons for the differences between reconstructions.

There are four identifiable factors that largely contribute to differences in reconstructions. Those are 1) using proxies as calibrators for surface temperature patterns, 2) the difference in character of the proxy networks used, 3) the target season of reconstruction, and 4) the target region of reconstruction. The intent of this study is to provide an assessment of the relative impacts of these four factors.

To measure the sensitivity of the proxy network selected, three networks were used: the multiproxy dataset used by Mann et al., the MXD data used by Briffa et al., and a combination of these datasets for the third network. To perform the reconstruction on these three networks a RegEM approach of climate field reconstruction was used. The RegEM method is an iterative method for estimating missing data through the estimation of means and covariances from an incomplete data field. The calibration interval for this approach was the time interval that includes overlap of proxy and instrumental data. Rutherford et al. made two modifications to the RegEM approach. First, they applied the method in a stepwise fashion, performing the reconstruction one step at a time using all available climate information. Second, they separated the datasets into low and high frequency datasets to create two independent reconstructions, which were then combined at the conclusion of the experiment to create a complete reconstruction. In the findings, Rutherford et al. stated that using a 20 year boundary for the frequency calibration gave superior results in almost all cases while the stepwise modification of the RegEM method did not produce any different results. Additionally, since the combined network showed only marginal improvement over the other two, it is likely that these reconstructions are relatively insensitive to the proxy network used.

To measure the sensitivity of the target season and region on reconstruction, Rutherford et al. performed an array of RegEM climate field reconstructions based on various seasons and regions. These reconstructions were compared with several previous reconstructions based on common predictor datasets. Rutherford et al. found that the optimal results for the MXD data were produced for the period in which the cold season *ends* while the optimal results for the multiproxy principal component reconstruction (Mann and coworkers reconstruction) were produced for the period in which the cold season *begins*. Additionally, the MXD network was found to outperform the combined network in the warm season. In terms of region, Rutherford et al. found differences in the target region lead to significant variability in the hemispheric mean estimates. While they found that reconstructions are sensitive to changes in season and region, Rutherford et al. maintained that the unprecedented temperatures in the late 20th century that are seen in many reconstructions are supported with respect to all of the factors considered in this study.

Summary of *Abrupt Climate Change: An Alternative View* by Carl Wunsch (2006)

A Dansgaard-Oeschger (D-O) event is a rapid climate fluctuation, taking place at the end of the Ice Age. Twenty-three such events have been identified between 110,000 and 23,000 years before present. A widely held view of abrupt climate change during the last glacial period is that these D-O events are at least hemispheric, if not global and caused by changes in ocean circulation. It has been hypothesized that there may be abrupt climate change similar to a D-O event because of ongoing global warming and its oceanic affects. Underlying the major conclusions about D-O events and abrupt climate change there are several assumptions, including (1) the ^{18}Oxygen variations appearing in ice cores are viable as a proxy, (2) climate fluctuations in Greenland reflect those on a hemispheric or global basis, (3) the cause of D-O events can be traced to major changes of the North Atlantic meridional overturning circulation and perhaps failure of the Gulf Stream, and (4) apparent detection of a D-O event at a remote location in a proxy implies local climatic importance. In this article Wunsch reexamines these assumptions in order to assess their relevance, specifically focusing on (2) and (3).

In terms of using ^{18}Oxygen in the Greenland ice cores as a climate proxy, Wunsch found that although is was relatively accurate for central Greenland, when aligned with other locations a visual similarity would appear on the spectral graph, but that there was actually little statistical correlation; this occurred when comparing time periods of less than 900 years. While this does not disprove the hypothesis of a large impact of the D-O events, it cannot be used to support this assumption. There are three possible explanations for the disappearance of covariance for these periods less than 900 years. First, although both records have wide variability, it is primarily regional in character and there is no simple relationship between them. Second, the age-model (the calibration of age versus depth in the core) error has a larger influence on the short period variations than the long period ones. Third, different physical processes dominate the proxies at high frequency in the two separate locations, but they have roughly similar low spectral moments. Any of these factors could affect the lack of covariance between geographical locations. Subsequently, the assumption that there exist large-scale hemispheric correlations with the D-O events is neither proven nor disproven.

The heat flux associated with meridional overturning (the sinking and spreading of cold water and dispersion of heat) of the ocean has the most direct impact on the atmosphere in terms of oceanic circulation patterns. The contribution of the oceanic Northern Hemisphere to this pole-ward circulation falls very rapidly as heat is transferred to the atmosphere. At the 40th latitude North, the oceanic contribution is less than 25% of the atmospheric contribution. Hypothetically, if warming continues, and the Northern Atlantic is injected with fresh water from glacial melting, the meridional overturning circulation would be dramatically reduced, resulting in a D-O-like event. However, models attempting to construct this theoretical climate change have not been successful, mostly in that they have not taken into account the overlying wind field response to this event. Since much of the temperature flux of the North Atlantic is carried in the Gulf Stream, scenarios requiring wind shifts sufficient to shut it down are likely a physical impossibility because of the need to conserve angular momentum in the atmosphere.

Coupled models that have been claimed to show an atmospheric response to oceanic flux shifts are so simplified and lack adequate resolution that they cannot be skillfully integrated over the time periods required to describe true climatic time scales. Again, these models are only indicators of processes that *can* be operating but with no evidence that they dominate.

While the abrupt climate changes in Greenland may not have occurred in other parts of the globe, there still is the question of why it occurred in Greenland. One apparent observation is that the D-O events ceased in the Holocene and have been remarkably placid since. As such, the operative mechanism causing the D-O events must have also disappeared. The answer is the disappearance of the Laurentide and Fennoscandian ice sheets. Two enormous mountain ranges of high albedo (reflection factor) were removed. In a study by Jackson (2000), he noted that small, regional changes in the ice sheet elevations had a large effect on the atmospheric stationary wave patterns. As a standing wave, the wind encountering the ice sheets had more than one equilibrium state. Major local climate change could appear with a slight shift in the wave pattern of the wind system. While the model for this hypothesis is rough, other studies have indicated great influence of the ice sheets on atmospheric scales as well. The body of these theories suggests that the most important and sensitive determinant of oceanic circulation is wind, and not the temperature flux. Similarly, the widely accepted view that D-O events were of global impact and may occur as a result of recent warming is based on four assumptions, which in turn are based on ambiguous data and a high degree of uncertainty. As such, to make conclusions about such events would be imprudent without first addressing the uncertainties in the age-model as well a cautious reinterpretation of proxy signals.

Summary of *The Spatial Extent of 20th Century Warmth in the Context of the Past 1200 Years* by Timothy Osborn and Keith Briffa (2006)

In this article Osborn and Briffa review past work on proxy-based climate reconstruction in an attempt to assess if the claim that the late 20th century was the warmest period during the past millennium is supported. Whether or not this claim is supported depends on the comparison of recent instrumental temperature records with the earlier proxy-based temperature reconstructions. This comparison is only valid if it takes an account of the uncertainties associated with interpreting a specific reconstruction as an estimate of the actual temperature. Some of the reviewed studies do not provide a reconstruction of the entire millennia and some do not estimate the uncertainty in an appropriate manner assessing the significance of late 20th century warmth. Osborn and Briffa focus on three studies that meet the criteria of a formal quantitative comparison of late 20th century temperatures against reconstructed temperatures for the past millennia. These studies are Mann et al. 1999, Mann and Jones 2003, and Jones, Osborn and Briffa 2001. While all of these studies supported the claim of unprecedented temperatures in the 20th century and published uncertainties associated with proxy reconstructions above the 95% uncertainty range, the Intergovernmental Panel on Climate Change (IPCC) concluded that this claim could only be made with a 66 to 90% confidence because of un-quantifiable error that may arise from the proxies in the dataset. Osborn and Briffa conduct their own analysis of the proxy data of Mann et al. 1999 by smoothing the data and simply counting the fraction of records with values that exceed one or two standard deviations from the mean. The differences between pairs of these fractional exceedance time series (or the fraction of records at least one standard deviation above the mean minus the fraction of records with at least one standard deviation below the mean) were also analyzed. The highest positive deviations occur in the late 20th century, even far exceeding those of the mid-20th century.

The instrumental temperature results show a close correspondence with the proxy records, especially for the early 20th century increase and variations during 1930-1975. Additionally, the multi-decadal intervals support the concepts of the medieval warming period and Little Ice Age period. However, the dates of onset are vague and the analysis geographically restricted. The most conclusive finding is that the 20th century is the most anomalous interval in the entire period of analysis, including significant positive extremes in the proxy records.

TAB 11

From Intergovernmental Panel on Climate Change First Assessment Report, 1990

7.1 Introduction

This Section focuses on changes and variations in the modern climate record. To gain a longer term perspective and to provide a background to the discussion of the palaeo-analogue forecasting technique in Section 3, variations in palaeo-climate are also described. Analyses of the climate record can provide important information about natural climate variations and variability. A major difficulty in using observed records to make deductions about changes resulting from recent increases in greenhouse gases (Sections 1 and 2) is the existence of natural climatic forcing factors that may add to, or subtract from, such changes. Unforced internal variability of the climate system will also occur, further obscuring any signal induced by greenhouse gases.

Observing the weather, and converting weather data to information about climate and climate change, is a very complex endeavour. Virtually all our information about modern climate has been derived from measurements which were designed to monitor weather rather than climate change. Even greater difficulties arise with the proxy data (natural records of climate-sensitive phenomena, mainly pollen remains, lake varves and ocean sediments, insect and animal remains, glacier termini) which must be used to deduce the characteristics of climate before the modern instrumental period began. So special attention is given to a critical discussion of the quality of the data on climate change and variability and our confidence in making deductions from these data. Note that we have not made much use of several kinds of proxy data, for example tree ring data, that can provide information on climate change over the last millennium. We recognise that these data have an increasing potential; however their indications are not yet sufficiently easy to assess nor sufficiently integrated with indications from other data to be used in this report.

A brief discussion of the basic concepts of climate, climate change, climate trends etc, together with references to material containing more precise definitions of terms, is found in the Introduction at the beginning of this Report.

7.2 Palaeo-Climatic Variations and Change

7.2.1 Climate Of The Past 5,000,000 Years

Climate varies naturally on all time scales from hundreds of millions of years to a few years. Prominent in recent Earth's history have been the 100,000 year Pleistocene glacial-interglacial cycles when climate was mostly cooler than at present (Imbrie and Imbrie, 1979). This period began about 2,000,000 years before the present time (BP) and was preceded by a warmer epoch having only limited glaciation, mainly over Antarctica, called the Pliocene. Global surface temperatures have typically varied by 5-7°C through the Pleistocene ice age cycles, with large changes in ice volume and sea level, and temperature variations as great as 10-15°C in some middle and high latitude regions of the Northern Hemisphere. Since the beginning of the current interglacial epoch about 10,000 BP, global temperatures have fluctuated within a much smaller range. Some fluctuations have nevertheless lasted several centuries, including the Little Ice Age which ended in the nineteenth century and which was global in extent.

Proxy data clearly indicate that the Earth emerged from the last ice age 10,000 to 15,000 BP (Figure 7.1). During this glacial period, continental-size ice sheets covered much of North America and Scandinavia, and world sea level was about 120m below present values. An important cause of the recurring glaciations is believed to be variations in seasonal radiation receipts in the Northern Hemisphere. These variations are due to small changes in the distance of the Earth from the sun in given seasons, and slow changes in the angle of the tilt of the Earth's axis which affects the amplitude of the seasonal insolation. These "Milankovitch" orbital effects (Berger, 1980) appear to be correlated with the glacial-interglacial cycle since glacials arise when solar radiation is least in the extratropical Northern Hemisphere summer.

Variations in carbon dioxide and methane in ice age cycles are also very important factors; they served to modify and perhaps amplify the other forcing effects (see Section 1). However, there is evidence that rapid changes in climate have occurred on time scales of about a century which cannot be directly related to orbital forcing or to changes in atmospheric composition. The most dramatic of these events was the Younger Dryas cold episode which involved an abrupt reversal of the general warming trend in progress around 10,500 BP as the last episode of continental glaciation came to a close. The Younger Dryas was an event of global significance; it was clearly observed in New Zealand (Salinger, 1989) though its influence may not have extended to all parts of the globe (Rind et al., 1986). There is, as yet, no consensus on the reasons for this climatic reversal, which lasted about 500 years and ended very suddenly. However, because the signal was strongest around the North Atlantic Ocean, suggestions have been made that the climatic reversal had its physical origin in large changes in the sea surface temperature (SST) of the North Atlantic Ocean. One possibility is that the cooling may have resulted from reduced deep water production in the North Atlantic following large-scale melting of the Laurentide Ice sheet and the resulting influx of huge amounts of low density freshwater into the northern North Atlantic ocean (Broecker et al., 1985). Consequential changes in the global oceanic circulation may have occurred (Street-Perrott and Perrott, 1990) which may have involved variations in the strength of the thermohaline circulation in the Atlantic. This closed oceanic circulation involves northward flow of water near the ocean surface, sinking in the sub-Arctic and a return flow at depth. The relevance of the Younger Dryas to today's conditions is that it is possible that changes in the thermohaline circulation of a qualitatively similar character might occur quite quickly during a warming of the climate induced by greenhouse gases. A possible trigger might be an increase of precipitation over the extratropical North Atlantic (Broecker, 1987), though the changes in ocean circulation are most likely to be considerably smaller than in the Younger Dryas. Section 6 gives further details.

The period since the end of the last glaciation has been characterized by small changes in global average temperature with a range of probably less than 2°C (Figure 7.1), though it is still not clear whether all the fluctuations indicated were truly global. However, large regional changes in hydrological conditions have occurred, particularly in the tropics. Wetter conditions in the Sahara from 12,000 to 4,000 years BP enabled cultural groups to survive by hunting and fishing in what are today almost the most arid regions on Earth. During this time Lake Chad expanded to become as large as the Caspian Sea is today (several hundred thousand km^2, Grove and Warren, 1968). Drier conditions became established after 4,000 BP and many former lake basins became completely dry (Street-Perrot and Harrison, 1985). Pollen sequences from lake beds of northwest India suggest that periods with subdued monsoon activity existed during the recent glacial maximum (Singh et al., 1974), but the epoch 8,000 to 2,500 BP experienced a humid climate with frequent floods.

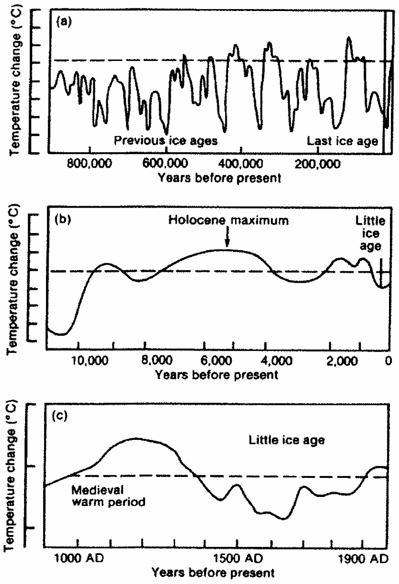


Figure 7.1: Schematic diagrams of global temperature variations since the Pleistocene on three time-scales: (a) the last million years, (b) the last ten thousand years, and (c) the last thousand years. The dotted line nominally represents conditions near the beginning of the twentieth century.

There is growing evidence that worldwide temperatures were higher than at present during the mid-Holocene (especially 5,000-6,000 BP), at least in summer, though carbon dioxide levels appear to have been quite similar to those of the pre-industrial era at this time (Section 1). Thus parts of western Europe, China, Japan, the eastern USA were a few degrees warmer in July during the mid-Holocene than in recent decades (Yoshino and Urushibara, 1978; Webb et al., 1987; Huntley and Prentice, 1988; Zhang and Wang, 1990). Parts of Australasia and Chile were also warmer. The late tenth to early thirteenth centuries (about AD 950-1250) appear to have been exceptionally warm in western Europe, Iceland and Greenland (Alexandre 1987; Lamb, 1988). This period is known as the Medieval Climatic Optimum. China was, however, cold at this time (mainly in winter) but South Japan was warm (Yoshino, 1978). This period of widespread warmth is notable in that there is no evidence that it was accompanied by an increase of greenhouse gases.

Cooler episodes have been associated with glacial advances in alpine regions of the world; such "neo-glacial" episodes have been increasingly common in the last few thousand years. Of particular interest is the most recent cold event, the "Little Ice Age", which resulted in extensive glacial advances in almost all alpine regions of the world between 150 and 450 years ago (Grove, 1988) so that glaciers were more extensive 100-200 years ago than now nearly everywhere (Figure 7.2). Although not a period of continuously cold climate, the Little Ice Age was probably the coolest and most globally-extensive cool period since the Younger Dryas. In a few regions, alpine glaciers advanced down-valley even further than during the last glaciation (for example, Miller, 1976). Some have argued that an increase in explosive volcanism was responsible for the coolness (for example Hammer, 1977; Porter, 1986); others claim a connection between glacier advances and reductions in solar activity (Wigley and Kelly, 1989) such as the Maunder and Sporer solar activity minima (Eddy, 1976), but see also Pittock (1983). At present, there is no

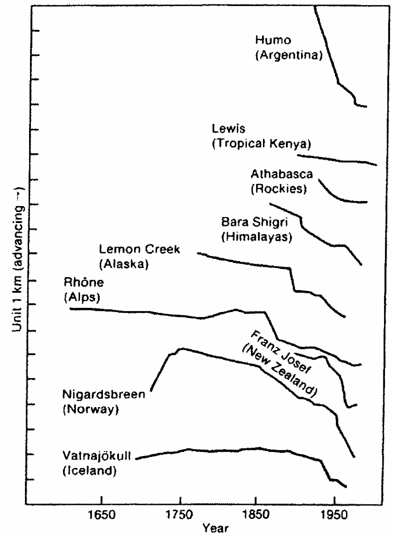


Figure 7.2: Worldwide glacier termini fluctuations over the last three centuries (after Grove, 1988, and other sources).

agreed explanation for these recurrent cooler episodes. The Little Ice Age came to an end only in the nineteenth century. Thus some of the global warming since 1850 could be a recovery from the Little Ice Age rather than a direct result of human activities. So it is important to recognise that natural variations of climate are appreciable and will modulate any future changes induced by man.

7.2.2 Palaeo-Climate Analogues from Three Warm Epochs

Three periods from the past have been suggested by Budyko and Izrael (1987) as analogues of a future warm climate. For the second and third periods listed below, however, it can be argued that the changed seasonal distribution of incoming solar radiation existing at those times may not necessarily have produced the same climate as would result from a globally-averaged increase in greenhouse gases.

1) The climate optimum of the Pliocene (about 3,300,000 to 4,300,000 years BP).
2) The Eemian interglacial optimum (125,000 to 130,000 years BP).
3) The mid-Holocene (5,000 to 6,000 years BP).

Note that the word "optimum" is used here for convenience and is taken to imply a warm climate. However such a climate may not be "optimal" in all senses.

7.2.2.1 Pliocene climatic optimum (about 3,300,000 to 4,300,000 BP)

Reconstructions of summer and winter mean temperatures and total annual precipitation have been made for this

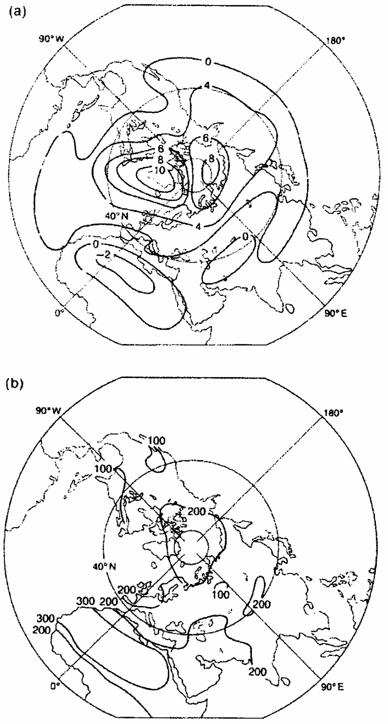
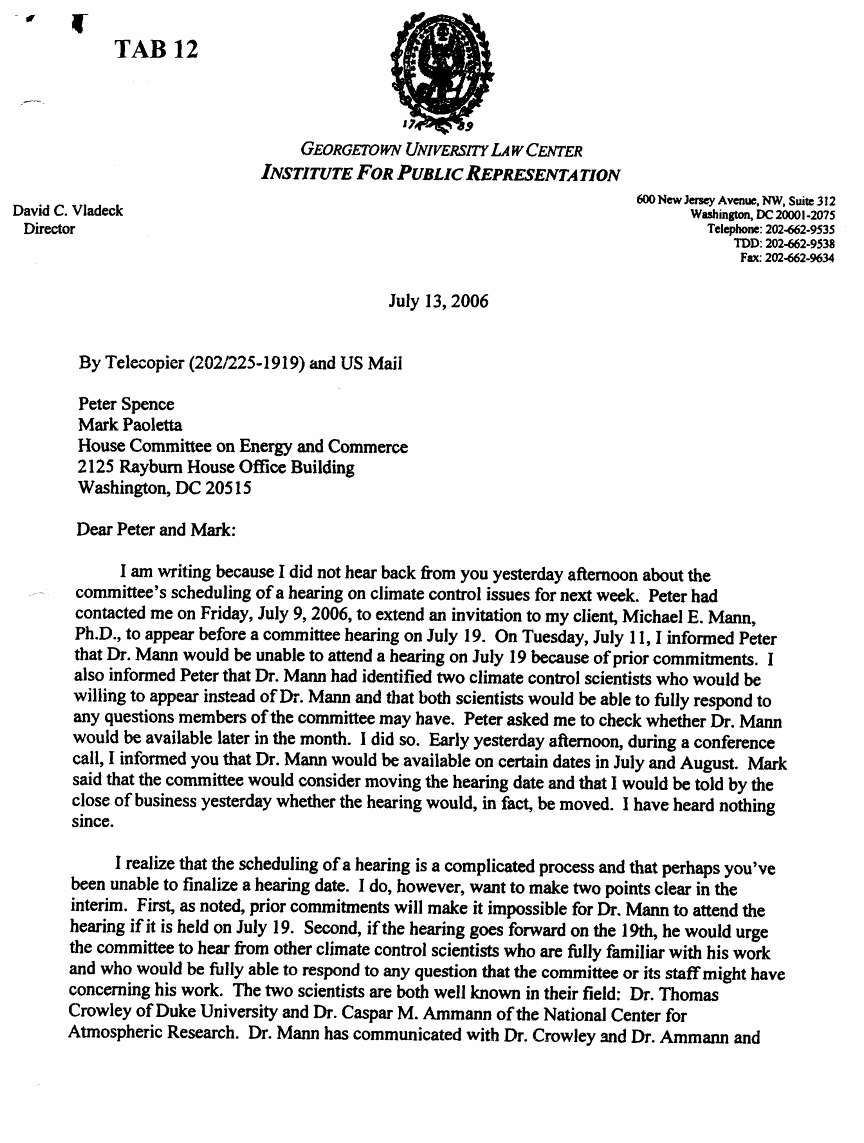


Figure 7.3: (a) Departures of summer air temperature (°C) from modern values for the Pliocene climatic optimum (4.3 to 3.3 million years BP) (from Budyko and Izrael, 1987).
(b) Departures of annual precipitation (mm) from modern values for the Pliocene climatic optimum (from Budyko and Izrael, 1987; Peshy and Velichko, 1990).



TAB 12

GEORGETOWN UNIVERSITY LAW CENTER
INSTITUTE FOR PUBLIC REPRESENTATION

David C. Vladeck
Director

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Washington, DC 20001-2075
Telephone: 202-662-9535
TDD: 202-662-9538
Fax: 202-662-9634

July 13, 2006

By Telecopier (202/225-1919) and US Mail

Peter Spence
Mark Paoletta
House Committee on Energy and Commerce
2125 Rayburn House Office Building
Washington, DC 20515

Dear Peter and Mark:

I am writing because I did not hear back from you yesterday afternoon about the committee's scheduling of a hearing on climate control issues for next week. Peter had contacted me on Friday, July 9, 2006, to extend an invitation to my client, Michael E. Mann, Ph.D., to appear before a committee hearing on July 19. On Tuesday, July 11, I informed Peter that Dr. Mann would be unable to attend a hearing on July 19 because of prior commitments. I also informed Peter that Dr. Mann had identified two climate control scientists who would be willing to appear instead of Dr. Mann and that both scientists would be able to fully respond to any questions members of the committee may have. Peter asked me to check whether Dr. Mann would be available later in the month. I did so. Early yesterday afternoon, during a conference call, I informed you that Dr. Mann would be available on certain dates in July and August. Mark said that the committee would consider moving the hearing date and that I would be told by the close of business yesterday whether the hearing would, in fact, be moved. I have heard nothing since.

I realize that the scheduling of a hearing is a complicated process and that perhaps you've been unable to finalize a hearing date. I do, however, want to make two points clear in the interim. First, as noted, prior commitments will make it impossible for Dr. Mann to attend the hearing if it is held on July 19. Second, if the hearing goes forward on the 19th, he would urge the committee to hear from other climate control scientists who are fully familiar with his work and who would be fully able to respond to any question that the committee or its staff might have concerning his work. The two scientists are both well known in their field: Dr. Thomas Crowley of Duke University and Dr. Caspar M. Ammann of the National Center for Atmospheric Research. Dr. Mann has communicated with Dr. Crowley and Dr. Ammann and both would be available to appear before the Committee on July 19. Their contact information is set forth in the margin to this letter.[1]

As we've made clear in the past, Dr. Mann stands ready to assist the committee in its important work.

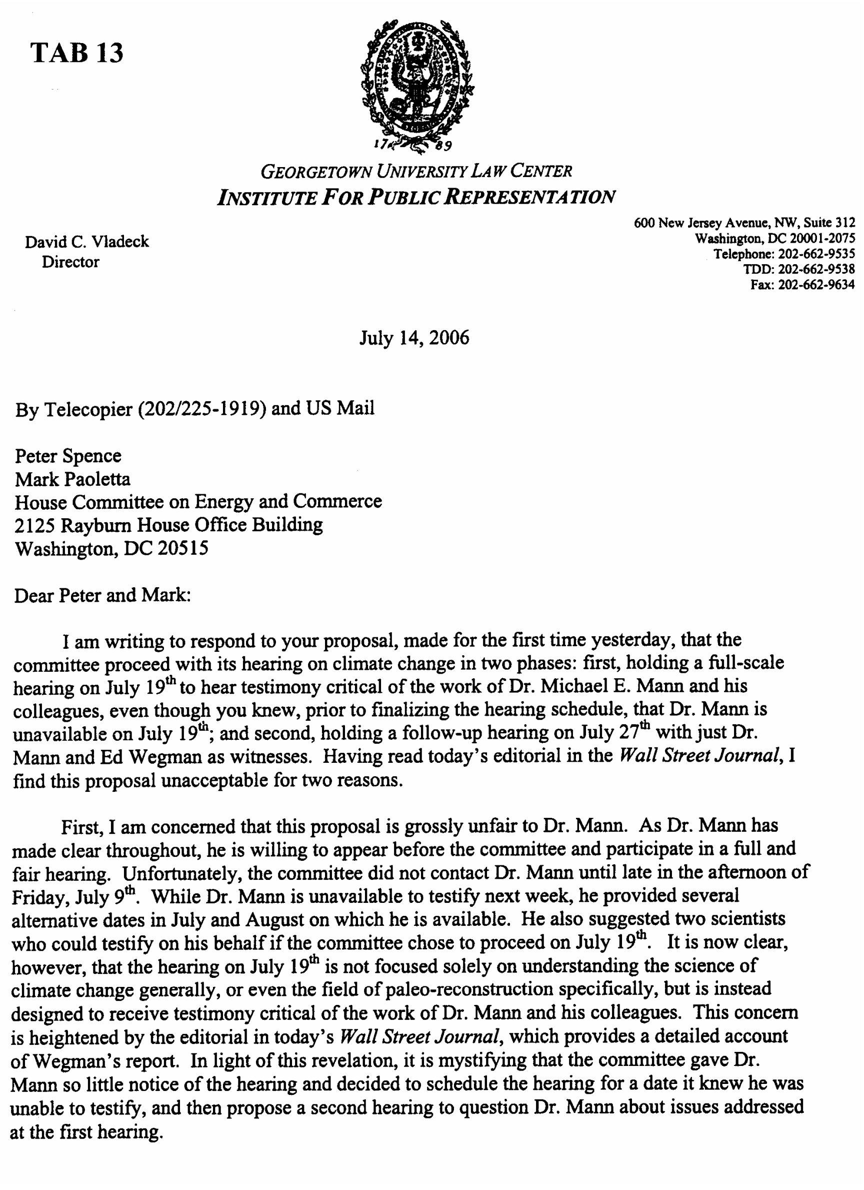
Sincerely,

/s/
David C. Vladeck
Attorney for Dr. Mann

---

[1] Dr. Thomas J. Crowley
Nicholas Professor of Earth Systems Science
Duke University
313 Old Chemistry
Box 90227
Durham, NC 27708
(919) 681-8228

Dr. Caspsar M. Ammann
National Center for Atmospheric Research
Climate and Global Dynamics Division
1850 Table Mesa Drive
Boulder, CO 80307-3000
(303) 497-1705



TAB 13

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July 14, 2006

By Telecopier (202/225-1919) and US Mail

Peter Spence
Mark Paoletta
House Committee on Energy and Commerce
2125 Rayburn House Office Building
Washington, DC 20515

Dear Peter and Mark:

I am writing to respond to your proposal, made for the first time yesterday, that the committee proceed with its hearing on climate change in two phases: first, holding a full-scale hearing on July 19th to hear testimony critical of the work of Dr. Michael E. Mann and his colleagues, even though you knew, prior to finalizing the hearing schedule, that Dr. Mann is unavailable on July 19th; and second, holding a follow-up hearing on July 27th with just Dr. Mann and Ed Wegman as witnesses. Having read today's editorial in the *Wall Street Journal*, I find this proposal unacceptable for two reasons.

First, I am concerned that this proposal is grossly unfair to Dr. Mann. As Dr. Mann has made clear throughout, he is willing to appear before the committee and participate in a full and fair hearing. Unfortunately, the committee did not contact Dr. Mann until late in the afternoon of Friday, July 9th. While Dr. Mann is unavailable to testify next week, he provided several alternative dates in July and August on which he is available. He also suggested two scientists who could testify on his behalf if the committee chose to proceed on July 19th. It is now clear, however, that the hearing on July 19th is not focused solely on understanding the science of climate change generally, or even the field of paleo-reconstruction specifically, but is instead designed to receive testimony critical of the work of Dr. Mann and his colleagues. This concern is heightened by the editorial in today's *Wall Street Journal*, which provides a detailed account of Wegman's report. In light of this revelation, it is mystifying that the committee gave Dr. Mann so little notice of the hearing and decided to schedule the hearing for a date it knew he was unable to testify, and then propose a second hearing to question Dr. Mann about issues addressed at the first hearing.

This is just not the way we do things in the United States. It is a bedrock principle of American jurisprudence, enshrined in the Sixth Amendment and elsewhere, that the right to confront one's critics --- at the time the criticism is made --- is fundamental. The *Wall Street Journal* editorial makes it evident that Ed Wegman and others believe that the work of Dr. Mann and his colleagues is flawed. To be sure, they are entitled to their views. But because Dr. Mann is unavailable on July 19th, he will not be able to confront directly witnesses who will come before the committee to criticize his work. Being required to respond at a later date to accusations or claims he did not hear is simply unreasonable.

These concerns are heightened by today's *Wall Street Journal* editorial, which raises questions about the goals of the hearing. Although the committee has suggested that it intends to hold this hearing to delve more deeply into the scientific questions surrounding the "Hockey Stick" temperature studies, it is now apparent from the *Wall Street Journal* editorial that the questions Wegman raises are not strictly ones of science. Wegman's study was not peer reviewed, it was not prepared by academics who are experts in climate studies, and, it does not even appear to acknowledge the recent report by the National Academy of Sciences/National Research Council which examined the findings of Dr. Mann and others. This omission is striking since the NAS/NRC report was prepared at Congress' direction. Wegman argues that there are statistical flaws that undermine the conclusions drawn by Dr. Mann and his colleagues. But the National Academy had two expert statisticians on its panel, Dr. Peter Bloomfield, Professor, North Carolina State University, and Dr. Douglas Nychka, Senior Scientist, National Center for Atmospheric Research, and the NAS/NRC report rejected precisely those claims. Unlike Wegman who has no expertise in climate studies, Dr. Bloomfield and Dr. Nychka have worked for decades as statisticians engaged in climate studies. To compound the problem, Wegman's paper appears to simply rehash the already discredited criticism of the work of Dr. Mann and his colleagues by Steve McIntrye and Ross McKitrick. The NAS/NRC report exhaustively reviews those criticisms and finds that they lack merit.

Please let me know if you have questions or would like to discuss this matter further.

Sincerely,

/s/
David C. Vladeck
Attorney for Dr. Michael E. Mann

TAB 14

Spencer, Peter

REDACTED

From: Yasmin Said [ysaid[redacted]
[illegible] Tuesday, July 18, 2006 6:54 PM
To: Spencer, Peter
Subject: FW: Re: Upcoming Congressional Testimony

REDACTED

>From: endersrobinso[redacted]
>To: "Edward Wegman" <ewegma[redacted]

REDACTED

Email Addresses
Redacted

>Subject: Re: Upcoming Congressional Testimony
>Date: Tue, 18 Jul 2006 22:46:15 +0000
>
>July 17, 2006
>
>To whom it my concern:
>
>I have read the report to Chairman Barton and to Chairman Whitfield
>entitled AD HOC COMMITTEE REPORT ON THE "HOCKEY STICK" GLOBAL CLIMATE
>RECONSTUCTION by Edward J. Wegman, David W. Scott, and Yasmin H. Said.
>In what follows this work of Wegman, Scott and Said is simply referred
>to as the Report.
>
>The assessment of previous results given in the Report is correct. The
>Report is entirely correct in stating that the most rudimentary
>additive model (the model of a simple temperature signal with
>superimposed noise) is not adequate to describe the complex
>relationships involved in climate dynamics. There is no physical
>process found in nature that does not involve feedback in one form or
>another to regulate the action of the system. The statistical methods
>and models described in the Report use more variables and make possible
>the construction of more elaborate reconstructions that allow feedback
>and interactions. The Report represents the correct way to proceed. It
>is especially important to bring the professional statistical community
>into the picture in order to assure that a sound analytical foundation
>   secured in the continuing development of this program.

>What has gone on in the past studies is not unusual in science. Many
>approaches are taken, nearly all of which contribute to understanding.
>The present Report represents a turning point, which was sure to come

>at one point or another. The turning point results in an assessment of
>past results, and the inclusion of workers from other disciplines to
>fill in gaps and bring the entire endeavor up to the highest scientific standards.
>We are indebted to the present Report for taking this step.

[illegible]would like to mention that the authors of the Report have long
>experience in the construction of statistical models for environmental
>studies. No research workers are better qualified in every sense than
>they are to deal with the problem at hand.
>
>Sincerely,
>
>
>Enders A. Robinson
>
>Professor Emeritus in the Maurice Ewing and J. Lamar Worzel Chair
>Department of Earth and Environmental Engineering Columbia University,
>New York, NY 10027
>
>Member of the National Academy of Engineering of the USA
>
>Fellow of the European Academy of Sciences
>
>
> -------------- Original message ----------------------
>From: "Edward Wegman" <ewegman[redacted]
> > Dear All,

Email Redacted

> >
> > ~~As many of you know, last September I was approached by a~~
> > Congressional staffer concerning doing an independent review of some
> > statistical
>issues
> > related to the so-called "Hockey Stick" temperature profile that was
[illegible] published in *Nature* in 1998 and featured prominently in the
[illegible] Intergovernmental Panel on Climate Change's *Climate Change 2001:
> > The Scientific Basis.* This U.N. report was used widely to support
> > the Kyoto Accord. The "hockey stick" graphic was also featured in Al
> > Gore's film,
>*An
> > Inconvenient Truth. *Along with Drs. David Scott and Yasmin Said, I
> > undertook to investigate the mathematics/statistics associated with
> > the "hockey stick" papers. John Rigsby, a recent graduate with a
> > M.S. in Statistical Science and a Ph.D. student in the College of
> > Science, and Denise Reeves, a Ph.D. student in the Volgenau School
> > also contributed
>to
> > our report. The incovenient truth is that the hockey stick is an
>artifact of
> > a misuse of statistical methodology. Our report was released by
> > Congress this morning and is available online at
> > http://energycommerce.house.gov/108/home/07142006_Wegman_Report.pdf.
> > The House Committee on Energy and Commerce press release is
> > available at
> > http://energycommerce.house.gov/108/News/07142006_1989.htm. A fact
>sheet is
> > available at
> > http://energycommerce.house.gov/108/home/07142006_Wegman_fact_sheet.pdf.
> > Finally, our report was the subject of a Wall Street Journal
> > editorial
>this
> > morning, which is available at
> > http://energycommerce.house.gov/108/News/07142006_1990.htm.

[illegible]Our testimony is scheduled for 10:00 am on July 19, 2006 and wil be
> > on
>the
> > web live and other media. The essence of our report was given as a

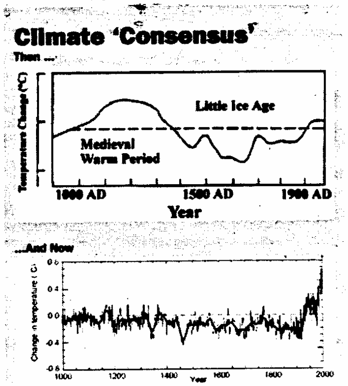
keynote
 > address at the NASA Workshop on Data Mining, May 23, 2006 and as a
keynote
 > address at the Symposium on the Interface of Computing Science and
 Statistics on May 25, 2006. It was also briefed to the Board of
[illegible]ectors of
 > the American Statistical Association. We are also tentatively
 > scheduled
for
 > a second round of testimony at a date to be announced. This has been
 > a really interesting effort that has consumed enormous amounts of time.
 >
 > Cheers,
 > Ed Wegman

TAB 15 Hockey Stick Hokum

It is routine these days to read in newspapers or hear—almost anywhere the subject of climate change comes up—that the 1990s were the "warmest decade in a millennium" and that 1998 was the warmest year in the last 1,000.

This assertion has become so accepted that it is often recited without qualification, and even without giving a source for the "fact." But a report soon to be released by the House Energy and Commerce Committee by three independent statisticians underlines yet again just how shaky this "consensus" view is, and how recent its vintage.

The claim originates from a 1999 paper by paleoclimatologist Michael Mann. Prior to Mr. Mann's work, the accepted view, as embodied in the U.N.'s 1990 report from the Intergovernmental Panel on Climate Change (IPCC), was that the world had undergone a warming period in the Middle Ages, followed by a mid-millennium cold spell and a subsequent warming period—the current one. That consensus, as shown in the first of the two IPCC-provided graphs nearby, held that the Medieval warm period was considerably warmer than the present day.



Source: Intergovernmental Panel on Climate Change

Mr. Mann's 1999 paper eliminated the Medieval warm period from the history books, with the result being the bottom graph you see here. It's a man-made global-warming evangelist's dream, with a nice, steady temperature oscillation that persists for centuries followed by a dramatic climb over the past century. In 2001, the IPCC replaced the first graph with the second in its third report on climate change, and since then it has cropped up all over the place. Al Gore uses it in his movie.

The trouble is that there's no reason to believe that Mr. Mann, or his "hockey stick" graph of global temperature changes, is right. Questions were raised about Mr. Mann's paper almost as soon as it was published. In 2003, two Canadians, Ross McKitrick and Steven McIntyre, published an article in a peer-reviewed journal showing that Mr. Mann's methodology could produce hockey sticks from even random, trendless data.

The report commissioned by the House Energy Committee, due to be released today, backs up and reinforces that conclusion. The three researchers—Edward J. Wegman of George Mason University, David W. Scott of Rice University and Yasmin H. Said of Johns Hopkins University—are not climatologists; they're statisticians. Their task was to look at Mr. Mann's methods from a statistical perspective and assess their validity. Their conclusion is that Mr. Mann's papers are plagued by basic statistical errors that call his conclusions into doubt. Further, Professor Wegman's report upholds the finding of Messrs. McIntyre and McKitrick that Mr. Mann's methodology is biased toward producing "hockey stick" shaped graphs.

Mr. Wegman and his co-authors are careful to point out that doubts about temperatures in the early part of the millennium do not call into question more-recent temperature increases. But as you can see looking at these two charts, it's all about context. In the first, the present falls easily within a range of natural historical variation. The bottom chart looks alarming and discontinuous with the past, which is why global-warming alarmists have adopted it so eagerly.

In addition to debunking the hockey stick, Mr. Wegman goes a step further in his report, attempting to answer why Mr. Mann's mistakes were not exposed by his fellow climatologists. Instead, it fell to two outsiders, Messrs. McIntyre and McKitrick, to uncover the errors.

Mr. Wegman brings to bear a technique called social-network analysis to examine the community of climate researchers. His conclusion is that the coterie of most frequently published climatologists is so insular and close-knit that no effective independent review of the work of Mr. Mann is likely. "As analyzed in our social network," Mr. Wegman writes, "there is a tightly knit group of individuals who passionately believe in their thesis." He continues: "However, our perception is that this group has a self-reinforcing feedback mechanism and, moreover, the work has been sufficiently politicized that they can hardly reassess their public positions without losing credibility."

In other words, climate research often more closely resembles a mutual-admiration society than a competitive and open-minded search for scientific knowledge. And Mr. Wegman's social-network graphs suggest that Mr. Mann himself—and his hockey stick—is at the center of that network.

Mr. Wegman's report was initially requested by the House Energy Committee because some lawmakers were concerned that major decisions about our economy could be made on the basis of the dubious research embodied in the hockey stick. Some of the more partisan scientists and journalists howled that this was an attempt at intimidation. But as Mr. Wegman's paper shows, Congress was right to worry; his conclusions make "consensus" look more like group-think. And the dismissive reaction of the climate-research establishment to the McIntyre-McKitrick critique of the hockey stick confirms that impression.

The Wall Street Journal, July 14, 2006

TAB 16

All Topics Menu

Help || About || Contact

Fast Moving Fronts Comments

< Return to menu of Fast Moving Fronts

ESI Special Topics, July 2002
Citing URL: http://www.esi-topics.com/fmf/july02-MichaelMann.html

From •>>July 2002

Professor Michael E. Mann answers a few questions about about this month's fast moving front in Geosciences.

Field: Geosciences

Title: "Global-scale temperature patterns and climate forcing over the past six centuries"

Authors: **Mann, ME**;Bradley, RS;Hughes, MK
Journal: NATURE, 392: (6678) 779-787 APR 23 1998
Addresses: Univ Massachusetts, Dept Geosci, Amherst, MA 01003 USA.
Univ Massachusetts, Dept Geosci, Amherst, MA 01003 USA.
Univ Arizona, Tree Ring Res Lab, Tucson, AZ 85721 USA.

ST: Why do you think your paper is highly cited?

I believe our '98 *Nature* article is highly cited because it establishes a new line of evidence, independent of the predictions of theoretical climate models, for the assertion that human beings are responsible in large part for 20th century global warming.

ST: Does it describe a new discovery or new methodology that's useful to others?

Our paper presented a novel multivariate statistical approach for assimilating the information contained in disparate "proxy" indicators of past climate change (natural archives such as tree rings, corals, and ice cores, which by their very nature record past climate changes at the annual timescale). Our methodology allowed this information to be synthesized and compared to modern instrumental climate records, allowing us to reconstruct large-scale patterns of surface temperature changes in the past, and, importantly, providing an estimate of the uncertainties inherent in the reconstructions.

ST: Could you summarize the significance of your paper in layman's terms?

Our paper established that the large-scale global warming which took place in the latter part of the 20th century is unprecedented over a fairly long period of geological time. The now oft-cited assertion that "the 1990s are the warmest decade of at least the past 1000 years" is attributable to our '98 *Nature* article (which established the result for the last 600 years) along with an extension of our work (which extended this conclusion to the past 1000 years) which we published in the journal "*Geophysical Research Letters*" in 1999. Our *Nature* article established that the warmth of the 1990s was outside the range of variability as indicated in our reconstruction of past Northern Hemisphere temperature variations, taking the uncertainties in the reconstruction into account. The paper also showed, from a statistical point of view, that the recent warming could not be explained in terms of "natural" influences (such as changes in solar output or explosive volcanic activity), but could only be explained in terms of anthropogenic factors (specifically, the increase of greenhouse gas concentrations due to modern industrial activity).

ST: How did you become involved in this research?

My Ph.D. dissertation (*Department of Geology & Geophysics, Yale University*) involved the development of statistical techniques for detecting "signals" in climate data. This work was limited to an analysis of the instrumental record, which only provides widespread spatial coverage over the globe for roughly the past century. My interest in extending such analyses to longer timescales inevitably led me to seek other sources of climate information, namely, "proxy" climate data sources of the sort discussed above. As I began to seek out scientists with expertise in this area to collaborate with in this undertaking, I had the good fortune to meet up with two top-notch paleoclimatogists in particular: Professor Raymond Bradley at the *University of Massachusetts*, and Professor Malcolm Hughes at the *University of Arizona*. Supported by a postdoctoral fellowship from the *Department of Energy*, and a grant from the *National Science Foundation "Earth Systems Research"* program, I collaborated with Bradley and Hughes on the scientific problem of reconstructing past climate changes from "proxy" climate data, for my postdoctoral research.

Professor Michael E. Mann,
Department of Environmental Sciences,
Clark Hall, University of Virginia,
Charlottesville, VA 22903

Fast Moving Fronts Comments Menu
Return to Fast Moving Fronts | Return to Special Topics main menu

ESI Special Topics, July 2002
Citing URL: http://www.esi-topics.com/fmf/july02-MichaelMann.html

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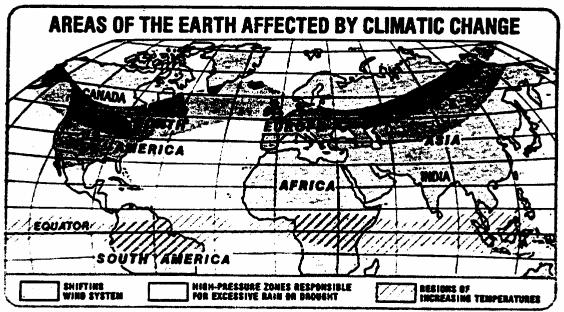
TAB 17

The Cooling World

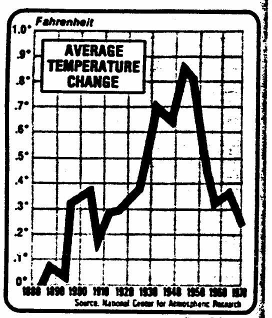
There are ominous signs that the earth's weather patterns have begun to change dramatically and that these changes may portend a drastic decline in food production—with serious political implications for just about every nation on earth. The drop in food output could begin quite soon, perhaps only ten years from now. The regions destined to feel its impact are the great wheat-producing lands of Canada and the U.S.S.R. in the north, along with a number of marginally self-sufficient tropical areas—parts of India, Pakistan, Bangladesh, Indochina and Indonesia—where the growing season is dependent upon the rains brought by the monsoon.



AREAS OF THE EARTH AFFECTED BY CLIMATIC CHANGE

CANADA
NORTH AMERICA
EUROPE
ASIA
INDIA
AFRICA
EQUATOR
SOUTH AMERICA
SHIFTING WIND SYSTEM
HIGH-PRESSURE ZONES RESPONSIBLE FOR EXCESSIVE RAIN OR DROUGHT
REGIONS OF INCREASING TEMPERATURES

Fenga & Freyer



AVERAGE TEMPERATURE CHANGE

Source: National Center for Atmospheric Research

Fenga & Freyer

The evidence in support of these predictions has now begun to accumulate so massively that meteorologists are hard-pressed to keep up with it. In England, farmers have seen their growing season decline by about two weeks since 1950, with a resultant over-all loss in grain production estimated at up to 100,000 tons annually. During the same time, the average temperature around the equator has risen by a fraction of a degree—a fraction that in some areas can mean drought and desolation. Last April, in the most devastating outbreak of tornadoes ever recorded, 148 twisters killed more than 300 people and caused half a billion dollars' worth of damage in thirteen U.S. states.

Trend: To scientists, these seemingly disparate incidents represent the advance signs of fundamental changes in the world's weather. The central fact is that after three quarters of a century of extraordinarily mild conditions, the earth's climate seems to be cooling down. Meteorologists disagree about the cause and extent of the cooling trend, as well as over its specific impact on local weather conditions. But they are almost unanimous in the view that the trend will reduce agricultural productivity for the rest of the century. If the climatic change is as profound as some of the pessimists fear, the resulting famines could be catastrophic. "A major climatic change would force economic and social adjustments on a worldwide scale," warns a recent report by the National Academy of Sciences, "because the global patterns of food production and population that have evolved are implicitly dependent on the climate of the present century."

A survey completed last year by Dr. Murray Mitchell of the National Oceanic and Atmospheric Administration reveals a drop of half a degree in average ground temperatures in the Northern Hemisphere between 1945 and 1968. According to George Kukla of Columbia University, satellite photos indicated a sudden, large increase in Northern Hemisphere snow cover in the winter of 1971-72. And a study released last month by two NOAA scientists notes that the amount of sunshine reaching the ground in the continental U.S. diminished by 1.3 per cent between 1964 and 1972.

To the layman, the relatively small changes in temperature and sunshine can be highly misleading. Reid Bryson of the University of Wisconsin points out that the earth's average temperature during the great Ice Ages was only about 7 degrees lower than during its warmest eras—and that the present decline has taken the planet about a sixth of the way toward the Ice Age average. Others regard the cooling as a reversion to the "little ice age" conditions that brought bitter winters to much of Europe and northern America between 1600 and 1900—years when the Thames used to freeze so solidly that Londoners roasted oxen on the ice and when iceboats sailed the Hudson River almost as far south as New York City.

Just what causes the onset of major and minor ice ages remains a mystery. "Our knowledge of the mechanisms of climatic change is at least as fragmentary as our data," concedes the National Academy of Sciences report. "Not only are the basic scientific questions largely unanswered, but in many cases we do not yet know enough to pose the key questions."

Extremes: Meteorologists think that they can forecast the short-term results of the return to the norm of the last century. They begin by noting the slight drop in over-all temperature that produces large numbers of pressure centers in the upper atmosphere. These break up the smooth flow of westerly winds over temperate areas. The stagnant air produced in this way causes an increase in extremes of local weather such as droughts, floods, extended dry spells, long freezes, delayed monsoons and even local temperature increases—all of which have a direct impact on food supplies.

"The world's food-producing system," warns Dr. James D. McQuigg of NOAA's Center for Climatic and Environmental Assessment, "is much more sensitive to the weather variable than it was even five years ago." Furthermore, the growth of world population and creation of new national boundaries make it impossible for starving peoples to migrate from their devastated fields, as they did during past famines.

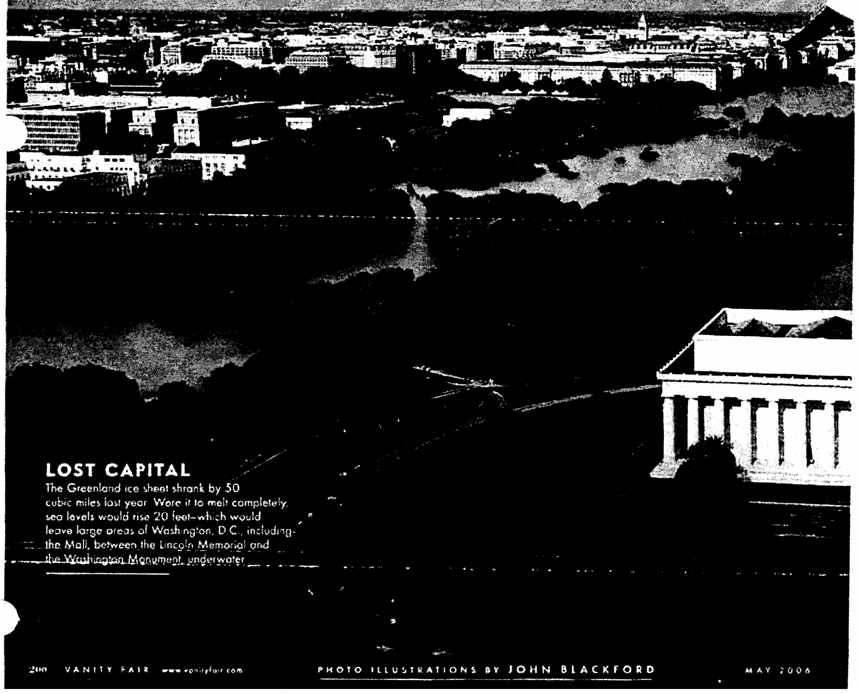
Climatologists are pessimistic that political leaders will take any positive action to compensate for the climatic change, or even to allay its effects. They concede that some of the more spectacular solutions proposed, such as melting the arctic ice cap by covering it with black soot or diverting arctic rivers, might create problems far greater than those they solve. But the scientists see few signs that government leaders anywhere are even prepared to take the simple measures of stockpiling food or of introducing the variables of climatic uncertainty into economic projections of future food supplies. The longer the planners delay, the more difficult will they find it to cope with climatic change once the results become grim reality.

—PETER GWYNNE with bureau reports

TAB 18

While Wash

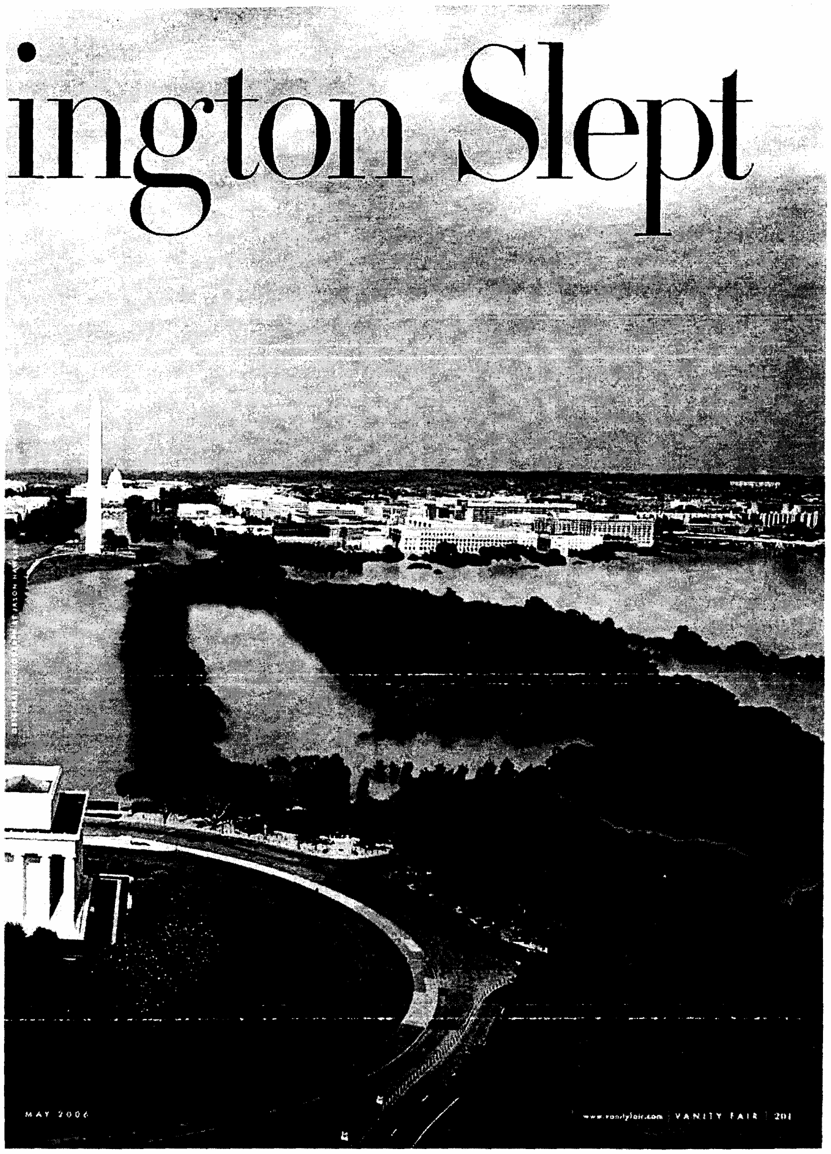
The Queen of England is afraid. International C.E.O.'s are nervous.
And the scientific establishment is loud and clear.
If global warming isn't halted, devastating sea-level rises will be inevitable by 2100.
So how did this virtual certainty get labeled a "liberal hoax" in the U.S.?
Try the same tactics Big Tobacco used to deny the dangers of smoking. As businesses,
governments, and climatologists around the globe call for action,
MARK HERTSGAARD reveals exactly how serious the threat has become—
and who persuaded the American people there is no crisis

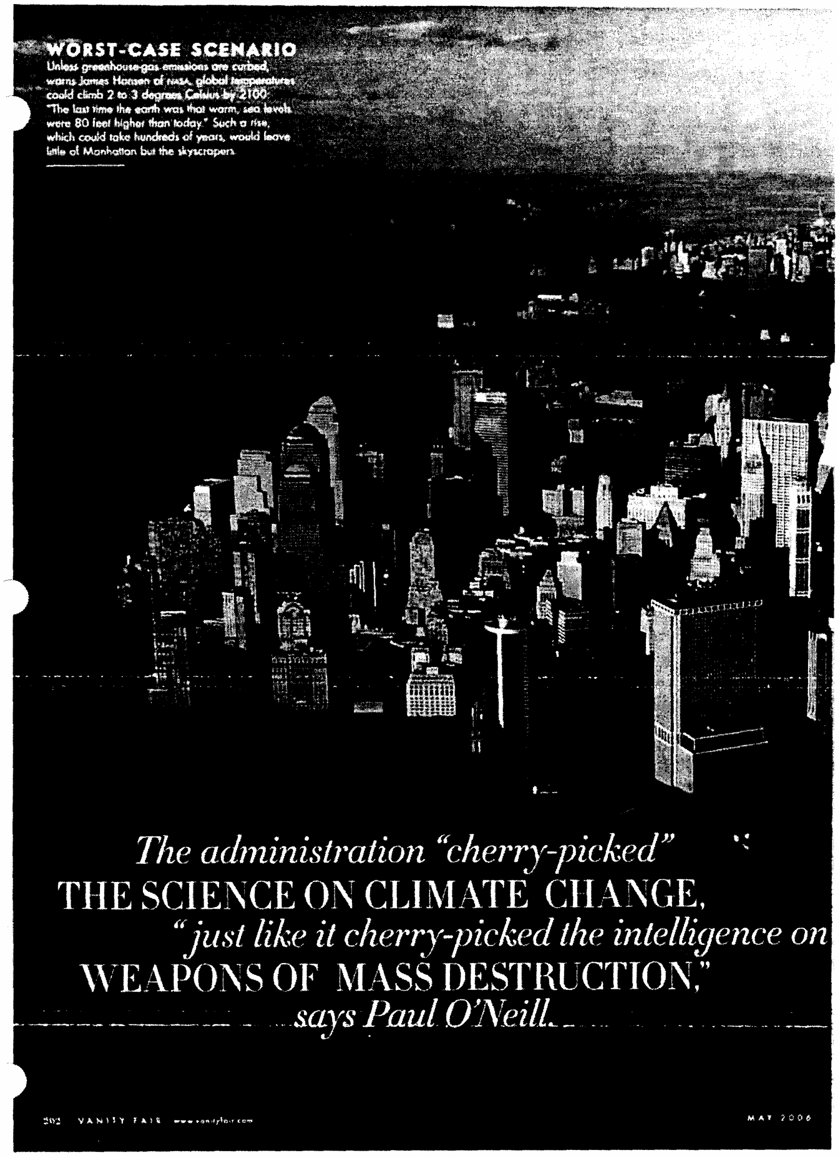


LOST CAPITAL
The Greenland ice sheet shrank by 50 cubic miles last year. Were it to melt completely, sea levels would rise 20 feet—which would leave large areas of Washington, D.C., including the Mall, between the Lincoln Memorial and the Washington Monument, underwater

PHOTO ILLUSTRATIONS BY JOHN BLACKFORD

ington Slept

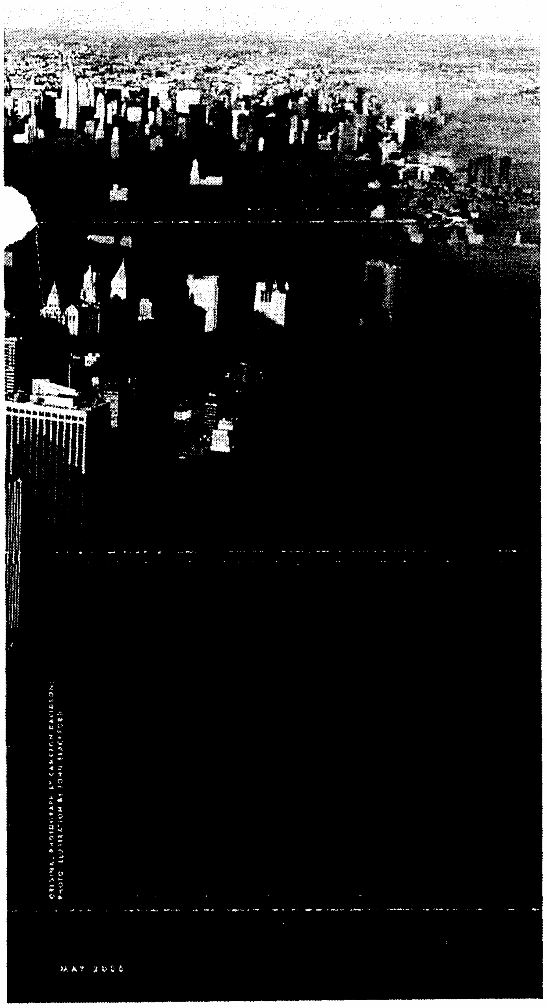




WORST-CASE SCENARIO

Unless greenhouse-gas emissions are curbed, warns James Hansen of NASA, global temperatures could climb 2 to 3 degrees Celsius by 2100. "The last time the earth was that warm, sea levels were 80 feet higher than today." Such a rise, which could take hundreds of years, would leave little of Manhattan but the skyscrapers.

The administration "cherry-picked" THE SCIENCE ON CLIMATE CHANGE, *"just like it cherry-picked the intelligence on* WEAPONS OF MASS DESTRUCTION," *says Paul O'Neill.*



ORIGINAL PHOTOGRAPH BY CARLTON DAVIDSON; PHOTO ILLUSTRATION BY JOHN BLACKFORD

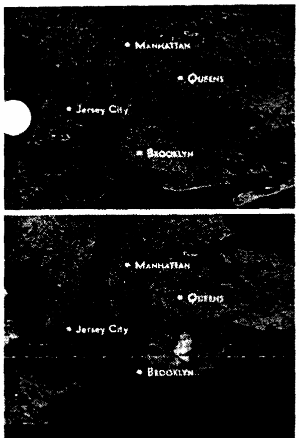
Ten months before Hurricane Katrina left much of New Orleans underwater, Queen Elizabeth II had a private conversation with Prime Minister Tony Blair about George W. Bush. The Queen's tradition of meeting once a week with Britain's elected head of government to discuss matters of state—usually on Tuesday evenings in Buckingham Palace and always alone, to ensure maximum confidentiality—goes back to 1952, the year she ascended the throne. In all that time, the contents of those chats rarely if ever leaked.

So it was extraordinary when London's *Observer* reported, on October 31, 2004, that the Queen had "made a rare intervention in world politics" by telling Blair of "her grave concerns over the White House's stance on global warming." *The Observer* did not name its sources, but one of them subsequently spoke to *Vanity Fair.*

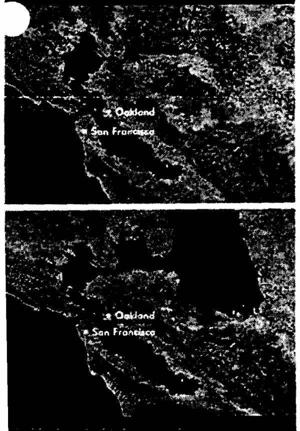
"The Queen first of all made it clear that Buckingham Palace would be happy to help raise awareness about the climate problem," says the source, a high-level environmental expert who was briefed about the conversation. "[She was] definitely concerned about the American position and hoped the prime minister could help change [it]."

Press aides for both the Queen and the prime minister declined to comment on the meeting, as is their habit. But days after the *Observer* story appeared, the Queen indeed raised awareness by presiding over the opening of a British-German conference on climate change, in Berlin. "I might just point out, that's a pretty unusual thing for her to do," says Sir David King, Britain's chief scientific adviser. "She doesn't take part in anything that would be overtly political." King, who has briefed the Queen on climate change, would not comment on the *Observer* report except to say, "If it were true, it wouldn't surprise me."

With spring arriving in England three weeks earlier than it did 50 years ago, the Queen could now see signs of climate change with her own eyes. Sandringham, her country estate north of London, overlooks Britain's premier bird-watching spot: the vast North Sea wetlands known as the Wash. A lifelong outdoorswoman, the Queen had doubtless



NEW YORK CITY
Current (top) and projected (above) aerial views of New York City, where a storm surge on top of a three-foot sea-level rise could flood prime real estate. (The images on these two pages are based on simulations created by the National Environmental Trust.)



SAN FRANCISCO
A rise in sea levels of three feet would leave San Francisco itself dry but could extend the bay 60 miles inland—all the way to Modesto and Sacramento.

observed the V-shaped flocks of pink-footed geese that descend on the Wash every winter. But in recent years, says Mark Avery, conservation director of the Royal Society for the Protection of Birds, she also would have seen a species new to the area: little egrets. These shiny white birds are native to Southern Europe, Avery says, "but in the last 5 to 10 years they have spread very rapidly to Northern Europe. We can't prove this is because of rising temperatures, but it sure looks like it."

Temperatures are rising, the Queen learned from King and other scientists, because greenhouse gases are trapping heat in the atmosphere. Carbon dioxide, the most prevalent of such gases, is released whenever fossil fuels are burned or forests catch fire. Global warming, the scientists explained, threatens to raise sea levels as much as three feet by the end of the 21st century, thanks to melting glaciers and swollen oceans. (Water expands when heated.)

This would leave much of eastern England, including areas near Sandringham, underwater. Global warming would also bring more heat waves like the one in the summer of 2003 that killed 31,000 people across Europe. It might even shut down the Gulf Stream, the flow of warm water from the Gulf of Mexico that gives Europe its mild climate. If the Gulf Stream were to halt—and it has already slowed 30 percent since 1992—Europe's temperatures would plunge, agriculture would collapse, London would no longer feel like New York but like Anchorage.

The Queen, says King, "got it" on climate change, and she wasn't alone. "Everyone in this country, from the political parties to the scientific establishment, to the Archbishop of Canterbury, to our oil companies and the larger business community, has come to a popular consensus about climate change—a sense of alarm and a conviction that action is needed now, not in the future," says Tony Juniper, executive director of the British arm of the environmental group Friends of the Earth.

At the time of his meeting with the Queen, Blair was being attacked on climate change from all ideological sides, with even the Conservatives charging that he was not doing enough. Yet Blair's statements on the issue went far beyond those of most world leaders. He had called the Kyoto Protocol, which has been ratified by 162 countries and requires industrial nations to reduce greenhouse-gas emissions 5 percent below 1990 levels, "not radical enough." The world's climate scientists, Blair pointed out, had estimated that 60 percent cuts in emissions were needed, and he committed Britain to reaching that goal by 2050.

But it wouldn't matter how much Britain cut its greenhouse-gas emissions if other nations didn't do the same. The U.S. was key, not only because it was the world's largest emitter but because its refusal to reduce emissions led China, India, Brazil, and other large developing countries to ask why they should do so. All this Blair had also said publicly. In 2001 he criticized the Bush administration for withdrawing from the Kyoto Protocol. In 2004 he said it was essential to bring the U.S. into the global effort against climate change, despite its opposition to Kyoto.

It was no secret that Bush opposed mandatory emissions limits, but Blair, who had risked his political future to back the deeply unpopular war in Iraq, was uniquely positioned to lobby the president. Bush owed him one. At the same time, Blair needed to show his domestic audience that he could stand up to Bush, that he wasn't the presidential "poodle" his critics claimed.

To compel Bush to engage the issue, Blair made climate change a lead agenda item at the July 2005 meeting of the Group of 8, the alliance of the world's eight richest nations. A month before the meeting, which was held at Gleneagles, in Scotland, Blair flew to Washington to see Bush face-to-face. That same day, the national academies of science of all the G-8 nations, as well as those of China, India, and Brazil, released a joint statement declaring that climate change was a grave problem that required immediate action.

On the morning of July 7, the summit was interrupted by the shocking news that four suicide bombers had set off explosions in London, killing 56 people. Blair rushed to the scene, but he returned that night, still determined to secure an agreement.

In the end, however, Bush held firm. Washington vetoed all references to mandatory emissions cuts or timelines, and the climate-change issue was overshadowed by African debt relief, which had been publicized by Bob Geldof's Live 8 concerts.

"There were no tough targets at Gleneagles because we would not have got all signatures on the document," says King, who adds, "We might well have" gotten seven—that is, every nation but the U.S. The farthest the G-8 leaders went—and even this required a battle, says King—was to include a sentence that read, in part, "While uncertainties remain in our understanding of climate science, we know enough to act now."

But seven weeks later, nature acted first, and it was the United States she hit.

PHOTO ILLUSTRATIONS, SECOND AND FOURTH FROM TOP, BOTH PAGES, BY JOHN BLACKFORD

No one can say for sure whether global warming caused Hurricane Katrina, which slammed into the Gulf Coast on August 29, 2005. But it certainly fit the pattern. The scientific rule of thumb is that one can never blame any one weather event on any single cause. The earth's weather system is too complex for that. Most scientists agree, however, that global warming makes extra-strong hurricanes such as Katrina more likely because it encourages hot oceans, a precondition of hurricane formation.

"It's a bit like saying, 'My grandmother died of lung cancer, and she smoked for the last 20 years of her life smoking killed her,'" explains Kerry Emanuel, a professor at the Massachusetts Institute of Technology who has studied hurricanes for 20 years. "Well, the problem is, there are an awful lot of people who die of lung cancer who never smoked. There are a lot of people who smoked all their lives and die of something else. So all you can say, even [though] the evidence statistically is clear connecting lung cancer to smoking, is that [the grandmother] upped her probability."

Just weeks before Katrina struck, Emanuel published a paper in the scientific journal *Nature* demonstrating that hurricanes had grown more powerful as global temperatures rose in the 20th century. Now, he says, by adding more greenhouse gases to the earth's atmosphere, humans are "loading the climatic dice in favor of more powerful hurricanes in the future."

But most Americans heard nothing about Hurricane Katrina's association with global warming. Media coverage instead reflected the views of the Bush administration—specifically, the National Oceanic and Atmospheric Administration, which declared that the hurricane was the result of natural factors. An outcry from N.O.A.A.'s scientists led the agency to backtrack from that statement in February 2006, but by then conventional wisdom was set in place. Post-Katrina New Orleans may eventually be remembered as the first major U.S. casualty of global warming, yet most Americans still don't know what hit us.

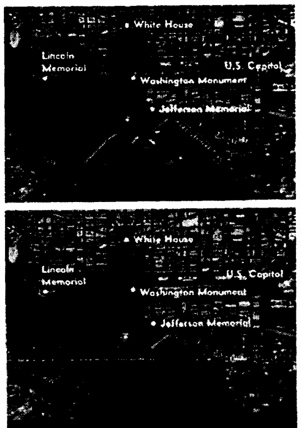
Sad to say, Katrina was the perfect preview of what global warming might look like in the 21st century. First, Katrina struck a city that was already below sea level—which is where rising waters could put many coastal dwellers in the years ahead. In 2001, the U.N.-sponsored Intergovernmental Panel on Climate Change (I.P.C.C.), a peer-reviewed, nternational collaboration among thousands of scientists that is the world's leading authority on climate change, predicted that sea levels could rise as much as three feet by 2100. By coincidence, three feet is about how much New Orleans sank during the 20th century. That was because levees built to keep the Mississippi River from flooding also kept the river from depositing silt that would have replenished the underlying land mass, explains Mike Tidwell, the author of *Bayou Farewell: The Rich Life and Tragic Death of Louisiana's Cajun Coast*. "You could say that in New Orleans we brought the ocean to the people," Tidwell adds, "which is pretty much what global warming will do to other cities in the future."

What's more, Katrina was a Category 5 hurricane, the strongest there is. Such extreme weather events will likely become more frequent as global warming intensifies, says the I.P.C.C. Yes, Katrina's winds had slowed to high–Category 3 levels by the time it made landfall, but it was the hurricane's storm surge that killed people—a surge that formed in the Gulf of Mexico when the storm was still Category 5. Thus, Katrina unleashed 10 to 15 feet of water on a city that was already significantly below sea level.

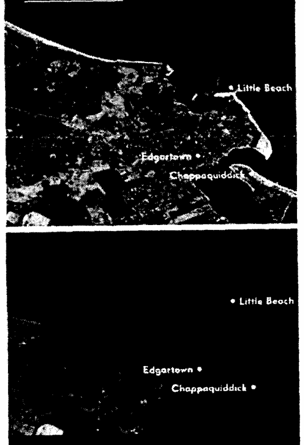
To envision global warming's future impacts, the illustrations accompanying this article reflect this and other scenarios. The three large-scale illustrations are an artist's interpretations of projections generated for *Vanity Fair* by Applied Science Associates Inc. (appsci.com), a marine-science consulting firm based in Rhode Island. The projections do not account for small-scale features such as coastal-protection structures.

The effects of a three-foot sea-level rise compounded by a storm surge from a Category 3 hurricane are shown in the image of the Hamptons, which would suffer severe flooding. The image of Washington, D.C., shows the effects of a 20-foot sea-level rise, which is what scientists expect if the entire Greenland ice sheet melts. The ice sheet has shrunk 50 cubic miles in the past year alone, and is now melting twice as fast as previously believed.

Finally, the image of New York City shows the effects of an 80-foot rise in sea levels. That's what would happen if not only the Greenland ice sheet but its counterpart in the Antarctic were to melt, says James Hansen, the director of NASA's Goddard Institute for Space Studies. Hansen, who put climate change on the media map in 1988 by saying that man-made global warming had already begun, made headlines again earlier this year when he complained that White House political appointees were trying to block him from speaking freely about the need for rapid reductions in greenhouse-gas emissions. Hansen warns that, if global emissions continue on their current trajectory, the ice sheets will not survive, because global tem-



WASHINGTON, D.C.
The capital would suffer significant flooding from the combined impact of a two-foot rise in sea levels and a storm of the kind expected once every 50 years. Such storms could become more common as ocean temperatures rise.



MARTHA'S VINEYARD
A three-foot sea-level rise could devastate Martha's Vineyard, the tony Massachusetts beach community that has been a favorite getaway for the Clintons and the Kennedys.

peratures will increase by 2 to 3 degrees Celsius by the end of this century. "The last time the earth was that warm, sea levels were 80 feet higher than today," he says. It will likely take hundreds of years for sea levels to rise the full 80 feet, but the process would be irreversible, and the rises would not be gradual. "You're going to be continually faced with a changing coastline, which will force coastal dwellers to constantly relocate," he says.

This article's smaller, aerial-view illustrations are based on simulations by the National Environmental Trust, a nonprofit group in Washington, D.C. N.E.T. relied on data from the I.P.C.C., the U.S. Geological Survey, and the N.O.A.A. Additional N.E.T. simulations are available at net.org. Philip Clapp, N.E.T.'s president, says, "The U.S. government has never released its own simulations. The Bush administration doesn't want these pictures in front of the American people because they show that a three-foot sea-level rise plus storm flooding would have catastrophic consequences."

"It's getting harder and harder to say we'll
AVOID A THREE-FOOT SEA-LEVEL RISE,"
says Michael Oppenheimer, a professor of
GEOSCIENCES AT PRINCETON.

In New York, it would leave much of Lower Manhattan, including the Ground Zero memorial and the entire financial district, underwater. La Guardia and John F. Kennedy airports would meet the same fate. In Washington, D.C., the Potomac River would swell dramatically, stretching all the way to the Capitol lawn and to within two blocks of the White House.

Since roughly half the world's 6.5 billion people live near coastlines, a three-foot sea-level rise would be even more punishing overseas. Amsterdam, Venice, Cairo, Shanghai, Manila, and Calcutta are some of the cities most threatened. In many places the people and governments are too poor to erect adequate barriers—think of low-lying Bangladesh, where an estimated 18 million people are at risk—so experts fear that they will migrate to neighboring lands, raising the prospect of armed conflict. A Pentagon-commissioned study warned in 2003 that climate change could bring mega-droughts, mass starvation, and even nuclear war as countries such as China, India, and Pakistan battle over scarce food and water.

These are just some of the reasons why David King wrote in *Science* in 2004, "Climate change is the most severe problem that we are facing today—more serious even than the threat of terrorism." King's comment raised hackles in Washington and led a top press aide to Tony Blair to try to muzzle him. But the science adviser tells me he "absolutely" stands by his statement. By no means does King underestimate terrorism; advising the British government on that threat, he says, "is a very important part of my job." But the hazards presented by climate change are so severe and far-reaching that, in his view, they overshadow not only every other environmental threat but every other threat, period.

"Take India," King says. "Their monsoon is a fact of life that they have developed their agricultural economy around. If the monsoon is down by 10 percent one year, they have massive losses of crops. If it's 10 percent over, they have massive flood problems. [If climate change ends up] switching off the monsoon in India, or even changing it outside those limits, it would lead to massive global economic de-stabilization. The kind of situation we need to avoid creating is one where populations are so de-stabilized—Bangladesh being flooded, India no food—that they're all seeking alternative habitats. These, in our globalized economy, would be very difficult for all of us to manage."

The worst scenarios of global warming might still be avoided, scientists say, if humanity reduces its greenhouse-gas emissions dramatically, and very soon. The I.P.C.C. has estimated that emissions must fall to 60 percent below 1990 levels before 2050, over a period when global population is expected to increase by 37 percent and per-capita energy consumption will surely rise as billions of people in Asia, Africa, and South America strive to ascend from poverty.

Yet even if such a reduction were achieved, a significant rise in sea levels may be unavoidable. "It's getting harder and harder to say we'll avoid a three-foot sea-level rise, though it won't necessarily happen in this century," says Michael Oppenheimer, a professor of geosciences and international affairs at Princeton. Oppenheimer's pessimism is rooted in the lag effects of the climate system: oceans store heat for a century or longer before releasing it; carbon dioxide remains in the atmosphere for decades or longer before dissipating.

According to King, even if humanity were to stop emitting carbon dioxide today, "temperatures will keep rising and all the impacts will keep changing for about 25 years."

The upshot is that it has become too late to prevent climate change; we can only adapt to it. This unhappy fact is not well understood by the general public; advocates downplay it, perhaps for fear of fostering a paralyzing despair. But there is no getting around it: because humanity waited so long to take decisive action, we are now stuck with a certain amount of global warming and the climate changes it will bring—rising seas, fiercer heat, deeper droughts, stronger storms. The World Health Organization estimates that climate change is already helping to kill 150,000 people a year, mainly in Africa and Asia. That number is bound to rise as global warming intensifies in the years ahead.

The inevitability of global warming does not mean we should not act, King emphasizes: "The first message to our political leaders is, action is required. Whether or not we get global agreement to reduce emissions, we all need to adapt to the impacts that are in the pipeline." That means doing all the things that were not done in New Orleans: building sound levees and seawalls, restoring coastal wetlands (which act like speed bumps to weaken hurricanes' storm surges), strengthening emergency-preparedness networks and health-care systems, and much more.

Beyond this crucial first step—which most governments worldwide have yet to consider—humanity can cushion the severity of future global warming by limiting greenhouse-gas

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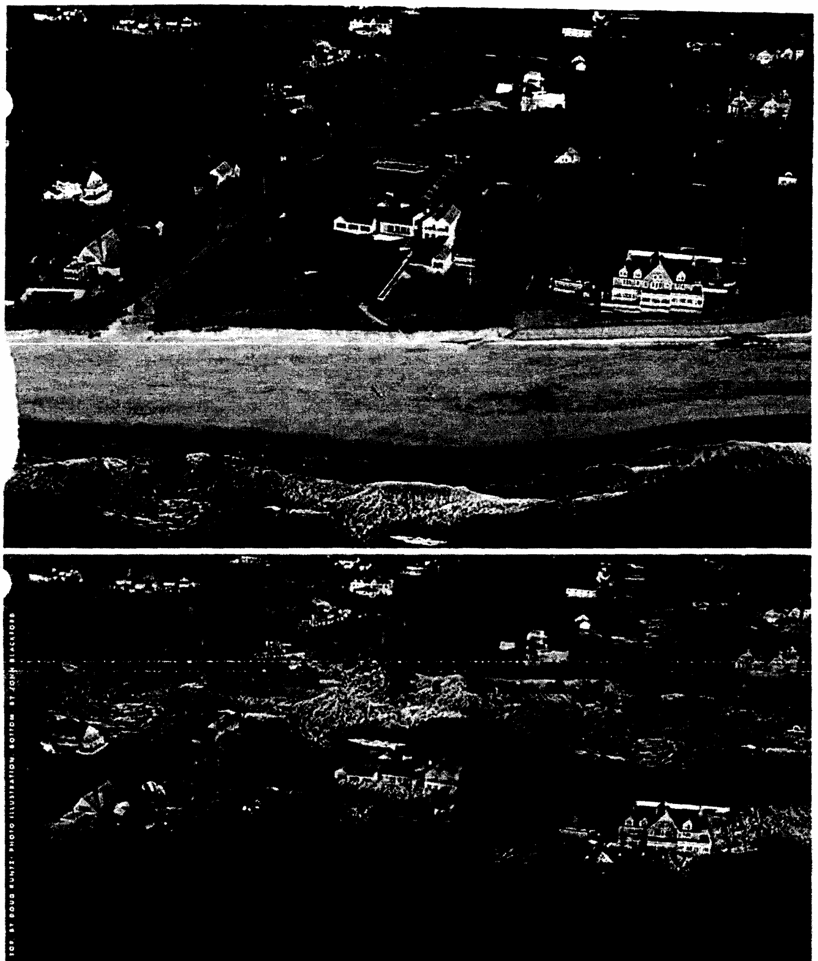


SUNKEN TREASURE

The Intergovernmental Panel on Climate Change has warned that global warming could add three feet to sea levels by 2100 and increase the strength and frequency of major storms. A three-foot sea-level rise combined with a Category 3 hurricane would do serious damage to the pricey homes of the Hamptons.

ORIGINAL PHOTOGRAPH, TOP, BY DOUG KUNTZ; PHOTO ILLUSTRATION, BOTTOM, BY JOHN BLACKFORD

Boston Strangler

were in apartment buildings where many people came and went and residents might not be surprised if a maintenance man knocked on their door. But this was a house in the suburbs, where a stranger would stand out immediately because everyone on the street knew one another by their first names. Once you have DeSalvo in the house the crime is pure Boston Strangler, but how do you get him there? And why would a killer who seemed to have developed such a perfect technique for killing women suddenly abandon it for something far riskier?

Tuney and Delaney parked on Scott Road and walked around the Goldberg house, noting where the front and back doors were and how far Smith had to walk to get to the bus stop on Pleasant Street. One of the first things that struck Delaney was that the Goldberg house was easily approached from the back; it was a route, in fact, that neighborhood children said they used as a shortcut. If a killer wanted to enter the Goldberg house unseen from Scott Road, all he had to do was cross behind the Hartunians' house on the corner of Pleasant Street and walk about 120 feet to the Goldbergs' backyard. Workmen would not ordinarily use the front door of a house like the Goldbergs', so Bessie might not be suspicious if a man knocked on her kitchen door and said, for example, that he worked for the Belmont water department and wanted to check her meter.

If Delaney was the idealist of the two, Tuney was the seasoned pragmatist. He'd been in police work long enough to know that the politics of a case are everything, and that if you ignore them you'll get nowhere. Consequently, the first thing he'd done on the way to Scott Road was to stop at the Belmont Police Department and let the police chief know they were in the area. It wasn't required, but it was a matter of respect, and it may have been a courtesy that paid off. Delaney is not positive where they got this information, but he believes it was from someone at the department: apparently a neighbor of the Goldbergs' had seen a suspicious person on Scott Road on the afternoon of the murder and had called the Belmont police with the information, but the police had not followed up on it. The lead, such as it was, now belonged to Tuney and Delaney.

The neighbor turned out to be an elderly man with a bedridden wife, and Delaney has a memory of standing back while Tuney asked the man to repeat his story. On the afternoon that Bessie Goldberg was killed, the neighbor said, he'd been approached by a man in work clothes who had offered to paint his house as a side job on weekends. The man was white and probably in his 30s and—in Delaney's mind, at least—roughly matched a description of DeSalvo. The old man said he declined the work offer by saying that a private nurse he'd hired to help his wife needed him back in the house. The incident had stuck in his mind, though, and an hour later—when he saw police cars and an ambulance on Scott Road—he called the police department.

By then, however, every cop in Massachusetts was already looking for Roy Smith, and a white man walking around a white neighborhood knocking on doors would have meant absolutely nothing. That was, however, something that DeSalvo said he often did to find weekend work. Maybe he knocked on the Goldbergs' door and Bessie opened, Delaney thought. Maybe she let him in. Maybe he said he needed to check her water meter or offered to paint her living room. Maybe she just turned away for a moment and he was on her. It was a classic Boston Strangling except that DeSalvo never confessed to it and Roy Smith was convicted of it; in every other respect it was identical to the 13 murders DeSalvo claimed to have committed.

Delaney and Tuney finished up on Scott Road and drove back to Boston without anything concrete to report. It was a delicate line of inquiry anyway—what with Smith's case under appeal and the attorney general himself warned away from making any awkward comparisons to other murders. It was a case, however, that Delaney never managed to get out of his head.

Roy Smith died of lung cancer 13 years into a life sentence. Two days earlier, a governor's commutation—effective immediately—had been handed to him at his hospital bed. It was unheard of for a lifer to be considered for commutation after only 10 years, and the only explanation was that a lot of people had to have doubts about Smith's guilt. DeSalvo was never linked to the Goldberg murder, but some thought it odd that he was stabbed to death within days of the 10-year anniversary of Smith's conviction for the crime. □

Three Feet of Water

CONTINUED FROM PAGE 206

emissions. Hansen says we must stabilize emissions—which currently are rising 2 percent a year—by 2015, and then reduce them. *Avoiding Dangerous Climate Change*, a book based on a scientific conference convened by Tony Blair before the G-8 summit, estimates that we may have until 2025 to peak and reduce.

The goal is to stop global warming before it crosses tipping points and attains unstoppable momentum from "positive feedbacks." For example, should the Greenland ice sheet melt, white ice—which reflects sunlight back into space—would be replaced by dark water, which absorbs sunlight and drives further warming.

Positive feedbacks can trigger the kind of abrupt, irreversible climate changes that scientists call "nonlinear." Once again, Hurricane Katrina provides a sobering preview of what that means. "Hurricanes are the mother of all nonlinear events, because small changes in initial conditions can lead to enormous changes in outcomes," says Hans Joachim Schellnhuber, the director of the Potsdam Institute for Climate Impact Research and the former chief environmental adviser to the German government. "A few percent increase in a hurricane's wind speed can double its destructiveness under certain circumstances."

Although scientists apply the neutral term "climate change" to all of these phenomena, "climate chaos" better conveys the abrupt, interconnected, wide-ranging consequences that lie in store. "It's a very appropriate term for the layperson," says Schellnhuber, a physicist who specializes in chaos theory. "I keep telling politicians that I'm not so concerned about a gradual climate change that may force farmers in Great Britain to plant different crops. I'm worried about triggering positive feedbacks that, in the worst case, could kick off some type of runaway greenhouse dynamics."

Among the reasons climate change is a bigger problem than terrorism, David King tells me, is that the problem is rooted in humanity's burning of oil, coal, and natural gas, "and people don't want to let that go." Which is understandable. These carbon-based fuels have powered civilization since the dawn of the industrial era, delivering enormous wealth, convenience, and well-being even as they overheated the atmosphere. Luckily, the idea that reducing greenhouse-gas emissions will wreck our economy, as President Bush said in 2005 when defending his opposition to the Kyoto Protocol, is disproved by experience. "In Britain," King told the environmental Web site Grist, "our economy since 1990 has grown by about 40 percent, and our emissions have decreased by 14 percent."

Ultimately, society must shift onto a new energy foundation based on alternative fuels, not only because of global warming but also because oil "will get harder and costlier to find" in the years ahead, says Ronald Oxburgh, the former chairman of the British arm of Royal Dutch Shell oil. "The group around President Bush have been saying that, even if climate change is real, it would be terribly costly to shift away from carbon-based fuels," Oxburgh continues. "Of course it would, if you try to make the change overnight. But that's not how you do it. If governments make the decision to shift our society to a new energy foundation, and they make it clear to everyone this is what we're doing by laying out clear requirements and incentives, corporations will respond and get the job done."

The opening move in this transition is to invest massively in energy efficiency. Amory Lovins, co-founder of the Rocky Mountain Institute, a think tank that consults for corporations and governments around the world, has demonstrated that measures such as insulating buildings and driving more fuel-efficient vehicles could reduce humanity's consumption of energy and natural resources by a factor of four. And efficiency investments have a demonstrated record of creating jobs and boosting profits, suggesting that emissions can be reduced without crippling economies.

One of the first moves Angela Merkel announced as the new chancellor of Germany last fall was the extension of a Green Party initiative to upgrade energy efficiency in the nation's pre-1978 housing stock. Most of that housing is in the former East Germany, where unemployment approaches 20 percent. Replacing old furnaces and installing efficient windows and lights will produce thousands of well-paying laborers' jobs that by their nature cannot be outsourced.

Corporations, too, have discovered that energy efficiency can be profitable. Over a three-year period beginning in 1999, BP invested $20 million to reduce the emissions from its internal operations and saved $650 million—32 times the original investment.

Individuals can cash in as well. Although buying a super-efficient car or refrigerator may cost more up front, over time it saves the consumer money through lower energy bills.

Efficiency is no silver bullet, nor can it forever neutralize the effects of billions of people consuming more and more all the time. It can, however, buy humanity time to further develop and deploy alternative-energy technologies. Solar and wind power have made enormous strides in recent years, but the technology to watch is carbon sequestration, a method of capturing and then safely storing the carbon dioxide produced by the combustion of fossil fuels. In theory, sequestration would allow nations to continue burning coal—the most abundant fuel in the world, and the foundation of the Chinese and Indian economies—without worsening the climate problem. "If carbon capture is not feasible, our choices are much less good, and the cost of climate change is going to be much higher," says Jeffrey D. Sachs, the director of the Earth Institute at Columbia University and a special adviser to the United Nations.

No one pretends that phasing out carbon-based fuels will be easy. The momentum of the climate system means that "a certain amount of pain is inevitable," says Michael Oppenheimer. "But we still have a choice between pain and disaster."

Unfortunately, we are getting a late start, which is something of a puzzle. The threat of global warming has been recognized at the highest levels of government for more than 25 years. Former president Jimmy Carter highlighted it in 1980, and Al Gore championed it in Congress throughout the 1980s. Margaret Thatcher, the arch-conservative prime minister of Britain from 1979 to 1990, delivered some of the hardest-hitting speeches ever given on climate change. But progress stalled in the 1990s, even as Gore was elected vice president and the scientific case grew definitive. It turned out there were powerful pockets of resistance to tackling this problem, and they put up a hell of a fight.

Call him the $45 million man. That's how much money Dr. Frederick Seitz, a former president of the National Academy of Sciences, helped R. J. Reynolds Industries, Inc., give away to fund medical research in the 1970s and 1980s. The research avoided the central health issue facing Reynolds—"They didn't want us looking at the health effects of cigarette smoking," says Seitz, who is now 94—but it nevertheless served the tobacco industry's purposes. Throughout those years, the industry frequently ran ads in newspapers and magazines citing its multi-million-dollar research program as proof of its commitment to science—and arguing that the evidence on the health effects of smoking was mixed.

In the 1990s, Seitz began arguing that the science behind global warming was likewise inconclusive and certainly didn't warrant imposing mandatory limits on greenhouse-gas emissions. He made his case vocally, trashing the integrity of a 1995 I.P.C.C. report on the op-ed page of *The Wall Street Journal*, signing a letter to the Clinton administration accusing it of misrepresenting the science, and authoring a paper which said that global warming and ozone depletion were exaggerated threats devised by environmentalists and unscrupulous scientists pushing a political agenda. In that same paper, Seitz asserted that secondhand smoke posed no real health risks, an opinion he repeats in our interview. "I just can't believe it's that bad," he says.

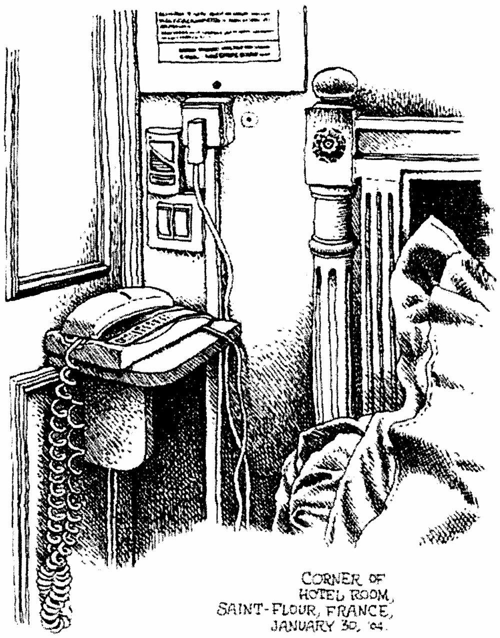
Al Gore and others have said, but generally without offering evidence, that the people who deny the dangers of climate change are like the tobacco executives who denied the dangers of smoking. The example of Frederick Seitz, described here in full for the first time, shows that the two camps overlap in ways that are quite literal—and lucrative. Seitz earned approximately $585,000 for his consulting work for R. J. Reynolds, according to company documents unearthed by researchers for the Greenpeace Web site ExxonSecrets.org and confirmed by Seitz. Meanwhile, during the years he consulted for Reynolds, Seitz continued to draw a salary as president emeritus at Rockefeller University, an institution founded in 1901 and subsidized with profits from Standard Oil, the predecessor corporation of ExxonMobil.

Seitz was the highest-ranking scientist among a band of doubters who, beginning in the early 1990s, resolutely disputed suggestions that climate change was a real and present danger. As a former president of the National Academy of Sciences (from 1962 to 1969) and a winner of the National Medal of Science, Seitz gave such objections instant credibility. Richard Lindzen, a professor of meteorology at M.I.T., was another high-profile scientist who consistently denigrated the case for global warming. But most of the public argument was carried by lesser scientists and, above all, by lobbyists and paid spokesmen for the Global Climate Coalition. Created and funded by the energy and auto industries, the Coalition spent millions of dollars spreading the message that global warming was an uncertain threat. Journalist Ross Gelbspan exposed the corporate campaign in his 1997 book, *The Heat Is On*, which quoted a 1991 strategy memo: the goal was to "reposition global warming as theory rather than fact."

"Not trivial" is how Seitz reckons the influence he and fellow skeptics have had, and their critics agree. The effect on media coverage was striking, according to Bill McKibben, who in 1989 published the first major popular book on global warming, *The End of Nature*. Introducing the 10th-anniversary edition, in 1999, McKibben noted that virtually every week over the past decade studies had appeared in scientific publications painting an ever more alarming picture of the global-warming threat. Most news reports, on the other hand, "seem to be coming from some other planet."

The deniers' arguments were frequently cited in Washington policy debates. Their most important legislative victory was the Senate's 95-to-0 vote in 1997 to oppose U.S. participation in any international agree-

SKETCHBOOK BY R. CRUMB



Three Feet of Water

ment—i.e., the Kyoto Protocol—that imposed mandatory greenhouse-gas reductions on the U.S.

The ferocity of this resistance helps explain why the Clinton administration achieved so little on climate change, says Tim Wirth, the first under-secretary of state for global affairs, who served as President Clinton's chief climate negotiator. "The opponents were so strongly organized that the administration got spooked and backed off of things it should have done," says Wirth. "The Kyoto negotiations got watered down and watered down, and after we signed it the administration didn't try to get it ratified. They didn't even send people up to the Hill to talk to senators about ratifying it."

"I wanted to push for ratification," responds Gore. "A decision was made not to. If our congressional people had said there was even a remote chance of ratifying, I could have convinced Clinton to do it—his heart was in the right place.... But I remember a meeting in the White House with some environmental groups where I asked them for the names of 10 senators who would vote to ratify. They came up with one, Paul Wellstone. If your most optimistic supporters can't identify 10 likely gettables, then people in the administration start to ask, 'Are you a fanatic, Al? Is this a suicide mission?'" (Clinton did not respond to e-mailed questions.)

James Hansen, without singling out any individual, accuses global-warming deniers of "acting like lawyers, not scientists, because no matter what new evidence comes in, their conclusion is already decided." Richard Lindzen responds that Hansen has been wrong time and time again and operates "one of the worst climate models around." Lindzen agrees that both global temperature and atmospheric concentration of carbon dioxide have increased over the last century. But temperatures won't rise much further, he says, because humans aren't the main driving force in the climate system. The reason most scientists disagree with him, Lindzen explains, is simple careerism. "Once President Bush the elder began spending $2 billion a year on climate science, scientists developed a self-interest in maintaining this is an urgent problem," he says, adding that the scientific community's fixation on climate change will be remembered as an episode of "mass insanity."

Among many rebuttals to the deniers' arguments, perhaps the most authoritative collection is found on the Web site of Britain's national academy of science, the Royal Society. But such rebuttals have little impact on true believers, says Robert May, the Society's former president. "[Nobel Prize–winning physicist] Max Planck used to say that people don't change their minds [because of evidence]," he adds. "The science simply moves on and those people eventually die off."

But if the deniers appear to have lost the scientific argument, they prolonged the policy battle, delaying actions to reduce emissions when such cuts mattered most. "For 25 years, people have been warning that we had a window of opportunity to take action, and if we waited until the effects were obvious it would be too late to avoid major consequences," says Oppenheimer. "Had some individual countries, especially the United States, begun to act in the early to mid-1990s, we might have made it. But we didn't, and now the impacts are here."

"The goal of the disinformation campaign wasn't to win the debate," says Gelbspan. "The goal was simply to keep the debate going. When the public hears the media report that some scientists believe warming is real but others don't, its reaction is 'Come back and tell us when you're really sure.' So no political action is taken."

Representative Henry Waxman, the California Democrat who chaired the 1994 hearings where tobacco executives unanimously declared under oath that cigarettes were not addictive, watches today's global-warming deniers with a sense of déjà vu. It all reminds him of the confidential slogan a top tobacco flack coined when arguing that the science on smoking remained unsettled: "Doubt is our product." Now, Waxman says, "not only are we seeing the same tactics the tobacco industry used, we're seeing some of the same groups. For example, the Advancement of Sound Science Coalition was created [in 1993] to debunk the dangers of secondhand smoking before it moved on to global warming."

The scientific work Frederick Seitz oversaw for R. J. Reynolds from 1978 to 1987 was "perfectly fine research, but off the point," says Stanton A. Glantz, a professor of medicine at the University of California, San Francisco, and a lead author of *The Cigarette Papers* (1996), which exposed the inner workings of the Brown & Williamson Tobacco Corporation. "Looking at stress, at genetics, at lifestyle issues let Reynolds claim it was funding real research. But then it could cloud the issue by saying, 'Well, what about this other possible causal factor?' It's like coming up with 57 other reasons for Hurricane Katrina rather than global warming."

For his part, Seitz says he was comfortable taking tobacco money, "as long as it was green. I'm not quite clear about this moralistic issue. We had absolutely free rein to decide how the money was spent." Did the research give the tobacco industry political cover? "I'll leave that to the philosophers and priests," he replies.

Seitz is equally nonplussed by the extraordinary disavowal the National Academy of Sciences issued following his most visible intervention in the global-warming debate. In 1998 he urged fellow scientists to sign an Oregon group's petition saying that global warming was much ado about little. The petition attracted more than 17,000 signatories and received widespread media attention. But posted along with the petition was a paper by four global-warming deniers that was presented in virtually the same layout and typeface used by the National Academy of Sciences in its scholarly journal. The formatting, combined with Seitz's signature, gave the clear impression that the academy endorsed the petition. The academy quickly released a statement disclaiming any connection with the petition or its suggestion that global warming was not real. *Scientific American* later determined that only 1,400 of the petition's signatories claimed to hold a Ph.D. in a climate-related science, and of these, some either were not even aware of the petition or later changed their minds.

Today, Seitz admits that "it was stupid" for the Oregon activists to copy the academy's format. Still, he doesn't understand why the academy felt compelled to disavow the petition, which he continues to cite as proof that it is "not true" there is a scientific consensus on global warming.

The accumulation of scientific evidence eventually led British Petroleum to resign from the Global Climate Coalition in 1996. Shell, Ford, and other corporations soon left as well, and in 2002 the coalition closed down. But Gelbspan, whose Web site tracks the deniers' activities, notes that key coalition personnel have since taken up positions in the Bush administration, including Harlan Watson, the State Department's chief climate negotiator. (Watson declined to be interviewed.)

ExxonMobil -long the most recalcitrant corporation on global warming—is still spending millions of dollars a year funding an array of organizations that downplay the problem, including the George C. Marshall Institute, where Seitz is chairman emeritus. John Passacantando, executive director of Greenpeace USA, calls the denial campaign "one of the great crimes of our era." Passacantando is "quite confident" that class-action lawsuits will eventually be filed against corporations who denied global warming's dangers. Five years ago, he told executives from one company, "You're going to wish you were the tobacco companies once this stuff hits and people realize you were the ones who blocked [action]."

The public discussion about climate change in the U.S. is years behind that in Britain and the rest of Europe, and the deniers are a big reason why. "In the United States, the Chamber of Commerce and National Association of Manufacturers are deeply skeptical of climate-change science and the need to reduce greenhouse-gas emissions," says Fiona Harvey, the environment correspondent for the *Financial Times*. "In Britain, the equivalent body, the Confederation of British Industry, is absolutely behind the science and agrees on the need to cut emissions. The only differences are over how to do that."

America's media coverage is also well behind the curve, says Harvey. "In the United States you have lots of news stories that, in the name of balance, give equal credence to the skeptics. We don't do that here—not because we're not balanced but because we think it's unbalanced to give equal validity to a fringe few with no science behind them."

Prominent right-wing media outlets in the U.S., especially the editorial page of *The Wall Street Journal*, continue to parrot the claims of climate-change deniers. (Paul A. Gigot, the page's editor, declined to be interviewed.) Few beat reporters are still taken in, but their bosses—the editors and producers who decide which stories run, and how prominently—are another matter. Charles Alexander, the former environmental editor at *Time*, complains that, while coverage has improved recently, media executives continue to regard climate change as just another environmental issue, rather than as the overriding challenge of the 21st century.

"Americans are hearing more about reducing greenhouse emissions from BP ads than from news stories in *Time*, *The New York Times*, or any other U.S. media outlet," Alexander says. "This will go down as the greatest act of mass denial in history."

In 2002, Alexander went to see Andrew Heyward, then the president of CBS News, after running into him at a Harvard reunion. "I talked to him about climate change and other global environmental threats, and made the case that they were more dangerous than terrorism and CBS should be doing much more coverage of them," Alexander recalls. "He didn't dispute any of my factual points, but he did say the reason CBS didn't do more of that coverage was that 'people don't want to hear all that gloom and doom'—in other words, the environment wasn't a ratings winner. He seemed to think CBS News's job was to tell people what they wanted to hear, not what they need to know, and I think that attitude is increasingly true for the news business in general."

"That's bullshit," responds Heyward, who left CBS in 2005. "I've never been one of those guys who thinks news has to be light and bright. And in talking to Charles, I wasn't stating the policy of CBS News. I was just trying to explain to an old college classmate why there isn't more coverage of the environment on TV. Charles is an advocate, and advocates are never happy with the amount of coverage their cause gets."

American television did, however, give prime-time coverage to the latest, and most famous, global-warming denier: novelist Michael Crichton. ABC's *20/20* broadcast a very friendly interview with Crichton when he published *State of Fear*, a novel arguing that anyone who bought into the phony scientific consensus on global warming was a modern equivalent of the early-20th-century eugenicists who cited scientific "proof" for the superiority of the white race.

When Crichton was invited to testify before the Environment and Public Works Committee, observers in Britain were floored. "This is fairyland," exclaims Michael Meacher, the member of Parliament who served as Tony Blair's environment minister from 1997 to 2003. "You have a science-fiction writer testifying before the United States Senate on global-warming policy? I mean, you can almost see the little boy off to the side, like in the story of the emperor's clothes, saying, 'But he's a science-*fiction* writer, isn't he?' It's just ludicrous."

The man who invited Crichton, committee chairman James M. Inhofe, a Republican from oil-rich Oklahoma, had already said on the floor of the Senate that global warming was "the greatest hoax ever perpetrated on the American people." In an e-mail interview, Inhofe defended Crichton's appearance, noting that the writer holds a medical degree from Harvard. (Crichton is also a post-doctoral fellow at the Salk Institute for Biological Studies.) The senator added that he stood by his hoax statement as well.

David King responded that Britain's climate-science research is headquartered within the Ministry of Defense, "and you wouldn't find a group of people less likely to perpetrate a hoax than the people in the Ministry of Defense."

King has "extremist views," Inhofe replied. If the I.P.C.C. and the world's leading academies of science echo King's views, he argued, it is because they actively silence dissidents: "Scientists who believe warming trends are naturally occurring, or benign, are almost always excluded from climate-change conferences and meetings because their conclusions do not support the political agendas of the others who host the conferences." (The I.P.C.C. denies this accusation.) The truth, Inhofe continued, is that "there is no consensus on the science of global warming." As proof, he cited—what else?—Frederick Seitz's Oregon petition.

Paul H. O'Neill, who served nearly two years as George W. Bush's secretary of the Treasury, does not buy the common notion that Bush and Vice President Dick Cheney resist taking action on global warming because they are oilmen. "I don't think either one of them is an oilman," insists O'Neill. "You have to have success to be an oilman. It's like saying you're a ballplayer, but you never got on the field."

In 1998, while running the aluminum giant Alcoa, O'Neill was among the first U.S. business leaders to recognize the enormity of climate change. He says Bush asked him, early in the first term, to put together a plan of action, but it was ignored. Like Bush, O'Neill opposed Kyoto, so he proposed other ways to move forward. But instead, he says, the administration "cherry-picked" the science on climate change to justify taking no action, "just like it cherry-picked the intelligence on weapons of mass destruction" to justify the invasion of Iraq.

"The United States is the only entity on this planet turning its back on this problem," says Massachusetts senator John Kerry. "Even as he talks about protecting the security of the nation, the president is willfully choosing not to tackle this problem. History will record it as one of the greatest derelictions of duty ever."

Bush-administration officials counter that they are doing more to fight global warming than anyone else—just with different tools than those favored by supporters of the Kyoto Protocol. James L. Connaughton, the head of the White House Council on Environmental Quality, starts by pointing out that Bush has raised federal mileage standards for S.U.V.'s and light trucks. When I point out that the increase is tiny (a mere 0.3 miles per gallon, says Dan Becker of the Sierra Club), Connaughton maintains that over time further increases will result in substantial energy savings, especially when paired with the administration's new tax credits for efficient vehicles. It's also important, he says, to "keep personal income taxes in check" to encourage people to buy these new cars. What's more, the administration recently provided $10 billion in incentives for alternative-energy development and $40 billion over 10 years to encourage farmers to plant trees and preserve grassland that can soak up carbon dioxide.

The administration opposes the Kyoto Protocol, Connaughton claims, because its mandatory emissions cuts would punish the American economy, costing as many as five million jobs. It would also dry up the capital needed to fund the technological research that will ultimately solve global warming.

"It's important not to get distracted by chasing short-term reductions in greenhouse emissions. The real payoff is in long-term technological breakthroughs," says John H. Marburger III, the president's science adviser. Besides, "there is no question that mitigating the impact of climate change as it takes place will be much less [expensive] than the costs of reducing oil and coal use in the short term."

"The world is now on a trajectory to slow the growth in greenhouse-gas emissions," concludes Connaughton, who as a lawyer represented mining and chemical interests before joining the administration. "I'm highly confident we will stabilize [those emissions]." He says that's exactly what happened over the last 80 years with air pollution. He seems to take pleasure in observing that, under Bush, the U.S. has actually reduced its annual emissions, which, he says, is more than some of its harshest critics overseas have done.

It's a cheerful story, but virtually no one else believes it. Waiting 80 years to eliminate greenhouse-gas emissions would guarantee runaway global warming, says James Hansen. In January, six former chiefs of the Environmental Protection Agency, including five who served Republican presidents, said Bush needed to do much more to fight climate change. In Britain, Peter Ainsworth, the Conservative Party's shadow secretary of state for the environment, says his party is "saddened" by the Bush administration's approach. "We would have preferred the Bush administration to take a leadership position on this problem … instead of allowing itself to be seen as foot-dragging."

Outsiders doubt President Bush's desire to confront the issue, pointing out that his right-wing political base agrees with Inhofe that global warming is a liberal hoax. Critics also question the administration's faith in volunteerism. They argue that imposing mandatory timelines and emissions limits would put a price tag on carbon and push corporations and individuals to use less of it. "Long-term research is fine, but to offer that as a substitute for the stark necessity of near-term cuts in emissions is a kind of magical thinking—trusting that something will happen to make everything all right," says Donald Kennedy, the editor in chief of *Science*. In fact, despite Bush's call to end our "addiction" to oil, his 2007 budget actually reduced funding for alternative energy and efficiency.

Nor has the Bush administration cut short-term emissions, says a European diplomat who requested anonymity because he has to work with Bush officials. Citing data from the Energy Information Administration, the diplomat says Connaughton is correct to say that U.S. greenhouse-gas emissions declined, but only in the single year following the 2001 terrorist attacks, owing to the ensuing economic recession. U.S. emissions increased in every other year of Bush's presidency, making it "complete hokum" to claim that Bush's policies are cutting emissions, the diplomat says, adding of Connaughton, "I'm afraid Jim has drunk the Kool-Aid."

As for John Marburger's assertion that it will be cheaper to adapt to climate change than to try to head it off, Michael Oppenheimer says, "It's a sad day when the president is being told by his science adviser that climate change isn't worth avoiding. It may be possible for rich nations and people to adapt, but 90 percent of humanity doesn't have the resources to deal with climate change. It's unethical to condemn them just because the people in power don't want to act."

"I think it is a slam dunk that we are on a path of dangerous anthropogenic interference with the climate, and it is also absolutely clear that what this administration has proposed so far will not get us off that path," says Jeffrey Sachs. "The administration says several things I agree with: technology is extremely important, global warming is a long-term issue, and we can't do it without China and India [because their greenhouse-gas emissions will soon outstrip our own]. But none of this adds up to taking no action. The fact that China and other developing economies have to be involved doesn't mean the United States refuses to commit to specific actions; it means the U.S. should commit itself, in part to help bring the others in.

"I've had discussions with leaders in China and India," adds Sachs. "They are very concerned about climate change because they see the effects it could have on them. We should help to set up prototype carbon-capture-and-sequestration power plants in China and India, and the rich countries should help to finance them. It's hard to ask poor countries to bear the full financial burden of these technologies, especially when it is the rich countries' past burning of carbon fuels that has created most of the problem. But the U.S. takes every opportunity to do virtually nothing to engage in practical steps with the developing countries."

Ask Al Gore how to avoid dangerous climate change and, despite his wonkish reputation, he doesn't begin by talking about hybrid cars or carbon sequestration. No, says Gore, the first imperative is to "punch through the massive denial and resistance" that still exist in the United States.

But the rest of the world is no longer waiting for the Bush administration. At the international climate conference held in Montreal last year, European nations called the administration's bluff when it refused to commit even to the breathtakingly modest step of someday discussing what framework might follow the Kyoto Protocol, which expires in 2012. At past summits, the administration's stubbornness led other nations to back down in hopes of keeping America involved in the process. At Montreal, the world quit waiting for Godot and recognized, as Elliot Morley, Tony Blair's minister of the environment, says, "there are a lot of voices in the United States in addition to the Bush administration, and we will work with all of them to address this problem."

The same thing is happening inside the U.S. "It is very clear that Congress will put mandatory greenhouse-gas-emission reductions in place, immediately after George W. Bush leaves office," says Philip Clapp of N.E.T. "Even the Fortune 500 is positioning itself for the inevitable. There isn't one credible 2008 Republican presidential candidate who hasn't abandoned the president's do-nothing approach. They have all adopted the approach the rest of the world took at the Montreal talks—we're moving forward, you're a lame duck, and we have to deal with it."

Regardless of what happens in Washington, D.C., state and local governments across America are aggressively confronting the problem. Two hundred and eight mayors have committed their cities to meet or exceed the emissions reductions mandated by the Kyoto Protocol, and some have gone further. Governor Arnold Schwarzenegger has committed California to 30 percent cuts by 2020.

California officials have also held talks with their counterparts in Oregon and Washington about launching a so-called carbon-trading system like the one currently in force in Europe. Such a system allows efficient users to profit while wasteful users must pay for burning more fuel. A similar mechanism worked in the 1990s to dramatically reduce emissions of sulfur dioxide—the cause of acid rain at far less cost than industrialists or environmentalists anticipated.

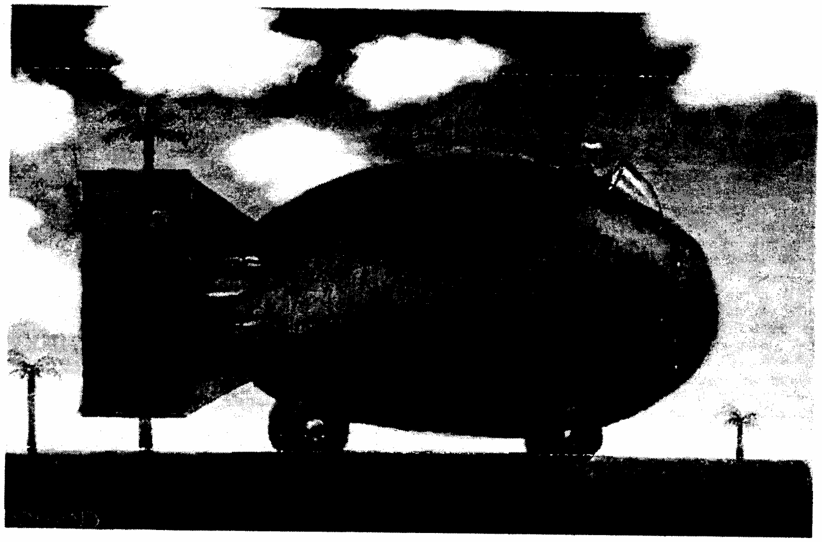
New York and seven other northeastern states, which together with California amount to the third-biggest economy in the world, are also considering a carbon-trading system. Their collective actions—investing in energy efficiency, installing wind turbines, sequestering carbon could boost production runs and lower costs to the point where the green technologies needed to fight global warming become affordable for everyone.

At the same time, investors and others worried about global warming are pressuring corporations and Wall Street to take the problem seriously. The Investor Network on Climate Risk, a coalition of pension-fund managers and institutional investors representing $3 trillion in assets, has put corporations on notice that its members will reconsider investing in companies that don't pay enough attention to climate change. In 2005, investment-banking giant Goldman Sachs pledged to embrace carbon trading and invest $1 billion in renewable energy.

"To use a term coined by George W. Bush in the context of the Iraq war, I think this coalition of the willing might be much more successful than the Kyoto process," says Hans Schellnhuber. "I've been to a lot of these international conferences, and it's a pretty frustrating experience that usually produces little more than cheap talk. Whereas a true coalition of the willing can bring together regional governments, enterprises, and individuals and show that it is technologically and economically possible to take meaningful action."

No matter what happens, the global warming that past human activity has already unleashed will make this a different planet in the years ahead. But it could still be a livable, even hospitable, planet, if enough of us get smart in time. If we don't, three feet of water could be just the beginning. □

SKETCHBOOK: IRANIAN HYBRID CAR BY BRAD HOLLAND



TAB 19

Joint science academies' statement: Global response to climate change

Climate change is real

There will always be uncertainty in understanding a system as complex as the world's climate. However there is now strong evidence that significant global warming is occurring[1]. The evidence comes from direct measurements of rising surface air temperatures and subsurface ocean temperatures and from phenomena such as increases in average global sea levels, retreating glaciers, and changes to many physical and biological systems. It is likely that most of the warming in recent decades can be attributed to human activities (IPCC 2001)[2]. This warming has already led to changes in the Earth's climate.

The existence of greenhouse gases in the atmosphere is vital to life on Earth – in their absence average temperatures would be about 30 centigrade degrees lower than they are today. But human activities are now causing atmospheric concentrations of greenhouse gases – including carbon dioxide, methane, tropospheric ozone, and nitrous oxide – to rise well above pre-industrial levels. Carbon dioxide levels have increased from 280 ppm in 1750 to over 375 ppm today – higher than any previous levels that can be reliably measured (i.e. in the last 420,000 years). Increasing greenhouse gases are causing temperatures to rise; the Earth's surface warmed by approximately 0.6 centigrade degrees over the twentieth century. The Intergovernmental Panel on Climate Change (IPCC) projected that the average global surface temperatures will continue to increase to between 1.4 centigrade degrees and 5.8 centigrade degrees above 1990 levels, by 2100.

Reduce the causes of climate change

The scientific understanding of climate change is now sufficiently clear to justify nations taking prompt action. It is vital that all nations identify cost-effective steps that they can take now, to contribute to substantial and long-term reduction in net global greenhouse gas emissions.

Action taken now to reduce significantly the build-up of greenhouse gases in the atmosphere will lessen the magnitude and rate of climate change. As the United Nations Framework Convention on Climate Change (UNFCCC) recognises, a lack of full scientific certainty about some aspects of climate change is not a reason for delaying an immediate response that will, at a reasonable cost, prevent dangerous anthropogenic interference with the climate system.

As nations and economies develop over the next 25 years, world primary energy demand is estimated to increase by almost 60%. Fossil fuels, which are responsible for the majority of carbon dioxide emissions produced by human activities, provide valuable resources for many nations and are projected to provide 85% of this demand (IEA 2004)[3]. Minimising the amount of this carbon dioxide reaching the atmosphere presents a huge challenge. There are many potentially cost-effective technological options that could contribute to stabilising greenhouse gas concentrations. These are at various stages of research and development. However barriers to their broad deployment still need to be overcome.

Carbon dioxide can remain in the atmosphere for many decades. Even with possible lowered emission rates we will be experiencing the impacts of climate change throughout the 21st century and beyond. Failure to implement significant reductions in net greenhouse gas emissions now, will make the job much harder in the future.

Prepare for the consequences of climate change

Major parts of the climate system respond slowly to changes in greenhouse gas concentrations. Even if greenhouse gas emissions were stabilised instantly at today's levels, the climate would still continue to change as it adapts to the increased emission of recent decades. Further changes in climate are therefore unavoidable. Nations must prepare for them.

The projected changes in climate will have both beneficial and adverse effects at the regional level, for example on water resources, agriculture, natural ecosystems and human health. The larger and faster the changes in climate, the more likely it is that adverse effects will dominate. Increasing temperatures are likely to increase the frequency and severity of weather events such as heat waves and heavy rainfall. Increasing temperatures could lead to large-scale effects such as melting of large ice sheets (with major impacts on low-lying regions throughout the world). The IPCC estimates that the combined effects of ice melting and sea water expansion from ocean warming are projected to cause the global mean sea-level to rise by between 0.1 and 0.9 metres between 1990 and 2100. In Bangladesh alone, a 0.5 metre sea-level rise would place about 6 million people at risk from flooding.

Developing nations that lack the infrastructure or resources to respond to the impacts of climate change will be particularly affected. It is clear that many of the world's poorest people are likely to suffer the most from climate change. Long-term global efforts to create a more healthy, prosperous and sustainable world may be severely hindered by changes in the climate.

The task of devising and implementing strategies to adapt to the consequences of climate change will require worldwide collaborative inputs from a wide range of experts, including physical and natural scientists, engineers, social scientists, medical scientists, those in the humanities, business leaders and economists.

Conclusion

We urge all nations, in the line with the UNFCCC principles[4], to take prompt action to reduce the causes of climate change, adapt to its impacts and ensure that the issue is included in all relevant national and international strategies. As national science academies, we commit to working with governments to help develop and implement the national and international response to the challenge of climate change.

G8 nations have been responsible for much of the past greenhouse gas emissions. As parties to the UNFCCC, G8 nations are committed to showing leadership in addressing climate change and assisting developing nations to meet the challenges of adaptation and mitigation.

We call on world leaders, including those meeting at the Gleneagles G8 Summit in July 2005, to:

- Acknowledge that the threat of climate change is clear and increasing.
- Launch an international study[5] to explore scientifically-informed targets for atmospheric greenhouse gas concentrations, and their associated emissions scenarios, that will enable nations to avoid impacts deemed unacceptable.
- Identify cost-effective steps that can be taken now to contribute to substantial and long-term reduction in net global greenhouse gas emissions. Recognise that delayed action will increase the risk of adverse environmental effects and will likely incur a greater cost.
- Work with developing nations to build a scientific and technological capacity best suited to their circumstances, enabling them to develop innovative solutions to mitigate and adapt to the adverse effects of climate change, while explicitly recognising their legitimate development rights.
- Show leadership in developing and deploying clean energy technologies and approaches to energy efficiency, and share this knowledge with all other nations.
- Mobilise the science and technology community to enhance research and development efforts, which can better inform climate change decisions.

Notes and references

1 This statement concentrates on climate change associated with global warming. We use the UNFCCC definition of climate change, which is 'a change of climate which is attributed directly or indirectly to human activity that alters the composition of the global atmosphere and which is in addition to natural climate variability observed over comparable time periods'.

2 IPCC (2001). Third Assessment Report. We recognise the international scientific consensus of the Intergovernmental Panel on Climate Change (IPCC).

3 IEA (2004). World Energy Outlook 4. Although long-term projections of future world energy demand and supply are highly uncertain, the World Energy Outlook produced by the International Energy Agency (IEA) is a useful source of information about possible future energy scenarios.

4 With special emphasis on the first principle of the UNFCCC, which states: 'The Parties should protect the climate system for the benefit of present and future generations of humankind, on the basis of equity and in accordance with their common but differentiated responsibilities and respective capabilities. Accordingly, the developed country Parties should take the lead in combating climate change and the adverse effects thereof'.

5 Recognising and building on the IPCC's ongoing work on emission scenarios.

Academia Brasiliera de Ciências
Brazil

Royal Society of Canada,
Canada

Chinese Academy of Sciences,
China

Academié des Sciences,
France

Deutsche Akademie der Naturforscher
Leopoldina, Germany

Indian National Science Academy,
India

Accademia dei Lincei,
Italy

Science Council of Japan,
Japan

Russian Academy of Sciences,
Russia

Royal Society,
United Kingdom

National Academy of Sciences,
United States of America

TAB 20

http://www.nytimes.com/2006/05/24/opinion/24easterbrook.html?ei=5088&en=a4de3b888f17125a&ex=1306123200&partner=rssnyt&emc=rss&pagewanted=print

May 24, 2006
OP-ED CONTRIBUTOR

Finally Feeling the Heat

By GREGG EASTERBROOK

Washington

TODAY "An Inconvenient Truth," Al Gore's movie about the greenhouse effect, opens in New York and California. Many who already believe global warming is a menace will flock to the film; many who scoff at the notion will opt for Tom Cruise or Tom Hanks. But has anything happened in recent years that should cause a reasonable person to switch sides in the global-warming debate?

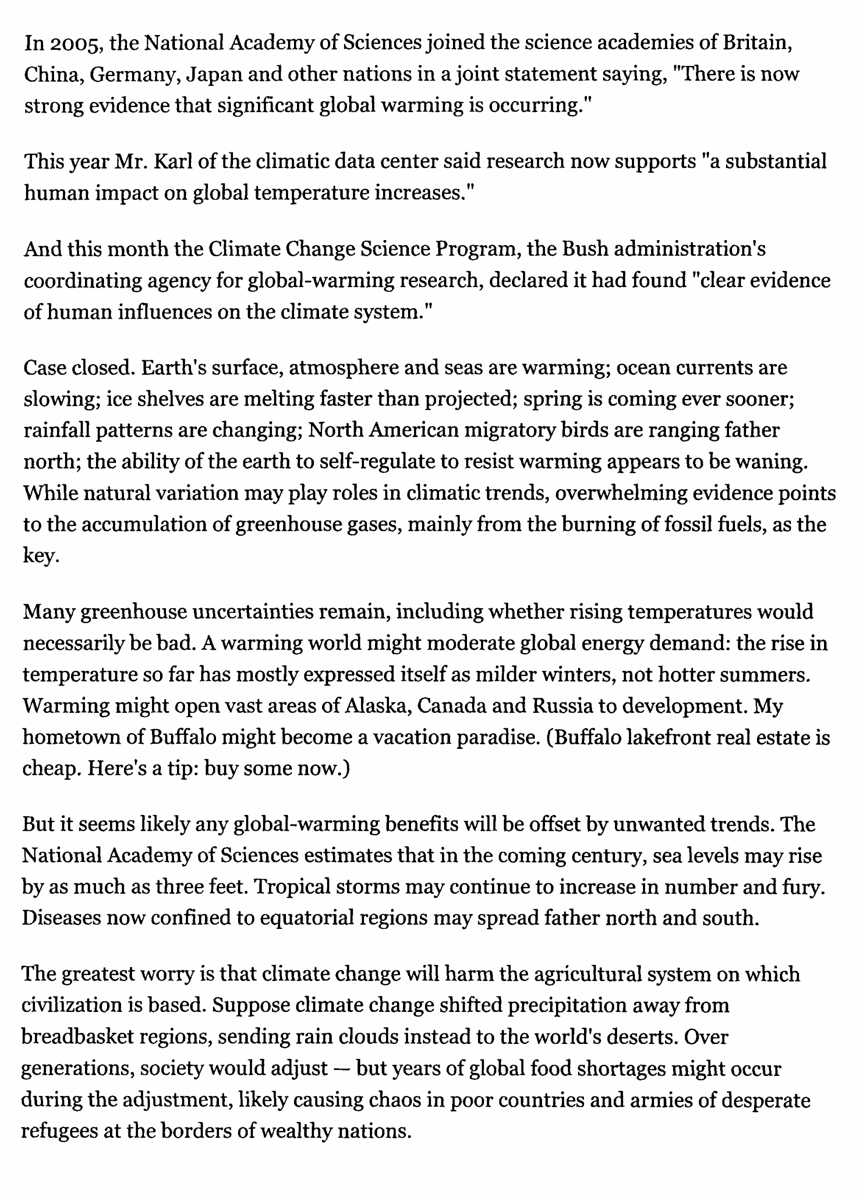
Yes: the science has changed from ambiguous to near-unanimous. As an environmental commentator, I have a long record of opposing alarmism. But based on the data I'm now switching sides regarding global warming, from skeptic to convert.

Once global-warming science was too uncertain to form the basis of policy decisions — and this was hardly just the contention of oil executives. "There is no evidence yet" of dangerous climate change, a National Academy of Sciences report said in 1991. A 1992 survey of the American Geophysical Union and the American Meteorological Society found that only 17 percent of members believed there was sufficient grounds to declare an artificial greenhouse effect in progress. In 1993 Thomas Karl, director of the National Climatic Data Center, said there existed "a great range of uncertainty" regarding whether the world is warming. Clearly, the question called for more research.

That research is now in, and it shows a strong scientific consensus that an artificially warming world is a real phenomenon posing real danger:

The American Geophysical Union and American Meteorological Society in 2003 both declared that signs of global warming had become compelling.

In 2004 the American Association for the Advancement of Science said that there was no longer any "substantive disagreement in the scientific community" that artificial global warming is happening.



In 2005, the National Academy of Sciences joined the science academies of Britain, China, Germany, Japan and other nations in a joint statement saying, "There is now strong evidence that significant global warming is occurring."

This year Mr. Karl of the climatic data center said research now supports "a substantial human impact on global temperature increases."

And this month the Climate Change Science Program, the Bush administration's coordinating agency for global-warming research, declared it had found "clear evidence of human influences on the climate system."

Case closed. Earth's surface, atmosphere and seas are warming; ocean currents are slowing; ice shelves are melting faster than projected; spring is coming ever sooner; rainfall patterns are changing; North American migratory birds are ranging father north; the ability of the earth to self-regulate to resist warming appears to be waning. While natural variation may play roles in climatic trends, overwhelming evidence points to the accumulation of greenhouse gases, mainly from the burning of fossil fuels, as the key.

Many greenhouse uncertainties remain, including whether rising temperatures would necessarily be bad. A warming world might moderate global energy demand: the rise in temperature so far has mostly expressed itself as milder winters, not hotter summers. Warming might open vast areas of Alaska, Canada and Russia to development. My hometown of Buffalo might become a vacation paradise. (Buffalo lakefront real estate is cheap. Here's a tip: buy some now.)

But it seems likely any global-warming benefits will be offset by unwanted trends. The National Academy of Sciences estimates that in the coming century, sea levels may rise by as much as three feet. Tropical storms may continue to increase in number and fury. Diseases now confined to equatorial regions may spread father north and south.

The greatest worry is that climate change will harm the agricultural system on which civilization is based. Suppose climate change shifted precipitation away from breadbasket regions, sending rain clouds instead to the world's deserts. Over generations, society would adjust — but years of global food shortages might occur during the adjustment, likely causing chaos in poor countries and armies of desperate refugees at the borders of wealthy nations.

Scientific substantiation of a warming world is not necessarily reason for gloom. Greenhouse gases are an air pollution problem, and all air pollution problems of the past have cost significantly less to fix than critics projected, and the solutions have worked faster than expected.

During the 1960's, smog in America was increasing at a worrisome rate; predictions were that smog controls would render cars exorbitantly expensive. Congress imposed smog regulations, and an outpouring of technical advances followed. Smog emissions in the United States have declined by almost half since 1970, and the technology that accomplishes this costs perhaps $100 per car.

Similarly, two decades ago a "new Silent Spring" was said to loom from acid rain. In 1991, Congress created a profit incentive to reduce acid rain: a system of tradable credits that rewards companies that make the fastest reductions. Since 1991 acid rain emissions have declined 36 percent, and the cost has been only 10 percent of what industry originally forecast.

Today no one can make money by reducing greenhouse gases, so emissions rise unchecked. But a system of tradable greenhouse permits, similar to those for acid rain, would create a profit incentive. Engineers and entrepreneurs would turn to the problem. Someone might even invent something cheap that would spread to the poorer countries, preventing reductions here from being swamped elsewhere. Unlikely? Right now reformulated gasoline and the low-cost catalytic converter, invented here to contain smog, are becoming common in developing nations.

President Bush was right to withdraw the United States from the cumbersome Kyoto greenhouse treaty, which even most signatories are ignoring. But Mr. Bush should speak to history by proposing a binding greenhouse-credit trading system within the United States. Waiting for science no longer justifies delay, as results are now in.

Gregg Easterbrook, a fellow at the Brookings Institution, is the author of "The Progress Paradox: How Life Gets Better While People Feel Worse."

TAB 21

AN INCONVENIENT TRUTH

THE PLANETARY EMERGENCY OF GLOBAL WARMING AND WHAT WE CAN DO ABOUT IT

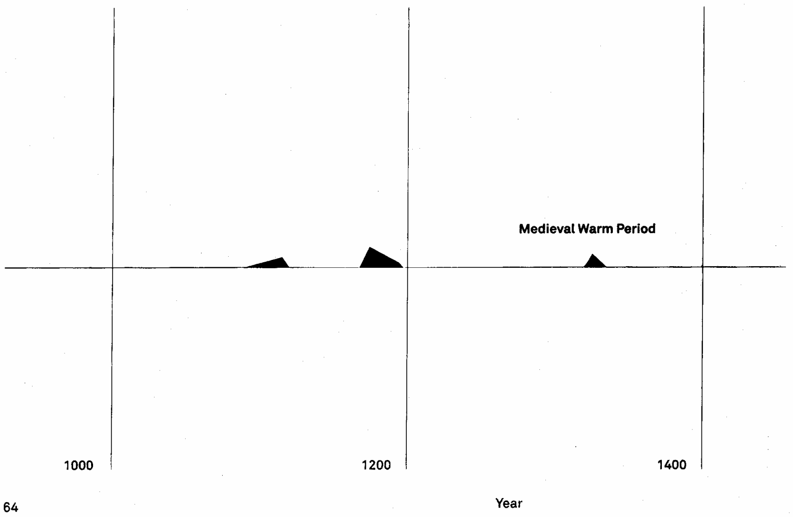
AL GORE

The correlation between temperature and CO_2 concentrations over the last 1,000 years—as measured in the ice core record by Thompson's team—is striking.

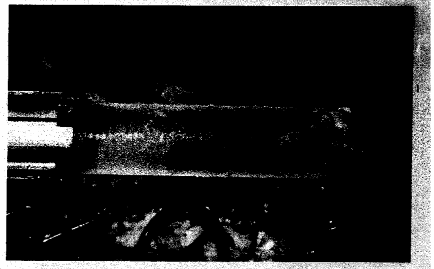
Nonetheless, the so-called global-warming skeptics often say that global warming is really an illusion reflecting nature's cyclical fluctuations. To support their view, they frequently refer to the Medieval Warm Period.

But as Dr. Thompson's thermometer shows, the vaunted Medieval Warm Period (the third little red blip from the left, below) was tiny compared to the enormous increases in temperature of the last half-century (the red peaks at the far right of the chart).

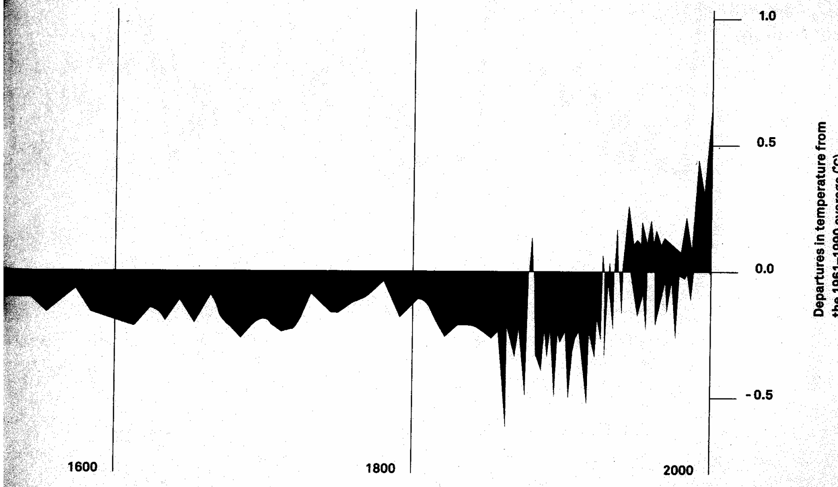
1000 YEARS OF NORTHERN HEMISPHERE TEMPERATURE (°C)



Those global-warming skeptics—a group diminishing almost as rapidly as the mountain glaciers—launched a fierce attack against another measurement of the 1,000-year correlation between CO_2 and temperature known as "the hockey stick," a graphic image representing the research of climate scientist Michael Mann and his colleagues. But in fact, scientists have confirmed the same basic conclusions in multiple ways—with Thompson's ice core record as one of the most definitive.



GLACIOLOGIST REMOVING AN ICE CORE, ANTARCTICA, 1993



SOURCE: IPCC

[Whereupon, at 4:12 p.m., the subcommittee was adjourned.]

RESPONSE FOR THE RECORD OF DR. GERALD R. NORTH, DEPARTMENT OF ATMOSPHERIC SCIENCES, TEXAS A&M UNIVERSITY

The Honorable Ed Whitfield

1. As you chaired the National Research Council panel that recently issued the report on millennial temperature reconstructions:

   a. Where in the report did the panel describe "plausible" as suggesting roughly a 2/3rds probability of being correct?

In the report we shunned the use of numerical probability assessments in favor of descriptive statements (e.g., "high confidence") and statements that describe our relative confidence in different conclusions (e.g. "less confidence"). I may have mistakenly mentioned the "two to one odds" figure in the oral press release of the report, and it may also have appeared in some press accounts, but it does not appear in the report, and I avoided using it in my sworn testimony.

   b. In the report, did the panel attach probability estimates to the term "plausible"?

No. The committee avoided numerical probability estimates because many of the uncertainties associated with reconstructing surface temperatures are not purely statistical in nature, but rather arise from physical factors associated with each proxy that are simply unquantifiable at this time. In our view it is not possible to quantify all of the inherent uncertainties associated with reconstructing surface temperatures from proxy data, which in turn precludes assigning numerical probabilities to statements regarding the unique nature of recent warmth.

   c. Why did the panel choose to use the term "plausible," as opposed for example to terms such as "likely," to describe confidence in millennial temperature reconstructions?

In the IPCC reports, the term "likely" is used to indicate an estimated probability of between 66% and 90%, i.e. greater than two-thirds odds but less than nine-in-ten chances. We avoided numerical estimates such as these because we did not want to imply that we had performed a rigorous probability assessment. Instead, we tried to express our collective confidence in different conclusions using descriptive language.

2. When considering the panel's findings that it is "plausible" that recent decades were the warmest in a millennium, is that correct to interpret that to mean the panel's consensus view was that plausible means roughly a 2/3rds probability of being correct, as was suggested in news reports following the press conference releasing the report?

Our working definition of "plausible" was that the assertion is reasonable, or in other words there is not a convincing argument to refute the assertion. We used this term to describe our assessment of the statement that "the last few decades of the 20th century were warmer than any comparable period over the last millennium" because none of the available evidence to date contradicts this assertion. In our view it is not currently possible to perform a quantitative evaluation of recent warmth relative to the past 1,000 years that includes all of the inherent uncertainties associated with reconstructing surface temperatures from proxy data. This precludes stronger statements of confidence, but it does not mean that the assertion is false. In fact, all of the large-scale surface *temperature reconstructions that we examined support the assertion that global-mean temperatures during the last few decades of the 20th century were unprecedented over at least the past 1,000 years, and a larger fraction of geographically diverse proxy records experienced exceptional warmth during the late 20th century than during any other extended period from 900 A.D. onward.*

3. Did the panel perform its own, in-depth technical analysis of the methods and procedures-- such as checking the underlying data sets or attempting to replicate the findings – used in the various temperature reconstruction articles and presentations it considered in formulating its report?

Our committee relied on the published, refereed scientific literature to reach its conclusions. We did not attempt to replicate the work of any previous author, with the lone exception of a simple computer program (reproduced in Appendix B of our report) that was used to illustrate an interesting artifact of the principal components methodology first noted by McIntyre and McKitrick. When evaluating the results of different studies, we placed higher confidence in those results that were reproduced in several different studies--for instance a number of independent lines of evidence indicate that the late 20th century warmth was unprecedented in at least the last 400 years, giving us high confidence in this conclusion. Less confidence can be placed in conclusions regarding large-scale surface temperatures prior to about 1600 A.D. because there are simply fewer independent lines of evidence to consider, although the evidence that does exist indicates that the late 20th century warmth is unprecedented for at least the last 1,000 years.

4. The NRC panel made specific reference to ice borehole studies in Greenland by Dahl-Jensen, which suggest warmer temperatures in that region during the Medieval Warm Period than today. Please explain the value of regional temperature measurements such as this for understanding the potential effects of recent warming trends?

There are two main reasons for using large-scale averages rather than individual regional measurements to evaluate global environmental changes: 1) Random measurement errors and climate fluctuations tend to cancel out when spatial averages are performed, allowing researchers to obtain a more reliable estimate than is possible for a local or a regional average; 2) The greenhouse effect operates at the global scale, hence large-scale averages are the best way to evaluate the response of the climate to increasing greenhouse gas concentrations. Current climate models also are better at computing large-scale averages than regional-scale values.

Of course in order to detect large-scale climate anomalies, either in the modern temperature record or in proxy-based temperature reconstructions, it helps to have a large network of high quality measurements for geographically-diverse regions. The main reason that we have high confidence in the temperature increase over the past 100 years and in the statement that temperatures are warmer now than at any other time over the last 400 years is because we have a sufficiently large number of well-characterized local measurements to calculate a reliable large-scale average. Several proxies (including historical and archeological evidence as well as quantitative temperature estimates from ice cores and boreholes) indicate that the area around Greenland was warmer between about 1000 and 1200 A.D. than it is today. There is also evidence for warm temperatures during medieval times from other regions of the world. However, studies suggest that these warm anomalies appear to have occurred at different times at *different places rather than being globally synchronous, and also appear to have been offset by cold anomalies in other regions. The few large-scale surface temperature reconstructions that extend back far enough to rigorously compare large-scale medieval temperatures to modern warmth suggest that the medieval period was, at most, comparable in warmth to the first half of the 20th century. However, as noted above in response to question (4), it is difficult to quantify the full uncertainty associated with estimates of surface temperature prior to about 1600 A.D.*

The Honorable Bart Stupak

1. In the study performed by a special committee of the National Academy of Sciences (NAS) on surface temperature reconstructions over the past 2,000 years, it was stated that, for the time prior to 1600 A.D., scientists are less certain about the actual average northern hemispheric surface temperatures. The Medieval Warm Period (MWP) occurred prior to 1600. How certain are climatologists that there was a globally or even hemispherically MWP that was warmer than the past several decades?

Indeed, the paucity of proxy data for periods prior to about 1600 A.D., especially in the tropics and the Southern Hemisphere, limits our confidence in statements regarding the global mean temperature of the past few decades compared to medieval times. Several proxies indicate that the area around Greenland was warmer between about 1000 and 1200 A.D. than it is today. There is also evidence for warm temperatures during medieval times from other regions of the world. However, studies suggest that these warm anomalies appear to have occurred at different times at different places rather than being hemispherically or globally synchronous, and also appear to have been offset by cold anomalies in other regions. Although it is difficult to quantify the full uncertainty associated with estimates of surface temperature prior to about 1600 A.D., all of the large-scale surface temperature reconstructions that we examined support the assertion that global-mean temperatures during the last few decades of the 20th century were unprecedented over at least the past 1,000 years, and a larger fraction of geographically diverse proxy records experienced exceptional warmth during the late 20th century than during any other extended period from 900 A.D. onward. Hence we find it plausible (or in other words, no evidence exists to refute the claim) that "the last few decades of the 20th century were warmer than any comparable period over the last millennium." This statement can be more strongly applied to the Northern Hemisphere than to the globe because there is very little proxy data from the Southern Hemisphere before about 1600 A.D.

2. The 1990 Intergovernmental Panel on Climate Change (IPCC) Report contains a "schematic diagram" that shows temperature changes for 900 A.D. through 1975, but does not give specific temperatures. The text of the report notes, "it is still not clear whether all the fluctuations indicated were truly global." Am I correct in my understanding that this schematic diagram is not a graph of specific data points consisting of global temperature for particular years or time periods? Am I also correct that the scientific consensus at the time was that there was significant uncertainty about whether the diagram accurately portrayed the global temperature profile over the last 1,000 years?

Yes, the schematic diagram that appeared in the 1990 IPCC Report was simply a qualitative depiction of how scientists thought that large-scale temperatures may have evolved from 900 A.D. to about 1975. There was very little proxy data available at that time, and the data that did exist tended to be concentrated in just a few geographical *regions, such as Greenland. The lack of a temperature scale and supporting documentation strongly suggests that the diagram was not based on a quantitative analysis, and also implies that there was considerable uncertainty about the magnitude and timing of the indicated fluctuations. As stated in our report, there is still considerable uncertainty about the exact timing and magnitude of past temperature fluctuations, especially prior to about 1600 A.D., but our knowledge has advanced considerably since 1990. Figure S-1 from our report illustrates the current state of the science in large-scale surface temperature reconstructions for the last 1,000 years.*

3. What level of certainty is there that the temperature ranges for the period of 900 through 1975 A.D. schematically displayed in the 1990 IPCC report are accurate? Prior to Dr. Mann's work, had anyone attempted to attach a level of certainty to the data relating to surface temperature reconstruction?

There were no uncertainty assessments attached to the 1990 IPCC diagram. As discussed in response to question (2) above, this diagram was simply a qualitative depiction of how scientists thought that large-scale temperatures may have evolved from 900 A.D. to about 1975. The papers by Dr. Mann and his colleagues in 1998 and 1999 were, to my knowledge, the first attempts to assign statistical error bars to a large-scale surface temperature reconstruction. As noted in our report, these error bars provide an indication of how well the reconstructed temperatures match observations during the "calibration period," but they do not represent all of uncertainties inherent in reconstructing surface temperature from proxy data. The actual uncertainties in the reconstruction would be somewhat larger, and difficult to quantify.

4. Mr. McIntyre has testified that the NAS report stated that the bristlecone pine proxy used by Dr. Mann in his original work should not have been used. Was that the conclusion of the panel? Please describe the conclusion and provide citations.

Let me quote directly from page 50 of the prepublication version of our report:

> *The possibility that increasing tree ring widths in modern times might be driven by increasing atmospheric carbon dioxide (CO_2) concentrations, rather than increasing temperatures, was first proposed by LaMarche et al. (1984) for bristlecone pines (Pinus longaeva) in the White Mountains of California. In old age, these trees can assume a "stripbark" form, characterized by a band of trunk that remains alive and continues to grow after the rest of the stem has died. Such trees are sensitive to higher atmospheric CO_2 concentrations (Graybill and Idso 1993), possibly because of greater water-use efficiency (Knapp et al. 2001, Bunn et al. 2003) or different carbon partitioning among tree parts (Tang et al. 1999). Support for a direct CO_2 influence on tree ring records extracted from "full-bark" trees is less conclusive. Increasing mean ring width was reported for Pinus cembra from the central Alps growing well below treeline (Nicolussi et al. 1995). Free-Air CO_2 Enrichment (FACE) data for conifer plantations in the Duke Forest (Hamilton et al. 2002) and at the alpine treeline (Hättenschwiler et al. 2002) also showed increased tree growth after exposure to atmospheric CO_2 concentrations about 50 percent greater than present. On the other hand, no convincing evidence for such effect was found in conifer tree ring records from the Sierra Nevada in California (Graumlich 1991) or the Rocky Mountains in Colorado (Kienast and Luxmoore 1988). Further evidence comes from a recent review of data for mature trees in four climatic zones, which concluded*

> *that pine growth at treeline is limited by factors other than carbon (Körner 2003). While 'strip-bark' samples should be avoided for temperature reconstructions, attention should also be paid to the confounding effects of anthropogenic nitrogen deposition (Vitousek et al. 1997), since the nutrient conditions of the soil determine wood growth response to increased atmospheric* CO_2 *(Kostiainen et al. 2004). However, in forest areas below treeline where modern nitrogen input could be expected to influence dendroclimatic records, such as Scotland (Hughes et al. 1984) and Maine (Conkey 1986), the relationship between temperature and tree ring parameters was stable over time.*

In summary, it appears that there is a carbon dioxide fertilization effect in some trees, but not in all the places where the samples used in the Mann et al studies were taken. Also note that this section of the report discusses the calibration of tree-ring records since atmospheric carbon dioxide levels started to increase around 150 years ago. Hence, in context, what the clause "strip-bark samples should be avoided for temperature reconstructions" was intended to convey is that strip-bark samples from the mid-19th century to the present are very difficult to calibrate against instrumental records of temperature, and the easiest solution is therefore not to use them. However, strip-bark data are considered suspect only after the modern increase in atmospheric carbon dioxide concentrations. This is why other studies that rely on strip-bark pine records only use them to infer past temperatures prior to 1850 (e.g., Biondi et al. 1999). This reference, and all of those cited in the above quote, can be found in the reference section of our report.

5. The recent work by Wahl & Amman redid Dr. Mann's original work, but recentered it as Mr. Mcintyre suggested. Wahl and Amman's work, however, resulted in the same "hockey stick" distribution. Please explain why this work was not fully considered and evaluated in the NAS study.

We did consider the Wahl and Ammann paper that was accepted for publication in the journal Climatic Change on February 28th of this year, in which they found that decentering has only a relatively minor influence on the shape of the final reconstruction. This paper was one of many that influenced our evaluation of the Mann et al. (1998, 1999) papers and the robustness of surface temperature reconstructions in general. The effects of decentering are described explicitly in Chapter 9 of our report, and our conclusions regarding how decentering influences surface temperature reconstructions can be found in the following excerpt from page 106 of the prepublication version of the report:

> *As part of their statistical methods, Mann et al. used a type of principal component analysis that tends to bias the shape of the reconstructions. A description of this effect is given in Chapter 9. In practice, this method, though not recommended, does not appear to unduly influence reconstructions of hemispheric mean temperature; reconstructions performed without using principal component analysis are qualitatively similar to the original curves presented by Mann et al. (Crowley and Lowry 2000, Huybers 2005, D'Arrigo et al. 2006, Hegerl et al. 2006, Wahl and Ammann in press).*

Drs. Wahl and Ammann (along with Dr. Ritson) also authored a paper that appeared in Science magazine on April 28th of this year alongside a response written by Drs. von Storch and Zorita. These papers were under embargo during our deliberations, and thus *we were not able to consider them during our deliberations, although we did note (on page 105) that "the…debate in the scientific literature continues even as this report goes to press (von Storch et al. 2006, Wahl et al. 2006)." These papers address a separate statistical issue than the one discussed above, in particular the issue of detrending the data prior to performing principal components analysis. My personal impression of these two papers is that the quote cited above still applies, that is, none of the statistical criticisms that have been raised by various authors unduly influence the shape of the final reconstruction. This is attested to by the fact that reconstructions performed without using principal components yield similar results.*

6. In the hearing, Dr. Wegman challenged "anybody" to tell him the difference between 72 and 74 degrees Fahrenheit. Please describe the climatic and other changes that can result from a global increase in temperature of 2 degrees Fahrenheit.

As context, let me first point out that the difference in global-mean temperature between today and the height of the last Ice Age, when New York and Seattle were covered with over a kilometer of ice, is estimated to be only about 10 degrees Fahrenheit. Hence, a change in global-mean temperature of two degrees would represent a considerable perturbation to the global climate system. Small changes in local temperatures can also be associated with large impacts. For example, for every degree Fahrenheit increase in mean annual temperature near Greenland, the rate of sea level rise is projected to increase by 10%. Snowpacks on mountains in the western U.S., which millions of people depend on for drinking water and other uses, is likewise extremely sensitive to small temperature changes. Natural ecosystems are also vulnerable to changes in temperature--in the Midwest, a one degree change in annual mean temperature might translate into several hundred miles in the ecological distribution of certain plants and grasses, and a warming of just a few degrees could have devastating impacts on New England's maple syrup industry and California's vineyards. Many parts of the climate system are already feeling the impacts of the one degree rise in global-mean temperature observed during the 20th century. As we noted on page 27 of the prepublication version of our report: "glaciers are retreating, permafrost is melting, snowcover is decreasing, Arctic sea ice is thinning, rivers and lakes are melting earlier and freezing later, bird migration and nesting dates are changing, flowers are blooming earlier, and the ranges of many insect and plant species are spreading to higher latitudes and higher elevations (e.g., ACIA 2001, Parmesan and Yohe 2003, Root et al. 2003, Bertaux et al. 2004, Bradshaw and Holzapfel 2006)."

7. Dr. Von Storch testified that the effect of the "decentering" error in the Mann study, which was the basis of the McIntyre and Wegman criticisms, was "very minor." The NAS study did not refer to "decentering." How significant was the analysis of "decentering" to the NAS conclusions?

I believe Dr. von Storch was referring to the same phenomenon that I described in my response to your question #5. Our committee did consider the effects of decentering, along with other criticisms of the Mann et al methodology, and found that it "does not appear to unduly influence reconstructions of hemispheric mean temperature."

8. At the hearing you were asked if you disputed the conclusions or the methodology of Dr. Wegman's report, and you stated that you did not. Were you referring solely to Dr. Wegman's criticism of the statistical approach of Dr. Mann, or were you also referring to

Dr. Wegman's social network analysis and conclusions?

Dr. Wegman's criticisms of the statistical methodology in the papers by Mann et al were consistent with our findings. Our committee did not consider any social network analyses and we did not have access to Dr. Wegman's report during our deliberations so we did not have an opportunity to discuss his conclusions. Personally, I was not impressed by the social network analysis in the Wegman report, nor did I agree with most of the report's conclusions on this subject. As I stated in my testimony, one might erroneously conclude, based on a social network analysis analogous to the one performed on Dr. Mann, that a very active and charismatic scientist is somehow guilty of conspiring or being inside a closed community or 'mutual admiration society'. I would expect that a social network analysis of Enrico Fermi or any of the other scientists involved with the development of modern physics would yield a similar pattern of connections, yet there is no reason to believe that theoretical physics has suffered from being a tight-knit community. Moreover, as far as I can tell the only data that went into Dr. Wegman's analysis was a list of individuals that Dr. Mann has co-authored papers with. It is difficult to see how this data has any bearing on the peer-review process, the need to include statisticians on every team that engages in climate research (which in my view is a particularly unrealistic and unnecessary recommendation), or any of the other findings and recommendations in Dr. Wegman's report. I was also somewhat taken aback by the tone of the Wegman Report, which seems overly accusatory towards Dr. Mann and his colleagues, rather than being a neutral, impartial assessment of the techniques used in his research. In my opinion, while the techniques used in the original Mann et al papers may have been slightly flawed, the work was the first of its kind and deserves considerable credit for moving the field of paleoclimate research forward. It is also important to note that the main conclusions of the Mann et al studies have been supported by subsequent research. Finally, while our committee would agree with Dr. Wegman that access to research data could and should be improved, as discussed on page 23 of the prepublication version of our report, we also acknowledge the complicated nature of such mandates, especially in areas such as computer code where intellectual property rights need to be considered.

The Honorable Marsha Blackburn

1. Dr. Mann used many temperature measurements from different sources to produce his graph. In your opinion, how much emphasis or reliance did he place on surface records and satellite measurements?

To perform their surface temperature reconstruction, Dr. Mann and his colleagues made use of proxy data derived primarily from tree rings, ice cores, and documentary sources. Tree rings and ice cores, like other natural proxies, do not record temperature directly, but are correlated with local temperatures through physical and physiological mechanisms. They also made use of surface thermometer records from the last 150 years, which were used to calibrate the reconstruction (i.e. translate the proxy data into a record of temperature) and to validate their results (i.e. test whether the reconstructed temperatures match a portion of the observations reserved for this purpose). All paleoclimate reconstructions use a similar methodology, with the exception of reconstructions based on borehole temperature measurements and glacier length records, which are translated directly into temperature time series using models based on the laws of physics. Satellite measurements are not used in any paleoclimate reconstructions because they only go back about 30 years, which is much too short for this application.

a. How much weight do you think should be given to these measurements?

Dr. Mann and his colleagues used all of the quality-controlled proxy data that they had at their disposal at the time. As we indicated in our report, the available proxy data are plentiful and geographically diverse for the last 400 years, but decrease in number and become subject to increasing uncertainties going back further into the past. Hence, we have high confidence in the surface temperature reconstructions based on these data for the last 400 years, but less confidence in reconstructions for the period from 900 to 1600 A.D. This increasing uncertainty moving back in time is reflected, in part, by the increasing size of the error bars prior to 1600 A.D. in the original 'hockey stick' curve, although these error bars do not account for all of the uncertainties inherent in the reconstruction.

2. The surface record and the satellite measurements indicate that if maybe natural warming and not human-induced warming. Yet, in your testimony, you say that increasing concentrations of greenhouse gases caused the warming. How do we reconcile your statement with the historical record?

The temperature record alone cannot tell us the difference between 'natural' and 'human-induced' temperature changes. One has to try to explain the observed warming using the laws of physics. During the last 100 years, the global-mean temperature first increased strongly, then remained constant or decreased slightly, then increased strongly again. Simple radiative transfer calculations and sophistical climate models both show that the total amount of warming observed over the 20th century is consistent with the observed increases in greenhouse gas concentrations in the atmosphere, which are undeniably the result of human activities. Changes in solar output can also influence the climate system. However, satellite measurements show that the sun has not increased in luminosity over the last 30 years, and estimates based on terrestrial measurements show only a modest increase in solar output during the first half of the 20th century. A third factor that may have had a significant influence on global-mean climate during the 20th century is atmospheric aerosols. These are the tiny particles that, like greenhouse gases, are emitted from volcanoes and other natural sources as well as from anthropogenic sources, but have been increasing in concentration in the atmosphere over the past century mainly due to the burning of fossil fuels and other human activities. Aerosols influence climate in a variety of ways, some of which are well known and others of which are active areas of research, but in general they have a cooling influence on climate. There is some evidence that suggests that aerosols may be primarily responsible for the slight decrease in global-mean temperature observed during the middle of the 20th century, and they might also be offsetting some of the warming due to greenhouse gases.

a. Also, the historical record indicates that in the past 100 years, the Earth's global temperature warmed and cooled significantly while the concentrations of carbon dioxide increased. Would this not also indicate that the level of carbon dioxide in the atmosphere has had little effect on the warming of the atmosphere?

No. The Earth's temperature over the past 100 years was influenced by increases in carbon dioxide and other greenhouse gases, which have a warming effect, by changes in aerosols, which generally cool the climate, and by other climate forcings. Thus, the observed temperature variations reflect the net effect of these different forcings.

We have a very good understanding of the direct impact of carbon dioxide and other *greenhouse gases on global temperature. Straightforward radiative transfer calculations tell us that carbon dioxide has a significant influence on global climate. Sophisticated climate models also show that the observed temperature changes during the 20th century cannot be reproduced unless greenhouse gases increases are included. There are also other lines of evidence indicating that carbon dioxide and other greenhouse gases have a strong influence on global climate. For example, models cannot reproduce the global-mean cooling that occurred during the last Ice Age without incorporating the reduced levels of greenhouse gases that prevailed during that time.*

3. You also state in your testimony that even if it was as warm or warmer 1000 years ago than today that it would not effect today's consensus on global warming. That seems to not be logical because if the Earth goes through natural cycles of warming and cooling, then would not the warming and cooling cycles over the past 60 and 500 years be a similar indication of phenomenon?

It is true that the Earth has experienced natural cycles of warming and cooling over its history, however natural climate forcings (solar activity, changes in natural aerosols) observed over the last century are not large enough to produce the observed warming, especially for the last 30 years. There is a large and compelling body of evidence indicating that human-induced greenhouse gas increases are responsible for at least part of the total warming over the 20th century, and most of the warming over the last 30 years. Over the last 100 years and especially the last 30 years, we have very good data for both temperature and all of the major climate forcings (greenhouse gases, solar activity, and aerosols). Analyses of these data indicate that human-induced greenhouse gases appear to be responsible for much of the warming over the last 30 years and at least part of the total warming over the last century. Reconstructions of surface temperature over the past 1,000 years are one piece of the scientific evidence, but these reconstructions are sufficiently uncertain, especially prior to 1600 A.D., that they are not usually considered to be among the primary evidence for human-induced global warming. In addition, temperature data alone do not tell us anything about cause and effect.

In contrast, we know that greenhouse gases did not vary much during the 1,000 years prior to the industrial revolution, but we have very little data about how solar output and aerosols varied over this period. Moreover, what little evidence we do have shows only small variations in climate forcing due to natural causes. Hence, if we were to find out that the global-mean temperature 1,000 years ago was warmer than today, this would mean that the Earth's climate is even more sensitive to small forcings than we thought, which would mean that projections of future warming may be overly conservative.

RESPONSE FOR THE RECORD OF DR. THOMAS J. CROWLEY, NICHOLAS PROFESSOR OF EARTH SCIENCE SYSTEM, DUKE UNIVERSITY

Response by T. Crowley to Followup Questions on July 19 Testimony

The Honorable Marsha Blackburn:

1. **Do you agree or disagree with the surface record and satellite data which indicate that global temperatures did not start to rise significantly until the 1998 El Nino?**

   I emphatically disagree with this statement. The surface temperature record clearly shows very substantial warming before 1998. Recent work furthermore indicates that the satellite observations are close to being reconciled with these surface observations – and that the prior differences between the two was to a coding error in the analysis of the satellite data, not a problem with the surface data. Congresswoman Blackburn, anyone who tries to tell you that the warming did not occur until 1998 is seriously misleading you.

2. **Do you believe the available data shows a global Little Ice Age and/or Medieval Warm Period?**

   It is not easy to give an unequivocal answer to this, because southern hemisphere data are considerably more spotty than northern hemisphere data. The available data suggest that the southern hemisphere did indeed have a cold period about the same time as the northern hemisphere. There are some indications of warmth in the southern hemisphere prior to that time, but it is not clear whether the timing of that warmth was the same as in the northern hemisphere. Although some northern hemisphere places during the Middle Ages were locally warmer than they are today, in the best-dated records the timing of Medieval warmth varied in different places. This is why composite reconstructions almost always show that the mean warmth for the Middle Ages is usually comparable to the mid-20th century but not the late 20th century.

3. **Do you agree or disagree with the statement that warming from 1900 to 1940 was caused by increase of solar activity or the warming of the Sun?**

   I disagree with the statement because it is too categorical. There are some indications that changes in solar behavior may have contributed to the mid-20th century warming. But when this "solar connection"is tested by going farther back in time the conclusions become much more equivocal. The most methodical analysis (see Attachment #1 – Hegerl et al. 2003) provides at best weak support for the long-term role of solar variability. Furthermore, the magnitude of past solar variations is very uncertain – even optimistic estimates indicate it is only a fraction of present greenhouse gas forcing. The present thinking is that the mid-20th century warming was due to a combination of weakened volcanic cooling, greenhouse warming, "natural variability", and perhaps a modest contribution from solar output changes.

4. **What is your opinion on the effect of the 1998 El Nino on the recent rise in temperatures?**

The 1998 El Nino certainly contributed to the (at that time) record global temperatures but I don't think anyone seriously thinks it has a long term effect on global temperature – the heat just dissipates too quickly in the atmosphere to have such an effect. I might add that it has taken less than a decade for the continually rising temperatures to approach or equal the 1998 temperatures. This increase is very disconcerting in terms of how fast the planet is warming.

End of reply to the Honorable Marsha Blackburn

The Honorable Bart Stupak:

1. **In the hearing, Dr. Wegman testified that your 2000 published work, which used a simple averaging proxy methodology, obtained the same "hockey stick" configuration as Dr. Mann's original work did. Dr. Wegman blames this conclusion on "proxies appropriately selected" apparently because of use of the bristlecone pine proxy.**

Please explain if and why your work also used the bristlecone pine proxy and respond to Dr. Wegman's criticisms of its use.

I do not recall Dr. Wegman making this testimony but will accept your claim. Actually the purpose of the Crowley-Lowery 2000 study (ms. submitted as hard copy during testimony) was not to reproduce Mann et al. with a different methodology but just to determine what would happen if we took a broad swath of data and just summed them up. I was as surprised as anyone that the result was as close to Mann et al. as it was – bristlecone pine or no bristlecone pine (the one we used was different than Mann et al's). The principal significance of our finding was that the Mann et al. result appeared to be robust because it could be reproduced with a different methodology – a standard approach in science.

The bristlecone pine business is a red herring. If the bristlecone pine record is removed from the composite of a dozen or so records, it will show slightly greater warming in the Middle Ages. But one record can only make so much a difference when it is averaged among a dozen, especially since the general shape of the bristlecone pine record is comparable to the other records.

A more important objection to the bristlecone pine argument is that it should not be included. Why not? In statistics anyone can use something as a predictor or something else. The question is how could a predictor is it? Some have claimed that it should not be included because it is more affected by some other process (for example, precipitation). But a principal assumption of regression based prediction approaches is that the variables used for making predictions are linearly correlated with the variable they are predicting (in this theoretical case, precipitation with temperature). The degree of skill in the predictor can be tested by its correlation with temperature. If it has a poor correlation, it has little skill. This is an approach we have adopted in later papers, but the purpose of the original study was to just take as simple as an approach as possible.

2. **There were numerous references in the hearing to a schematic drawing of what scientists supposed surface temperatures might have been from 1000 A.D. to 1975 in the 1990 report of the Intergovernmental Panel for Climate Change (IPCC) . You stated in your testimony that Dr. Mann's study represented the first attempt to estimate the uncertainties for surface temperature reconstructions prior to the instrumental period. Can you describe what level of uncertainty would have been placed on the 1990 schematic drawing, and what level of uncertainty Dr. Mann established for the period prior to 1600 A.D.**

This is a good question! But before answering it I have to explain what happened during the formulation of the 1990 figure. At that time we really did not have any hemispheric estimates of temperature. What IPCC did in 1990 was informally poll various experts for a "guesstimate" of what the temperatures were like (I vaguely recall being asked by someone around that time, but I do not know if it was related to the IPCC figure). Many scientists had heard of the "Medieval Warm Period" and stories of warmth greater than the present. Despite warnings from a prominent Chinese scientist, and a prominent English scientist, that the timing of warmth in the Middle Ages was not the same in all places, many people (including some still now) assumed that the Medieval Warmth was globally synchronous. Thus the 1990 figure – entirely schematic and left standing until it could be replaced by an alternate quantitative estimate, with meaningful uncertainty estimates (i.e., the Mann et al. paper, and others that have followed).

Now for the uncertainty estimates. One would have to be very wary to apply uncertainty estimates to a qualitative figure, but if one were to do so, then maybe a "ball park" 0.5 °C (about 1.0 °F) uncertainty might be applied. If so, then one would have to conclude that is not possible to make a robust statement that the Middle Ages were warmer than the present, because the original estimate likely did not exceed 0.5°C above "present" (which at the time of writing of the report was about seventeen years ago). [Note that I cannot find my copy of the original figure, so I would have to doublecheck the 0.5*C peak, but because the uncertainty estimate is also uncertain, I still stand by my conclusion about "inability to make a robust statement"

With respect to the uncertainty estimates prior to 1600 in the Mann et al. paper, the most that can be stated is the estimates are substantially larger than for the later period just because there are much fewer records. The uncertainties for estimates of annual temperature are about 0.5°C in Mann et al. (1999). However, the degree of uncertainty would decrease as records are smoothed. For example, forty year smoothing of the Mann et al. record yields uncertainties of about 0.4°C. Smoothing comparable to the very smoothed 1990 IPCC figure has not, to my knowledge, been computed, but a reasonable guess would be that it would be in the range of 0.2-0.3°C.

3. **Please describe the peer review process for your most recent publications.**

The peer review process has been pretty similar for my entire scientific career. The paper goes out to 2-3 reviewers, who almost always provide anonymous peer reviews (i.e., they can say anything they want about it!). If the reviewers like the paper but have questions, the editor will request that a revised manuscript be prepared that takes into account reviewer concerns, and that a separate accounting be made to the editor and reviewer about how specifically we addressed those concerned. Depending on the seriousness of the concerns, the editors will then either review the response themselves, or send it back to the reviewers (if the concerns are minor he or she would probably not sent it back to the reviewers). In some cases the reviewer may still be dissatisfied, in which case the authors would have to reiterate, but in many cases the reviewers will be satisfied. In some cases an editor might decide that if a reviewer is still dissatisfied, then the editor may choose to reject the paper. Only after the editor is fully satisfied that reviewers and reconciled will the editor accept the paper. In some cases the editor may accept a paper even if there are disagreements with reviewers, because a subject matter may be controversial and an editor may feel that all sides of an issue deserve a public airing. In that case an editor may still accept a paper that has been opposed by a reviewer.

End of reply to the Honorable Bart Stupak

The Honorable Henry A. Waxman:

1. **You were added to the witness list for this hearing on short notice, and therefore had very little time to prepare your testimony. In reviewing your previously submitted testimony, is there anything you would like to clarify or supplement for the record.**

Thank you for the opportunity to respond to this. I am satisfied with most of the document but there are a few typos and grammatical mishaps I would like to correct. I am also chagrined by the choice of words I sometimes used to describe some of Dr. Wegman's report, and would like to change those. I will therefore send you a slightly revised version of the original document that makes such changes. If it is not possible to replace the original with the revision, then my statement herein is all I would like to add as a supplement.

End of Reply to the Honorable Henry A. Waxman

GEOPHYSICAL RESEARCH LETTERS, VOL. 30, NO. 5, 1242, doi:10.1029/2002GL016635, 2003

Detection of volcanic, solar and greenhouse gas signals in paleo-reconstructions of Northern Hemispheric temperature

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Received 19 November 2002; revised 10 January 2003; accepted 3 February 2003; published 12 March 2003.

[1] We apply a multiple regression method to estimate the response to anthropogenic and natural climate forcings simultaneously from a number of paleo-reconstructions of Northern Hemispheric average temperature. These long records (600 to 1000 years) provide a unique opportunity to distinguish between different external influences on climate. The response to volcanic forcing is reliably detected in all reconstructions, and the simulated temperature response to volcanic eruptions compares favorably with observations. The response to solar forcing is detected in Hemispheric mean data only over some periods in some records, and appears weak. Although most records can be used only to the middle of the 20th century, the temperature response to CO_2 can be detected by this time in most records. *INDEX TERMS:* 1699 Global Change: General or miscellaneous; 3344 Meteorology and Atmospheric Dynamics: Paleoclimatology; 1650 Global Change: Solar variability; 1620 Global Change: Climate dynamics (3309). **Citation:** Hegerl, G. C., T. J. Crowley, S. K. Baum, K.-Y. Kim, and W. T. Hyde, Detection of volcanic, solar and greenhouse gas signals in paleo-reconstructions of Northern Hemispheric temperature, *Geophys. Res. Lett., 30*(5), 1242, doi:10.1029/2002GL016635, 2003.

1. Introduction

[2] Results from recent detection and attribution studies based on 20th century instrumental data yield an increasing confidence in the detection of the anthropogenic greenhouse gas signal in 20th century temperature records [see, *Mitchell et al.*, 2001]. One of the key uncertainties of detection efforts based on instrumental data is that estimates of internal climate variability need to be derived from simulations with coupled climate models, and that the interdecadal to secular variability of climate models cannot be easily validated. Paleoclimatic data over several centuries provide a framework to consider both forced climate change and observed internal climate variability. The length of the records also enables better separation of the influence of different external forcings on climate, thus providing better estimates of the temperature response, particularly to natural climate forcing, such as volcanism and changes in solar radiation [cf. *Tett et al.*, 1998].

[3] Studies based on paleoclimatic data indicate a role of solar and volcanic forcing [e.g., *Mann et al.*, 1998; *Free and Robock*, 1999; *Crowley*, 2000; *Shindell et al.*, 2001]. Fingerprint detection methods (see below) are particularly suitable to reliably distinguish the influence of all relevant external influences from each other and from climate variability [see *Mitchell et al.*, 2001]. Here, we apply such a method to the detection and attribution of natural and anthropogenic forcings in a range of reconstructions of temperature over the past 6 to 10 centuries.

2. Detection Method

[4] We apply a multiple regression approach to detect and estimate fingerprints of anthropogenic forcing in paleo-reconstructions of Northern Hemispheric mean temperature. Such methods have been successfully applied to the detection of 20th century temperature change and its attribution to anthropogenic and natural forcings [e.g., *Hegerl et al.*, 1997; *Tett et al.*, 1998]. The observed record is linearly composed from a number of externally forced signals (here, the climate response to volcanism, solar forcing, and a combination of greenhouse gases and sulfate aerosols), and the residual is attributed to internal climate variability. The shape of the externally forced signal ("fingerprint", here its time evolution) is derived from simulations with a climate model (here an energy balance model, "EBM"). The amplitude of the signal is estimated from observations. If the amplitude of a signal is significantly different from zero, then the signal is "detected". If a signal amplitude of "1" is within the uncertainty range, the model signal is consistent with observations. Only if all signals with a substantial presence in the observed record are considered simultaneously can the observed climate variations be reliably attributed to external forcings and climate variability.

[5] The uncertainty range for the amplitude estimate is based on variations in fingerprint amplitudes that arise from internal climate variability and random errors in the reconstructions ("noise"). Noise samples are based on the residual proxy-timeseries after subtracting the best estimate of the externally forced signals. To provide a large sample, the residual timeseries has been shifted by increments of one year (appending the cut-off from the beginning at the end), yielding as many samples as years in the record, each of them of the same length as the record. The effective sample size is limited due to autocorrelation, its estimate (after subtracting three for fitting three time series to the data) typically exceeds 20 samples for entire records. The residual timeseries generally agree with a Gaussian distribution, and the resulting confidence limits are nearly identical if Monte-Carlo simulations of a fitted red-noise process are used. In a

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few cases, the residual exhibits unusually large trends over some period that cannot be explained by the forcings. Results then need to be treated with caution.

[6] The results of the detection analysis are sensitive to the accuracy of the simulated climate response to forcing, and errors in the forcing and paleo reconstruction. Errors in the *amplitude* of forcing, EBM response or paleo reconstruction do not influence the detection of signals, but will bias the scaling required to best fit observations. Errors in the *shape* of the fingerprint and proxy-timeseries will, however, lead to low estimates of the signals and possibly prevent their detection.

3. Observations

[7] We utilized a range of proxy-based reconstructions for Northern Hemispheric temperature evolution. Among them are records based on tree ring data only [*Briffa et al.*, 2001, hereinafter referred to as B01; *Esper et al.*, 2002, hereinafter referred to as E02]. Both records are based on tree ring data which have been standardized using an average age curve (such as "Age banding" in B01 and "Regional Curve Standardization" in E02). This preserves decadal and secular variability better than the earlier used individual age model [cf. *Briffa et al.*, 1998]. B01 is a record of Northern Hemisphere (NH) 20N–90N growing season (April–September) land temperature; the dimensionless record E02 has been also scaled to the NH growing season average over land.

[8] We also consider the *Mann et al.* [1999, hereafter referred to as M99] multi-proxy reconstruction of annual NH temperature (0–90N) and a modified version [*T. J. Crowley et al.*, in preparation, "CLH"] of the *Crowley and Lowery* [2000, hereinafter referred to as CL00] reconstruction (correlation with CLH 0.94). The latter is a weighted average of 9 long decadal or decadally averaged records over the Northern Hemisphere mid-to-high latitudes (30–90 N, the records sample both the warm and cold season, with a likely bias towards the summer half year). The weights are determined from the regression coefficients of individual records with the 30–90 N annual mean instrumental record during the period of overlap [*Jones et al.*, 1999]. The resulting paleo time series was scaled so that the regression fit with the instrumental data from 1880–1960 had a slope of 1.0 [decadal correlations of 0.81 (with trend) and 0.66 (detrended)]. For consistency, the scaling of E02 is based on the same period and also decadally filtered data.

[9] There is good qualitative agreement between the reconstructions - all show a more or less pronounced Medieval warm period, warm intervals for most of the 16th and 18th century, a cool 17th and early 19th century, and a temperature rise in the early 20th century. However, the amplitude and timing of fluctuations varies between records [cf. *Briffa and Osborn*, 2002].

4. Model and Forcing Time Series

[10] Ideally, fingerprints for external climate forcing are derived from large ensembles of general circulation model (GCM) simulations driven by variations in individual forcings over 600–1000 years. However, such simulations are not yet available. EBM simulations reproduce many aspects of the large-scale temperature response of GCMs to radiative forcing without influence from internal climate variability. Previously, hemispheric mean temperature reconstructions [e.g., M99, CL00] were compared with EBM simulations for the whole Northern Hemisphere [*Free and Robock*, 1999; *Crowley*, 2000]. Since most paleoclimatic data are from the mid- and high-latitudes of the Northern Hemisphere, we employ here a linear North-type [*North et al.*, 1983] 2D (i.e., realistic land-sea distribution) seasonal model to compare the model results over the same season and the same subsection of the Northern Hemisphere that is covered by the data (for example, land only records based on data from 20–90 N are compared with EBM land data from the same latitude strip). The EBM responds similarly as GCMs to changes in boundary conditions, including the seasonal cycle of insolation [*Crowley et al.*, 1991] and shows a similar response to volcanism in the late 20th century [cf. *Stott et al.*, 2000].

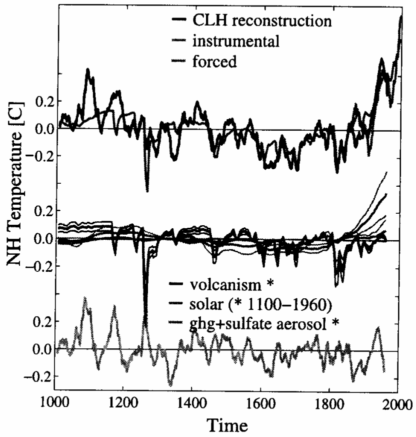

Figure 1. Detection results for the updated *Crowley and Lowery* [2000] reconstruction of decadal Northern Hemispheric mean temperature (north of 30N, calendar year average). Upper panel: Paleo reconstruction (black) compared to the instrumental data (grey) and the best estimate of the combined forced response (red), middle panel: response attributed to individual forcings (thick lines) and their 5–95% uncertainty range (thin lines), lower panel: residual variability attributed to internal climate variability and errors in reconstruction and forced response. An asterisk "*" denotes a response that is detected at the 5% significance level.

[11] The sensitivity of the EBM is set to 2.5 K for CO2 doubling in the present study, since a sensitivity between 2 and 3 yielded reasonable agreement between the amplitude of combined forcing simulations and the CLH paleodata [T. J. Crowley et al., in preparation]. The model is driven by external forcing changes in greenhouse gases, solar irradiance, volcanism, and tropospheric aerosols. The greenhouse gas and solar forcing used in the present study are from

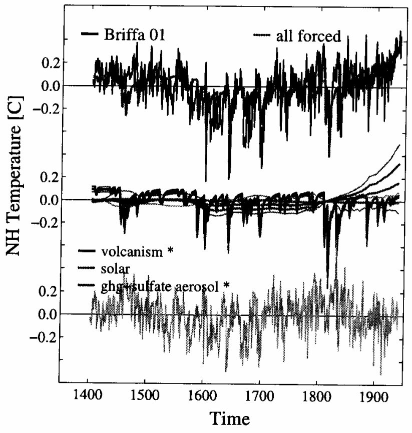

Figure 2. As Figure 1, but based on the *Briffa et al.* [2001] reconstruction of NH growing season temperature (April through September) over land (north of 20 N) 1402 to 1940.

Crowley [2000]. We employ an aerosol forcing of 0.5 W/m^2 for 30–90 N from 1900 on, and 0.3 W/m^2 from 0–30 N. The sulfate aerosol fingerprint is added to the greenhouse gas only fingerprint and then both are estimated together. Results proved insensitive to doubling or omitting the aerosol fingerprint except for a small variation in the estimate of the combined signal.

[12] The volcano forcing time series used in *Crowley* [2000] has been slightly revised [*T. J. Crowley et al.*, in preparation]. In addition to ice core data used previously, we factored in the assessment of *Robock and Free* [1996]. The ice core Aerosol Optical Depth (AOD) was initially determined by matching the 1883 Krakatau peak in the ice cores to the instrumental AOD record [*Sato et al.*, 1993]. Because there is considerable uncertainty about the absolute value of Krakatau AOD estimates, the preliminary scaling for 1900–1960 was validated against the presumably more-reliable 30–90 N 1900–1960 portion of the AOD record. The aerosol optical depth was converted to radiative forcing using a factor of 30 [*Sato et al.*, 1993] determined by detailed radiative convective modeling. A volcano catalog [*Simkin and Siebert*, 1994] was used to assign tentative sources to many of the eruptions. Unknown ice core peaks in the pre-anthropogenic record were assigned a high latitude (>50°) origin unless they could be verified in ice cores from Antarctica [see *Crowley*, 2000]; thus minimizing the effects of ice core volcano peaks unless their larger-scale imprint can be verified from independent data. This proxy-reconstruction agrees in general with other reconstructions of volcanic forcing, and has an estimated uncertainty of ca. 50%.

5. Results

[13] Figure 1 shows the estimated contribution of solar, volcanic and greenhouse gas forcing to the CLH record, and Figure 2 to the B01 record. A comparison of signal estimates and detection results between the different reconstructions is given in Table 1. Results using different reconstructions compare favorably. The results are dominated by low frequency aspects of the signals (low-pass filtering the annual/seasonal data yielded nearly identical results). In all cases, the volcanic signal is highly significant. A CO$_2$/aerosol signal is detected in B01 (although this record can only reliably be used to 1940), in CLH and E02 by 1960 (Table 1), and in M99 by 1980 (not shown).

[14] Attributed signal amplitudes are generally consistent between analysis periods (c.f. Table 1). The observed response to volcanic forcing is consistent with the model simulations in B01, CLH and E02 and tends to be smaller than simulated in M99. This may be, at least partly, due to dynamically induced winter warming [*Robock and Mao*, 1992] which reduces the effect of volcanism on annual means. The greenhouse gas signal is larger than simulated in E02 and smaller than simulated in M99. The residual from the M99 record, after analyzing a period encompassing the 19th century, shows a large fluctuation not explained by either forcing. A possible reason is that the response to other external influences, such as land use change, has a stronger influence on M99 than the other records and disturbs the agreement between simulations and observations. If this period is omitted from the analysis, the estimates of the contributing forcings are generally larger.

[15] Solar signals with an amplitude that is consistent with simulations are detected in a two-way regression between solar and volcanic signals between 1000 and 1830 in M99 and in CLH. However, if the CO$_2$ forcing is included in the analysis, some of the Maunder mimimum cooling gets attributed to a small drop in CO$_2$ (c.f. Figures 1–2. In that case, the response to solar forcing is only

Table 1. Estimated signal amplitudes (unit-less) as scaling factors by which energy balance model simulations need to be scaled for best agreement with observations ("1" indicates a correct amplitude of the simulation) and 5–95% uncertainty levels. Signals that are detected at the 5% significance level (one-sided) are shown in bold, an arrow "↓" ("↑") denotes that the signal amplitude is significantly smaller (larger) than simulated. B01 refers to the *Briffa et al.* [2001] record, CLH to the updated *Crowley and Lowery* [2000] record, M99 to the *Mann et al.* [1999] data and E02 to the *Esper et al.* [2002] record, the period of the analysis is given in the second row. The bottom row lists the standard deviation (K) of the (decadally smoothed for annual records) residual, and the percent variance explained by the external forcing (in parentheses).

Record Period	B01 1400–1940	CLH 400–1960	M99 1400–1960	E02 1400–1960	CLH 1000–1960
volcano	**0.92 ± 0.21**	**1.22 ± 0.39**	**0.63 ± 0.23** ↓	**1.01 ± 0.34**	**1.14 ± 0.40**
solar	−0.1 ± 0.83 ↓	0.18 ± 0.48 ↓	0.43 ± 0.61	−0.18 ± 0.96↓	0.63 ± 0.67
ghg + aer	**1.11 ± 0.55**	**1.13 ± 0.26**	0.26 ± 0.35 ↓	**1.88 ± 0.57** ↑	**0.96 ± 0.42**
res std.	0.09 (57%)	0.08 (77%)	0.07 (49%)	0.13 (67%)	0.10 (57%)

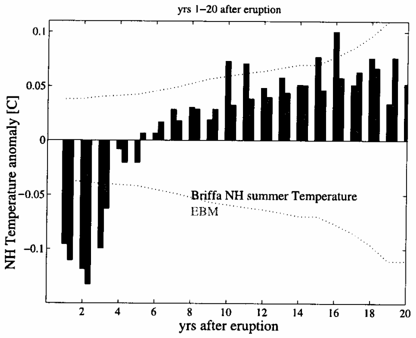

Figure 3. Comparison of the average response to volcanic eruptions in the energy balance model and the *Briffa et al.* [2001] reconstruction from the year of the eruption (year 1) to the next major eruption. 5–95% uncertainty ranges for the observed response are given by the dotted lines (note that sample size decreases with time).

detectable in segments of M99 and in CLH 1100–1960. It is possible that annual averaging and the wintertime dynamical response [*Shindell et al.*, 2001] helps in the detection of the solar signal, particularly in M99.

[16] The EBM response tends to associate inter-decadal temperature variations with periods of unusually heavy or weak volcanism. A similar tendency occurs in coupled climate models, [e.g., *Stott et al.*, 2000]. In order to assess if such a simulated response is realistic, we have conducted a superposed epoch analysis by averaging the temperature response after 50 major volcanic eruptions between 1400 and 1940 (Figure 3). The average considers only the response before the next major eruption. To avoid contamination by other external forcing, the estimated solar and greenhouse gas signal has been subtracted from the observations prior to the analysis (result not sensitive to taking solar forcing into account). The observations show a significant cooling in the first three years of the eruption, which compares very favorably to the magnitude and duration of the simulated response. Afterwards, a marginally significant temperature increase reflects the relaxation to an equilibrium climate state without volcanic forcing in model and data. The results of this analysis are qualitatively confirmed if the *Briffa et al.* [1998] data or the M99 record is used.

6. Conclusions

[17] The response to volcanic forcing is reliably detected in all reconstructions of Northern Hemispheric mean temperature considered, and the simulated timescale of temperature response to volcanic eruptions compares very favorably with observations. Although most records can be only used to the middle of the 20th century, the temperature response to CO_2 can be detected by then in most records (in all by 1980). The response to solar forcing is detected only over some periods in some records. The overall impression is that solar variability plays a relatively modest role in multi-decadal climate variability of hemispherically averaged temperature. The early 20th century warming is attributed to a composite of greenhouse warming, an uncertain contribution from solar forcing, and a recovery from a previous period of heavy volcanism.

[18] **Acknowledgments.** This work has been supported by NOAA grant NA16GP2242 and by NOAA's office of global programs and the DOE's Office of Biological and Environmental Research. GCH is also supported by the Nicholas School of the Environment and Earth Sciences and by NSF grant 0296007. We thank Phil Jones, Keith Briffa, Mike Mann and Ed Cook for valuable discussions and assistance, and two anonymous reviewers for their valuable comments.

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QUESTIONS SURROUNDING THE ‘HOCKEY STICK’ TEMPERATURE STUDIES: IMPLICATIONS FOR CLIMATE CHANGE ASSESSMENTS

THURSDAY, JULY 27, 2006

HOUSE OF REPRESENTATIVES,
COMMITTEE ON ENERGY AND COMMERCE,
SUBCOMMITTEE ON OVERSIGHT AND INVESTIGATIONS,
Washington, DC.

The subcommittee met, pursuant to call, at 2:00 p.m., in Room 2322 of the Rayburn House Office Building, Hon. Ed Whitfield (Chairman) presiding.

Members present: Representatives Stearns, Pickering, Bass, Blackburn, Barton (ex officio), Stupak, Schakowsky, Inslee, Baldwin, Waxman, and Whitfield.

Staff present: Mark Paoletta, Chief Counsel for Oversight and Investigations; Peter Spencer, Professional Staff Member; Tom Feddo, Counsel; Matt Johnson, Legislative Clerk; John Halliwell, Policy Coordinator; Clayton Matheson, Analyst; Mike Abraham, Legislative Clerk; Edith Holleman, Minority Counsel; David Vogel, Minority Research Assistant; Chris Knauer, Minority Investigator; and Lorie Schmidt, Minority Counsel.

MR. WHITFIELD. This hearing will come to order, and I want to certainly welcome everyone to today’s hearing. This is the second day of our hearing regarding questions about what we popularly call the hockey stick temperature studies and the implications for climate change assessments. We have reconvened this hearing to accommodate a key person in the matters before us, and that is Dr. Michael Mann of Penn State University. Dr. Mann was unable to attend the session on the subject last week, and we are looking forward to his testimony.

As you know, he was one of the leaders in the methodology of developing the methodology that developed the hockey stick graph, and we hope we can continue to explore some of the broader questions surrounding temperature reconstruction findings, their use in the IPCC assessment, and other issues that prompted our inquiry into this matter last year. Now the hockey stick graphic and the underlying studies were influential in a prominent set of findings by the IPCC, and really the hockey stick graphic has become an icon for all those concerned about global warming.

In point of fact, from the very first set of findings on the very first page of discussion in its 2001 summary for policy makers the IPCC states that 20th Century temperature increases were likely the largest in 1000 years, and it was likely that in the Northern Hemisphere the 1990s was the warmest decade and 1998 the warmest year, a phrase that is almost verbatim what Dr. Mann and his colleagues wrote in their 1999 paper. Next to these findings the IPCC summary then displays Dr. Mann and his colleagues' hockey stick shaped temperature graph which helped this work prominently and moved it into the public eye.

Now let me just take a moment and make a few observations about last week's hearing. First, through our discussion of both the National Research Council report and the Wegman report the original studies by Mann and his co-authors appeared to be flawed, and cannot support the related findings of the 2001 IPCC assessment. Dr. Wegman's independent committee found and reported that Dr. Mann and his co-authors incorrectly applied a statistical methodology that would preferentially create hockey stick shapes.

Dr. Wegman also found that more recent methodologies used in temperature reconstruction studies may also generate problematic biases when determining temperature histories. Now the National Research Council based on the Mann analysis and newer supporting evidence finds that it is plausible that the Northern Hemisphere was warmer during the last few decades of the 20th Century than during any period comparable in the preceding millennium. Even less confidence, and I am quoting from their report, even less confidence can be placed in the original conclusion by Mann that the 1990s are likely the warmest decade, and 1998 the warmest year.

The NRC's panel review determined that Dr. Mann made in the words of the NRC witnesses inappropriate choices and that the panel had much the same misgivings about Dr. Mann's work, That was documented at much greater length by Dr. Wegman. Moreover, both the NRC and Wegman reports essentially corroborated the main criticisms raised by the McIntyre-McKitrick studies about Dr. Mann's initial hockey stick studies. Now while much attention was given to Dr. Wegman's social network analysis, I think it is only fair to observe the limits of what he was trying to illustrate as he himself explained.

Dr. Wegman was not seeking to impugn the integrity of any of the scientists who work in the area, but it is clear that peer review somehow failed to pick up the flaws in the hockey stick studies. Dr. Wegman simply raises the possibilities that given the evident publishing relationship among the authors of many of the relevant works combined with the failure to involve statisticians that Dr. Manns' peers may have been too close to the topic to scrutinize the studies as rigorously as they might have.

Whatever the case, Dr. Manns' peers failed to catch the errors that Wegman, the NRC and McIntyre identified. Now this failure as Dr. von Storch suggested last week may be less an issue with the community of paleoclimatologists than with the journal editors themselves. Now finally I think it is important to note that virtually everyone at the hearing last week, both members and witnesses, took the view that criticisms of the hockey stick studies or of the peer review and assessment process should not be considered as a judgment about the changes in global temperature, but rather the issues at hand concern legitimate questions about the rigor of scientific analysis, the results of which ultimately reach policy makers and that is what we base our decision-making decisions on.

So the hockey stick story provides a clear case study into what may be the lack of proper scrutiny, and the questions last week about the independence of peer review or the gate keeping issues in my mind are legitimate. And I think that everyone would agree that we must be very careful and make sure that when we do these analyses and they receive the publicity that they do that they be scientifically based and as close to accurate as possible.

Now in addition to Dr. Mann, both Dr. Wegman and Dr. McIntyre are returning to recap their testimony and to answer any questions related to their work, and certainly Dr. Mann may want to raise some issues regarding what you all said. We have a few additional panelists as well. As we were preparing this panel, some have been suggested by the minority side, and I am not sure which ones, but I want to welcome Dr. John Christy, the Director of the Earth System Science Center, and an Alabama State climatologist at the University of Alabama, Huntsville, and Dr. Gulledge of the Pew Center for Climate Change. And then finally I would like to recognize Dr. Ralph Cicerone, who is the President of the National Academy of Sciences, and happened to be in the same fraternity that I was, so, Dr. Cicerone, welcome.

And he has been instrumental in the National Academy's focus on climate change research in recent years. Indeed, he chaired the National Research Council's 2001 report for President Bush that helped pave the way for the United States to conduct its own climate change assessment. I want to welcome all of you. Thank you for your time. We look forward to your testimony. And I yield and recognize the distinguished ranking member, Mr. Stupak.

[The prepared statement of Hon. Ed Whitfield follows:]

PREPARED STATEMENT OF THE HON. ED WHITFIELD, CHAIRMAN, SUBCOMMITTEE ON OVERSIGHT AND INVESTIGATIONS

Good afternoon and welcome to a second day of our hearing regarding questions about what we popularly call the “hockey stick” temperature studies and the implications for climate change assessments.

We’ve reconvened this hearing to accommodate a key person in the matters before us, Dr. Michael Mann, of Penn State University. Dr. Mann was unable to attend the informative session on this subject last week. Although Dr. Thomas Crowley – Dr. Mann’s personally recommended replacement – did testify, we are providing Dr. Mann the opportunity to discuss his work and respond to some of the views expressed about his work.

Welcome Dr. Mann, I’m looking forward to your testimony and participation. I hope we can continue to explore some of the broader questions surrounding temperature reconstruction findings, their use in the IPCC assessment, and other issues that prompted our inquiry into this matter last year.

The hockey stick graphic and the underlying studies were influential in a prominent set of findings by the IPCC. In point of fact, from the very first set of findings on the very first page of discussion in its 2001 *Summary for Policymakers*, the IPCC states that 20th Century temperature increases were likely the largest in 1,000 years and it was [quote] “likely that, in the Northern Hemisphere, the 1990s was the warmest decade and 1998 the warmest year,” a phrase that is almost verbatim what Dr. Mann and his colleagues wrote in their 1999 paper. Next to these findings, the IPCC Summary then displays Dr. Mann and his colleagues’ hockey stick-shaped temperature graph, which helped this work prominently into the public eye.

Let me take a moment and make few observations about last week’s hearing.

First, through our discussion of both the National Research Council report and the Wegman report, we established that the original studies by Mann and his coauthors were flawed, and could not support the related findings of the 2001 IPCC assessment. Dr. Wegman’s independent committee found and reported that Dr. Mann and his coauthors incorrectly applied a statistical methodology that would preferentially create hockey stick shapes. Dr. Wegman also found that more recent methodologies used in temperature reconstruction studies may also generate problematic biases when determining temperature histories.

The National Research Council, upon its review of the current state of science on this subject, likewise found that the hockey stick studies could not support the 2001 IPCC finding drawn from them. The NRC panel’s review determined that Dr. Mann made, in the words of the NRC witnesses, “inappropriate” choices, and that the panel had “much the same misgivings about [Dr. Mann’s] work that was documented at much greater length by Dr. Wegman.”

Moreover, both the NRC and Wegman reports essentially corroborated the main criticisms raised by the McIntyre-McKitrick studies about Dr. Mann’s initial hockey stick studies.

While much attention was given to Dr. Wegman’s social network analysis, I think it is only fair to observe the limits of what he was trying to illustrate, as he himself tried to explain.

Dr. Wegman was not seeking to impugn the integrity of any of the scientists who work in this area, but it is clear that peer review somehow failed to pick up the flaws in the hockey stick studies. Dr. Wegman simply raises the possibility that, given the evident publishing relationship among the authors of many of the relevant works, combined with the failure to involve statisticians, Dr. Mann’s peers may have been too close to the topic to scrutinize the studies as rigorously as they might have. Whatever the case, Dr. Mann’s peers failed to catch the errors Wegman, the NRC, and McIntyre identified.

This failure, as Dr. von Storch suggested last week, may be less an issue with the community of paleoclimatologists, than with the journal editors themselves. The Committee can remain cautious about Dr. Wegman's social network analysis, as he is, and still legitimately raise the broader question about the rigor of review and breadth of reviewers in this field.

Finally, I think it is important to note that virtually everyone at the hearing last week – both members and witnesses – took the view that criticisms of the hockey stick studies or of the peer-review and assessment process should not be construed as a judgment about the changes in global temperatures.

Rather, the issues at hand concern legitimate questions about the rigor of scientific analysis, the results of which ultimately reach policy makers. The hockey stick story provides a clear case study into the lack of proper scrutiny, and the questions last week about the independence of peer-review, or the "gate keeping" issues, were entirely legitimate. I hope that as we proceed today, we keep this in mind. And I hope that we can all reach agreement on ways to improve the process.

Let me note that we have, in addition to Dr. Mann, both Dr. Wegman and Mr. McIntyre returning to recap their testimony and to answer questions related to their work, if necessary. Both of them graciously agreed to adjust their busy schedules, including family and work obligations, to return today at our request so that Dr. Mann could confront his critics. Thank you very much for coming back.

We have a few additional panelists as well. As we were preparing this panel, our minority counterparts requested an additional witness. In the event, we accommodated their requests so that we could have as informative and balanced a panel as possible.

So let me welcome Dr. John Christy, the Director of the Earth System Science Center and Alabama State Climatologist at the University of Alabama, Huntsville and Dr. Jay Gulledge, of the Pew Center for Climate Change.

Finally, I'd like to recognize a most-distinguished witness, Dr. Ralph Cicerone [sisserone], President of the National Academy of Sciences. Dr. Cicerone has been instrumental in the National Academies' focus on climate change research in recent years. Indeed, he chaired the National Research Council's 2001 report for President Bush that helped pave the way for the United States to conduct its own climate change assessments.

Welcome Dr. Cicerone, and welcome all the witnesses, I look forward to another informative panel.

I now yield to my distinguished Ranking Member, Mr. Stupak.

MR. STUPAK. Thank you, Mr. Chairman. Today we are holding a very strange hearing. Originally scheduled to give Dr. Michael Mann a chance to respond to critics who provided testimony to this committee last week, this hearing has now expanded to allow these critics to attack the very science of global warming. Witnesses reappearing in the committee today, once commissioned by the Majority to do a very limited and biased review, had attempted to discredit Dr. Mann's 8-year old study on reconstruction of surface temperatures over the last thousand years, and his conclusion that the earth is warming at an unprecedented rate.

However, as Dr. North testified last week, a comprehensive review of temperature reconstruction research by the National Academy of Science at the request of the Science Committee found that there were numerous other studies concluding that the Earth is warming at an unprecedented rate. Now instead of allowing Dr. Mann to respond to last week's allegations, two of our witnesses, apparently unhappy with the outcome of last week's hearing have decided to rewrite and expand their testimony to raise new issues, new complaints, and new questions.

This re-written testimony is no longer limited to Dr. Mann's statistical methods and their own work, but also includes areas of climatology totally outside their expertise. As a result, it appears that these critics have lost interest in simply attacking Dr. Mann's work. Now the purpose of today's hearing is to cast doubt on all scientific evidence of global warming. Mr. Chairman, if we are going to discuss the larger issue of global warming, which many of us on this side would be happy to do, we need to put more time and effort into putting together a series of well thought out hearings with adequate time for witnesses and staff to prepare.

If the Majority were truly interested only in temperature reconstruction over the past thousand years we could have heard from all of the scientists who have worked on this topic both before and after Dr. Mann's original 1998 and 1999 publications. Instead, the Majority asked Dr. Wegman, a statistician with no expertise in paleoclimatology, to verify only Mr. McIntyre's critique of Dr. Mann's initial work. Dr. Wegman was not even asked if Dr. Mann's conclusions would change if the criticisms were incorporated and the analysis were re-created, nor did he volunteer to do that.

Other climatologists have recreated Dr. Mann's work and have come to the same conclusions using both similar and different data sets and methodologies. Dr. Wegman, who has not reviewed this work and did not discuss any of the studies in his testimony last week, will try to discredit all of these studies with an unsupported hypothesis questioning the independence of a large group of scientists work.

Another witness we will hear from today, Dr. Christy, has supported the science behind global warming but will argue that by acting to curb global warming we may deny the poor in other countries the advantages that we have here in America. This is also the argument of a new group, the Interfaith Stewardship Alliance, but we have not heard from the alliance when trying to provide low-income emergency assistance for people in my district.

However, the threat of rising temperatures and the negative results of them, including diminished agricultural production, and quite possibly the flooding of vast heavily populated coastal areas due to the melting of the polar ice caps, can be far more of a threat to developing countries than efforts to limit harmful industrial emissions. The National Climatic Data Center has recently confirmed that the first half of 2006 was the warmest first half of any year in the United States since 1895. This warming trend is continuing.

Today's headline in the Washington Post, I should say Tuesday's headline in the Washington Post, "Deadly Heat Continues in California." The morgue in Fresno, California has many bodies of elderly people overcome by heat. Unprecedented temperatures have been recorded recently in Oregon and South Dakota, among other places. Forty-five percent of the United States is in moderate to extreme drought conditions. These conditions have spawned more than 50,000 wildfires burning approximately 4 million acres.

Congress is not particularly capable to judge science that deals with linear regressions, Pearson's R square, centering and de-centering, or regulized expectation maximization. As Dr. Cicerone will remind us, that is why Congress created the National Academy of Science. We are, however, able to understand the strategy of Exxon Mobil, outlined in their 1998 action plan. This plan argued, and I quote, "victory will be achieved when average citizens understand, recognize uncertainties in climate science." This appears to be the focus of today's hearing, to confuse and complicate the findings of climate scientists, and Dr. Mann is unfortunately in the crosshairs. I yield back the balance of my time.

[Additional information submitted for the record follows:]

Dominion. Stewardship. Conservation.

Interfaith
Stewardship
Alliance

An Open Letter to the Signers of "Climate Change: An Evangelical Call to Action" and Others Concerned About Global Warming

"They only asked us to remember the poor–the very thing I was eager to do."
–The Apostle Paul, Galatians 2:10

Widespread media reports tell of a scientific consensus that:

- the world is presently experiencing unprecedented global warming;
- the main cause of it is rising atmospheric carbon dioxide because of human use of fossil fuels for energy; and
- the consequences of continuing this pattern will include (1) rising sea levels that could inundate highly populated and often poor low-lying lands, (2) more frequent deadly heat waves, droughts, and other extreme weather events, (3) increased tropical diseases in warming temperate regions, and (4) more frequent and intense hurricanes.

Recently eighty-six evangelical pastors, college presidents, mission heads, and other leaders signed "Climate Change: An Evangelical Call to Action," under the auspices of the Evangelical Climate Initiative. The document calls on the federal government to pass national legislation requiring sufficient reductions in carbon dioxide emissions to fight global warming and argues that these are necessary to protect the poor from its harmful effects.

In light of all this, many people are puzzled by the Interfaith Stewardship Alliance's opposition to such calls. Do we not *care* about the prospect of catastrophic global warming? Do we not *care* that with rising temperatures the polar ice caps will melt, and the sea will inundate low island countries and coastal regions? Do we not *care* that the world's poor might be most hurt by these things?

Yes, we care. But we also believe, with economist Walter Williams, that "truly compassionate policy requires dispassionate analysis." That is the very motive for our opposing drastic steps to prevent global warming. In short, we have the same motive proclaimed by the Evangelical Climate Initiative in its "Call to Action."

But motive and reason are not the same thing. It matters little how well we mean, if what we do actually harms those we intend to help.

That is why we take the positions we do. In the accompanying document, **"A Call to Truth, Prudence, and Protection of the Poor: An Evangelical Response to Global Warming,"** we present extensive evidence and argument against the extent, the significance, and perhaps the existence of the much-touted scientific consensus on catastrophic human-induced global warming. Further, good science–like truth–is not about counting votes but about empirical evidence and valid arguments. Therefore we also present data, arguments, and sources favoring a different perspective:

- Foreseeable global warming will have moderate and mixed (not only harmful but also helpful), not catastrophic, consequences for humanity–including the poor–and the rest of the world's inhabitants.

- Natural causes may account for a large part, perhaps the majority, of the global warming in both the last thirty and the last one hundred fifty years, which together constitute an episode in the natural rising and falling cycles of global average temperature. Human emissions of carbon dioxide and other greenhouse gases are probably a minor and possibly an insignificant contributor to its causes.

- Reducing carbon dioxide emissions would have at most an insignificant impact on the quantity and duration of global warming and would not significantly reduce alleged harmful effects.

- Government-mandated carbon dioxide emissions reductions not only would not significantly curtail global warming or reduce its harmful effects but also would cause greater harm than good to humanity–especially the poor–while offering virtually no benefit to the rest of the world's inhabitants.

- In light of all the above, the most prudent response is not to try (almost certainly unsuccessfully and at enormous cost) to prevent or reduce whatever slight warming might really occur. It is instead to prepare to adapt by fostering means that will effectively protect humanity–especially the poor–not only from whatever harms might be anticipated from global warming but also from harms that might be fostered by other types of catastrophes, natural or manmade.

We believe the harm caused by mandated reductions in energy consumption in the quixotic quest to reduce global warming will far exceed its benefits. Reducing energy consumption will require significantly increasing the costs of energy–whether through taxation or by restricting supplies. Because energy is a vital component in producing all goods and services people need, raising its costs means raising other prices, too. For wealthy people, this might require some adjustments in consumption patterns–inconvenient and disappointing, perhaps, but not devastating. But for the world's two billion or more poor people, who can barely afford sufficient food, clothing, and shelter to sustain life, and who are without electricity and the refrigeration, cooking, light, heat, and air conditioning it can provide, it can mean the difference between life and death.

Along with all the benefits we derive from economic use of energy, another consideration–a Biblical/theological one–points in the same direction. The stewardship God gave to human beings over the earth–to cultivate and guard the garden (Genesis 2:15) and to fill, subdue, and rule the whole earth (Genesis 1:28)–strongly suggests that caring for human needs is compatible with caring for the earth. As theologian Wayne Grudem put it, "It does not seem likely to me that God would set up the world to work in such a way that human beings would eventually destroy the earth by doing such ordinary and morally good and necessary things as breathing, building a fire to cook or keep warm, burning fuel to travel, or using energy for a refrigerator to preserve food."

Whether or not global warming is largely natural, (1) human efforts to stop it are largely futile; (2) whatever efforts we undertake to stem our small contributions to it would needlessly divert resources from much more beneficial uses; and (3) adaptation strategies for whatever slight warming does occur are much more sensible than costly but futile prevention strategies. Therefore, we believe it is far wiser to promote economic growth, partly through keeping energy inexpensive, than to fight against potential global warming and thus slow economic growth. And there is a side benefit, too: wealthier societies are better able and more willing to spend to protect and improve the natural environment than poorer societies. Our policy, therefore, is better not only for humanity but also for the rest of the planet.

We recognize that reasonable people can disagree with our understanding of the science and economics. But this is indeed our understanding.

Please join us in endorsing **"A Call to Truth, Prudence, and Protection of the Poor: An Evangelical Response to Global Warming."** To do so, send an e-mail with your name, degree(s) (with subject, level, and granting institution), professional title, professional affiliation (for identification purposes only), mailing address, e-mail address, and (for verification) phone number to isa@interfaithstewardship.org. If you have questions, please e-mail the same address.

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(Updated July 21, 2006)

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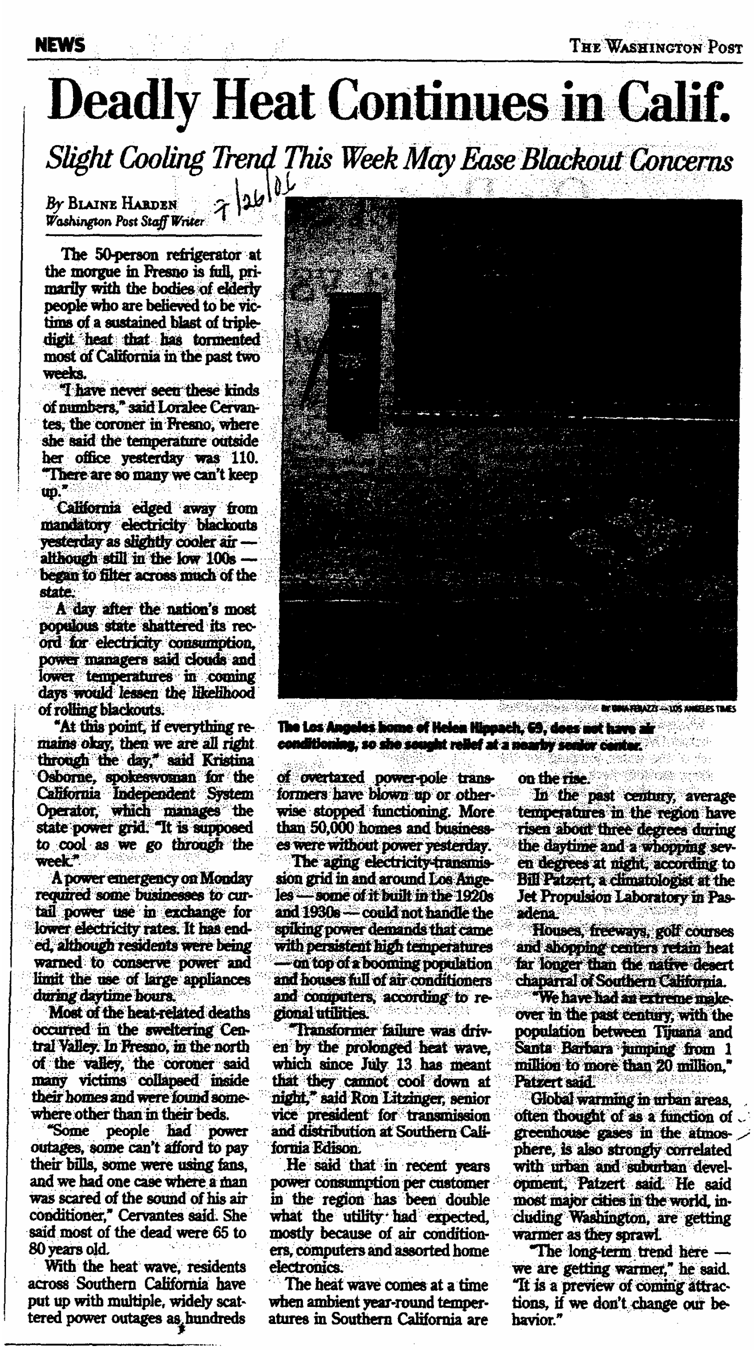


NEWS The Washington Post

Deadly Heat Continues in Calif.

Slight Cooling Trend This Week May Ease Blackout Concerns

7/26/06

By Blaine Harden
Washington Post Staff Writer

The 50-person refrigerator at the morgue in Fresno is full, primarily with the bodies of elderly people who are believed to be victims of a sustained blast of triple-digit heat that has tormented most of California in the past two weeks.

"I have never seen these kinds of numbers," said Loralee Cervantes, the coroner in Fresno, where she said the temperature outside her office yesterday was 110. "There are so many we can't keep up."

California edged away from mandatory electricity blackouts yesterday as slightly cooler air — although still in the low 100s — began to filter across much of the state.

A day after the nation's most populous state shattered its record for electricity consumption, power managers said clouds and lower temperatures in coming days would lessen the likelihood of rolling blackouts.

"At this point, if everything remains okay, then we are all right through the day," said Kristina Osborne, spokeswoman for the California Independent System Operator, which manages the state power grid. "It is supposed to cool as we go through the week."

A power emergency on Monday required some businesses to curtail power use in exchange for lower electricity rates. It has ended, although residents were being warned to conserve power and limit the use of large appliances during daytime hours.

Most of the heat-related deaths occurred in the sweltering Central Valley. In Fresno, in the north of the valley, the coroner said many victims collapsed inside their homes and were found somewhere other than in their beds.

"Some people had power outages, some can't afford to pay their bills, some were using fans, and we had one case where a man was scared of the sound of his air conditioner," Cervantes said. She said most of the dead were 65 to 80 years old.

With the heat wave, residents across Southern California have put up with multiple, widely scattered power outages as hundreds of overtaxed power-pole transformers have blown up or otherwise stopped functioning. More than 50,000 homes and businesses were without power yesterday.

The aging electricity-transmission grid in and around Los Angeles — some of it built in the 1920s and 1930s — could not handle the spiking power demands that came with persistent high temperatures — on top of a booming population and houses full of air conditioners and computers, according to regional utilities.

"Transformer failure was driven by the prolonged heat wave, which since July 13 has meant that they cannot cool down at night," said Ron Litzinger, senior vice president for transmission and distribution at Southern California Edison.

He said that in recent years power consumption per customer in the region has been double what the utility had expected, mostly because of air conditioners, computers and assorted home electronics.

The heat wave comes at a time when ambient year-round temperatures in Southern California are on the rise.

In the past century, average temperatures in the region have risen about three degrees during the daytime and a whopping seven degrees at night, according to Bill Patzert, a climatologist at the Jet Propulsion Laboratory in Pasadena.

Houses, freeways, golf courses and shopping centers retain heat far longer than the native desert chaparral of Southern California.

"We have had an extreme makeover in the past century, with the population between Tijuana and Santa Barbara jumping from 1 million to more than 20 million," Patzert said.

Global warming in urban areas, often thought of as a function of greenhouse gases in the atmosphere, is also strongly correlated with urban and suburban development, Patzert said. He said most major cities in the world, including Washington, are getting warmer as they sprawl.

"The long-term trend here — we are getting warmer," he said. "It is a preview of coming attractions, if we don't change our behavior."

BY GINA FERAZZI — LOS ANGELES TIMES

The Los Angeles home of Helen Hippach, 69, does not have air conditioning, so she sought relief at a nearby senior center.

MR. WHITFIELD. The chair recognizes the chairman of the Energy and Commerce Committee, Mr. Barton of Texas.

CHAIRMAN BARTON. Thank you, Mr. Chairman, and thank you for this hearing. I want to thank our witnesses for being here, some of them for the second time. We are obviously glad to have Dr. Mann here. We appreciate you being able to join us. It is clear from last week's hearing on global climate temperature studies that we face issues involving more than the particulars of Dr. Mann's specific hockey stick study. However, it is the particulars of these studies and how the existing climate assessment process has dealt with them that got us here today.

I appreciate the participation of this panel. I am glad that Dr. Ralph Cicerone is here. He is the President of the National Academy of Sciences. I think he is going to add significant weight and gravitus to the hearing today. As you noted in your statement, Chairman Whitfield, last week's hearing demonstrated why we as policymakers need to understand the quality and the reliability of the science on which we are urged to base public policy that is both sweeping and costly. Some very respected and authoritative sources testified last week that Dr. Mann's studies were flawed. They couldn't support the findings for which they were used in the United Nations Climate Change Assessment, the IPCC. Today I hope that we are going to examine some of these issues in more detail.

I recognize that additional work has been published that supports the broad outline of some of those conclusions in Dr. Mann's initial hockey stick study, but according to the National Research Council even that subsequent work cannot provide the level of confidence that IPCC placed upon the original hockey stick analysis. Nothing about the process of turning observations into accepted theory is smooth. It has been said that the politics of small towns and big universities are brutal. They make us look amateurs by comparison. Looking at what is happening in this issue, I think that might well be true. Unfortunately, that is the way this science progresses.

I not only accept it, bumps included, but, believe it or not, I support it. What I can't accept is the improbable notion that this committee may not ask science or research-related questions that bear on policy making when the answers could improve the information we use to reach the policy decisions that we are elected to make. It is just wrong to say that questions are not permitted, free debate is improper or that anyone who wonders if the scientific establishment really has it right should be dismissed as anti-science or oblivious to the real risk of man-made climate change.

This committee holds a very key role in any policy-making decision related to climate change. As its Chairman, I have an obligation to be cognizant of that and to do everything possible to get a fair record but also get into the details of some of the theories that the policies, the recommended policies, are supposedly based upon. We are interested in Dr. Mann's work, not because of Dr. Mann, as nice a fellow as he may or may not be; we are interested in Dr. Mann's work because it was the original. It was seminal. It is referred to.

I haven't seen Vice President Gore's movie, but I am told in that movie Dr. Mann's hockey stick diagram is shown repeatedly. It is only fair to take a look at the original seminal work to see if it really lives up to what it claims to be. During our last hearing, we were shrugged at for asking about that particular study saying it was too early, too distant, but the fact is that that particular study is the study that much of the latter conclusions have been based upon. It is only common sense to take a look at it. We are going to work on the issue, and if it turns out that that study is not the right study and if there are more current studies that are more correct, we will take a look at those too and we will find out what the truth is. The truth is the truth. The truth may be inconvenient. It may be politically incorrect, but the truth is the truth.

A couple of months ago Chairman Whitfield and I asked the U.S. Government Accountability Office to help us examine Federal data sharing policies especially as they related to climate change research. This work will help our efforts to improve the exchange of scientific data and other essential information, which as we have seen has been a particular problem in the climate change arena. When the dust settles on these hearings, I am going to prepare a request to the National Research Council, which Dr. Cicerone who is with us today chairs, to take some of the issues that Dr. Wegman and others have raised and take a look at it.

I am going to ask for a study to assess how to include a wider spectrum of scientific disciplines in climate change research so that we can be enlightened by the very best work across the field of scientific research. I am going to ask that this study be coordinated and run though the NRC's Division on Engineering and Physical Sciences so that we can ensure that the disciplines like mathematics and physics and statistics participate up front. I would be happy to hear any of Dr. Cicerone's comments on that today as we go forward.

Letting a wider scientific community address questions about climate change assessments can only help the process and improve the results. We have an obligation on this committee on behalf of the American people to ensure that the decision makers have the best information possible, not just the politically correct information. I want to thank again our panel for coming. I want to especially thank Dr. Mann for changing his schedule to be here. I look forward to a very productive exchange of views as we go forward today. Thank you, Mr. Chairman.

[The prepared statement of Hon. Joe Barton follows:]

PREPARED STATEMENT OF THE HON. JOE BARTON, CHAIRMAN, COMMITTEE ON ENERGY AND COMMERCE

Thank you, Chairman Whitfield. It is clear from last week's hearing on global climate temperature studies that we face issues involving much more than the particulars of Dr. Mann's "hockey stick" studies. However, it is the particulars of these studies – and how the existing climate assessment process dealt with them – that got us here today. And so I appreciate that Dr. Mann accepted our invitation to lay out his important work on global temperature reconstruction, as well as to answer our broader questions concerning climate change assessments.

I also appreciate the participation and perspective of our distinguished panelists today, including Dr. Ralph Cicerone, the President of the National Academy of Sciences. Let me also welcome back Dr. Wegman and Mr. McIntyre, who testified last week.

As you noted, Chairman Whitfield, last week's hearing demonstrated why we as policymakers need to understand the quality and reliability of the science on which we are urged to base policy that is both sweeping and costly. Some very respected and authoritative sources testified last week that Dr. Mann's studies were flawed, and that they couldn't support the findings for which they were used by the United Nation's climate change assessment, the IPCC. Today I hope we can examine some of these issues a bit more.

I do recognize that additional work has been published that supports in broad outline some of the conclusions of Dr. Mann's initial "hockey stick" studies. But according to the National Research Council, even that subsequent work cannot provide the level of confidence that IPCC placed upon the hockey stick studies.

Nothing about the process of turning observations into accepted theories is smooth. It has been said that the politics of small towns and big universities are brutal enough to make our kind look amateurish by comparison, and I think that might be true. In any case, that's the way science progresses. I not only accept it -- bumps included -- but I support it.

What I can't accept is the improbable notion that this committee may not ask science- or research-related questions that bear on policymaking when the answers could improve the information we use to reach those policy decisions. It is just wrong to say that questions are not permitted, or that free debate is improper, or that anyone who wonders if the scientific establishment really has it right should be dismissed as anti-science or oblivious to the real risks of manmade climate change. Because this Committee holds a key role in any policymaking relating to climate change, as its Chairman I will do everything I can to ensure that the very best information on these issues is available to us.

We're interested in Dr. Mann's work because it is seminal. During our last hearing, some shrugged at it as distant and early, but the fact is that Dr. Mann's conclusions influence both current research and global policy. As we try to close the loop on our concerns, I also want to emphasize that this Committee will continue to work on the issues raised here, to help ensure the reliability of future scientific assessments.

A couple of months ago, Chairman Whitfield and I asked the U.S. Government Accountability Office to help us examine federal data sharing policies, especially as they related to climate change research. This work will help our efforts to improve the exchange of scientific data and other essential information – which as we have seen has been a particular problem in this climate change arena.

Also, when the dust settles on these hearings, I'm going to prepare a request to the National Research Council, which Dr. Cicerone chairs, to take on some of the issues that Dr. Wegman and others have raised for us. I will ask for a study that assesses how to include a wider spectrum of scientific disciplines in climate change research so that we can be enlightened by the very best work that our scientists conduct, all of them. I'll ask that this study be coordinated and run though the NRC's Division on Engineering and Physical Sciences, so that we can ensure that disciplines like mathematics, physics, and statistics participate up front. I'll welcome Dr. Cicerone's perspective on this today, so that we can formulate an effective request.

Letting a wider scientific community address questions about climate change assessments can only help the process and improve the results. We have an obligation on this Committee to ensure that America's decision-makers have the best information possible.

Thank you all for coming to testify today. I yield back the remainder of my time.

MR. WHITFIELD. Mr. Waxman of California is recognized.

MR. WAXMAN. Thank you very much, Mr. Chairman. The magnitude of global warming and the crisis that we are facing on this planet demands a serious response from this body. We should be holding hearings to understand the ramifications of recent studies detailing the harmful effects of global warming that we are seeing all around us from increased wildfires in the west to more intense hurricanes, more acidic oceans. We should examine practical steps this Congress and the Administration must take to reduce global warming pollution. We should explore how best to re-engage with the international community on addressing this problem because this is going to require all countries to do their part.

We should investigate the well-funded effort by certain oil companies to manufacture controversy and cast doubt on the reality of global warming and the human contribution to it. This hearing today is the third that this committee has held on the issue of global warming. We are the committee that would move legislation forward on this subject, and this is really a continuation of the second one, which was last week. In that hearing, the Republican majority attempted to discredit a respected climate scientist and a study he published 8 years ago. Well, not only is this use of the subcommittee ridiculous and unfair, it is also a waste. Yet, despite its intended focus, today's hearing does give us the opportunity to learn more about the current state of climate science, and I am looking forward to hearing the views of Dr. Ralph Cicerone, who is the President of the National Academy of Sciences, and the Chairman of the National Research Council and a fraternity brother of the Chairman of this subcommittee, and he is an eminent climate scientist.

I am also very pleased we are going to hear from Dr. Mann, who is one of the world's most distinguished paleoclimatologists. Eight years ago, Dr. Mann and his colleagues published a groundbreaking study that reconstructed the temperature of the Earth over the past 600 years using proxy data such as tree rings. Since 2002, Dr. Mann has published another half dozen papers revising and building on his work. These latter studies, as well as many independent paleoclimate reconstructions by other scientists continue to find the same thing. The warmer temperatures in the last few decades are unprecedented compared to anything we have experienced in the last thousand years.

Now the Majority, the Republicans, won't use this hearing to examine Dr. Mann's recent studies or the independent confirmation of its work. Instead, they want to focus exclusively on his original work in 1998 and 1999 because they think they can find a statistical flaw. So what? The strategy is not a subtle one. Because they think they found a flaw in one study out of thousands the Majority wants to build the one study into the pillar of the scientific case for global warming. The Chairman seems to think that if he can discredit one climate scientist, Dr. Mann, he can cast doubt on all the climate change research. In effect, it is back to the tactics of the tobacco industry.

I remember well when they would send their scientists to come in and just cast a little doubt about whether smoking cigarettes really do cause cancer, whether there is really a medical problem. I think intimidation is part of the strategy we are seeing. This subcommittee launched this campaign against Dr. Mann and several of his colleagues last year by demanding to know the source of funding for every study they had ever conducted and demanding that they turn over all the data for all their research. These are bullying tactics and they drew highly unusual protests from the American Association for the Advancement of Science, the National Academy of Sciences, and the Republican Chairman of the House Science Committee, among others.

Well, we are having Dr. Mann here today. It is important that he be here. Last week we held a hearing where he was criticized. Now he has got his accusers back again. They couldn't wait to have the hearing where all of them were together. But this subcommittee will hear about Dr. Mann's work from him and those who criticize him. The subcommittee will hear the many other completely independent lines of evidence that support the reality of global warming and the role of humans in causing it.

The scientific evidence of human contribution to global warming is clear and compelling. The only open question is how long members of this subcommittee will keep pretending that it doesn't exist. I don't know how many hearings we are going to have on the subject of Dr. Mann's one study in 1998, but it seems to me that as we look around this country and in in fact all around the world just today we are seeing a continuation of some of the highest temperatures on record. We ought to get serious about this matter of global warming and climate change. We ought to be holding hearings about the important issues that relate to it and not this one issue over and over again. I yield back the balance of my time.

MR. WHITFIELD. I would point out that even though Dr. Mann was not here last week, he did suggest that Dr. Crowley come on his behalf, and Dr. Crowley did testify. I recognize the gentleman from Mississippi for an opening statement.

MR. PICKERING. Mr. Chairman, I thank you for this hearing, and I yield back my time. I want to get to the panel as quickly as possible.

MR. WHITFIELD. Mrs. Blackburn, you are recognized.

MRS. BLACKBURN. Thank you, Mr. Chairman. I do want to thank you for the hearing, and I want to welcome all of our witnesses. I want to thank you for being patient with us and allowing us some more time to visit with you. At last week's hearing we did hear testimony regarding errors in Dr. Mann's 1998 and 1999 hockey stick report, and today we are going to be able to hear Dr. Mann's response to that. We are pleased to have him join us and are looking forward to that response.

I do still have some questions, and I find some of the circumstances involving Dr. Mann's paper a little bit disconcerting. It seems that it could only be corroborated by a social network and that seems to be a problem. It is difficult for me to see how scientists and policy makers could agree with and legislate anything based on research which by all appearances cannot be corroborated by independent review. Second, it is apparent that until now no independent experts have examined Dr. Mann's data and statistical procedures.

Again, it is difficult to rely on data that has not been rigorously examined for consistency and validity. I am looking forward to some answers on that, and I would not say that it is intimidation that has brought questions forward. I would say it really is curiosity and a desire to know answers. Finally, I have noticed a trend, and this trend raises questions, and it is that trend by where a close group of scientists who support climate change theory tend to be serving as the primary peer reviewers and the lack of that independent review, and those reviewers are checking one another's work. And it may be strictly coincidence but again it does not lead me to believe these papers are being as thoroughly examined as they might by those that are independent, and the public is not being as well served as they should of what they are told is scientific proof.

It is critical that even if we should discount the 1990 IPCC report, recent analysis of over 250 climate studies and historical records showed that the medieval warm period was global and higher than present day temperature, and they both concurred that the little Ice Age occurred worldwide and produced a substantial drop in the average temperature. Also, satellite data and the U.S. surface record indicate that the Earth's temperature in the past 100 years has undergone both warming and cooling trends.

Last week I mentioned in 1960 when I was in high school there was a commonly held premise that we were returning to the Ice Age and by the time I reached my current age and a new millennium dawned we would be in a perpetual winter with food shortages, et cetera. So we had that, that we were dealing with in a cooling trend and that we were being taught as high schoolers in the '60s, but recent trends seemed to be caused by solar activity in the 1990 El Nino, not necessarily by the increase of green house gas emissions.

Mr. Chairman, policy makers depend on the integrity of data. The public depends on the integrity of data. Educational institutions depend on the integrity of data and results, and I believe it is necessary and proper for us to set quality standards for data release and verification for any research that receives Federal funds. Thank you, and I yield back.

MR. WHITFIELD. Thank you. I recognize Ms. Baldwin of Wisconsin for her opening statement.

MS. BALDWIN. Thank you, Mr. Chairman. Again, we are here discussing global warming, and again I think our focus is off target. Rather than addressing action steps to address global warming in a bipartisan coordinated and effective manner, we are covering up the real issues with irrelevant chatter about the basis of a study that was released almost a decade ago, a study that has been updated, revised, reviewed, and validated time and time again in recent years. Unfortunately, the goal of these hearings is not to show that there is an abundance of science demonstrating that the Earth is warming at an unprecedented rate, that carbon dioxide levels are rising, and that human activities are largely the cause. Rather it is an attempt to poke holes in an old study and divert attention away from the decisions that we as policy makers often have to make.

Decisions like should we let big business profits override human interests or should our policy time horizon be a few short years or should we be thinking about protecting generations yet to come. For if this hearing and even Dr. Wegman's analysis were not commissioned for political reasons but rather out of a concern that study after study shows the Earth is warming, sea levels rising and snow caps melting, then we would be focusing on current information. The committee would have asked Dr. Wegman to review Dr. Mann's and other reputable scientists' work that has been published in recent years. But this is not what the committee requested nor what Dr. Wegman studied.

Instead, the focus is on Dr. Mann's 1998 and 1999 study that contains acknowledged flaws. The argument made over the last week against Dr. Mann's early work are old and tired and really I believe their desperate attempts to divert attention away from what countless experts agree, that climate change is happening, the global warming is happening, and that our actions, things we as humans do each and every day, contribute to this crisis. It is troubling that the United States appears to be alone on this island of skeptics. More troubling is that we are virtually alone and are inaction.

Despite being the largest consumer of electricity, oil, and natural gas, we refuse to take bold steps that will allow us to lead the world on environmental issues, yet countries with significantly smaller footprints on the world are making incredible advances that improve the quality of the air they breathe, the food and water they consume, and the lifestyles they lead. Let me just give a couple of examples. China's fuel economy standards are more stringent than those in Australia, Canada, California, and the United States. Meanwhile, we haven't increased our fuel economy standards in over 20 years.

Brazil's ethanol program, the largest in the world, has created rural jobs, reduced air pollution, and reduced Brazil's green house gas emissions while reducing its dependence on imported oil, yet we refuse to take necessary steps to reduce our dependence on foreign energy. Denmark has the highest utilization rate of wind energy in the world with wind producing approximately 20 percent of Danish electrical consumption. Meanwhile, our government has issued notices of presumed hazard to wind developers in the Midwest halting and threatening to permanently derail wind production.

And just yesterday Northern Ireland announced that all new homes built starting in 2008 must have solar roof panels. In this country, I look forward to the day when we take this bold step. Mr. Chairman, we could spend the next few hours discussing the fine points of Dr. Mann's 1998 and 1999 study, and Dr. Wegman's analysis of it, or we could focus on what is really going on. The Earth's temperature is rising and has reached levels higher than ever recorded. It is true regardless of whether you center, de-center, or average the data each and every way you read it the conclusion is the same.

False logic will not bring us closer to an understanding of the scientific truth, so let's stop politicizing science. Rather, let's show our commitment to our environment which we have a moral and ethical obligation to protect. I hope that today we will take steps in that right direction but I fear we will not. I yield back, Mr. Chairman.

MR. WHITFIELD. Thank you, Ms. Baldwin. I recognize Mr. Stearns of Florida.

MR. STEARNS. Thank you, Mr. Chairman, and I thank you for having this hearing. Listening to the folks on the other side, I would say to my colleague from California asking why aren't we spending our time developing legislation, I would say it is probably incumbent upon us as Chairman Barton pointed out to find out if the facts are correct. We have from the last hearing some inquiry that shows there potentially exists some dubious research particularly embodied on the hockey stick effect that shows a huge global warning in our period.

Now if you look at the data and you go to the recent release from the National Research Council, Thursday, June 22, 2006, it shows that from the period 1400 A.D. to 1900 A.D. were in a little Ice Age, but when you go back further back to 1000 A.D. to 1400 A.D. we were in a warm period, so is it possible that what we are seeing here is sinusoidal and that perhaps we should inquire if this hockey stick graph is the basis for this alarm that we should start developing legislation immediately. Obviously, it is the centerpiece of movies. It is the centerpiece of documents that have been popular, but what it shows is that the temperatures were stable in other parts of our period and were much higher in the medieval and obviously there was not the human population, there was not the gasoline that supposedly is driving this warming period now.

So I think we owe it to our constituents. We owe it to all the Americans to find out if the policy decision for this hockey stick is accurate so I think what we are doing today, Mr. Chairman, is just simply trying to develop an accurate understanding of what is out there. Now we had the hearing last week and we heard from Dr. Wegman. This report provided an independent critique of the statistical method of Dr. Mann, which shows that his information basically produced the hockey stick. Dr. Mann asserted that the increase in the temperatures in the Northern Hemisphere in the 20th Century is likely to have been the largest of any century during the past 1,000 years.

The report also found that 1990 was the warmest decade and 1998 the warmest year of the millennium. Dr. Mann's claims are repeated so often they are now considered facts, but as often the case with statistics, a deeper look at some of these claims show that perhaps there is more than meets the eye. Dr. Wegman's final report found that Dr. Mann misused certain statistical methods in his studies which inappropriately selected hockey stick shapes in the temperature history. Dr. Wegman concludes that Dr. Mann's work cannot support the claim that the 1990s were the warmest decade of the millennium.

Specifically, Dr. Wegman found that the temperature proxies used by Dr. Mann are incorrectly centered on the mean of the period 1902 to 1995 rather than on the whole time period. Because the hockey stick proxies are centered too low, they will exhibit a larger affected variance allowing the graph to exhibit a much more dramatic jump in average temperature. The net effect of de-centering in Dr. Mann's study is to produce this hockey stick shape. Centering on the overall mean is a critical factor in using the principal component of methodology properly according to Dr. Wegman.

So that is sort of in a nutshell what we have here so by golly, I think it is worthwhile to have a second hearing on this, Mr. Chairman, and to try to understand what is happening here, and at the same time not be overly critical of anybody because in the end all we want is the truth, and to understand if we are in an emergency situation or basically we are in a period where there are highs and lows in this earth temperature. And, in fact, in the report that has come out with the working group of the Intergovernmental Panel on Climate Change, which was used in many reports, it shows the last 140 years the temperature of the Earth has gone up 1.5 degrees Fahrenheit, so that is 140 years. Now that could be coming off a cold cycle which means 1.5 degrees Fahrenheit is even more negligible.

So the question of global warming is something we should look at, and I think before we pass legislation or as Ms. Baldwin talked about this chattering irrelevance, we should find out what is relevant to the studies and if they make sense before we pass legislation. I yield back, Mr. Chairman.

MR. WHITFIELD. Thank you. The chair recognizes Ms. Schakowsky of Illinois.

MS. SCHAKOWSKY. Thank you, Mr. Chairman. I find this hearing really depressing among other things. There is a sense that somehow there is a pretense that what we are engaged in here is some sort of scientific-like inquiry but the fact of the matter is that the scientific community has reached consensus. You can say anything you want at this hearing but that is simply the truth. I want to read something from Al Gore's book but lest you think it is Al Gore's words it is a statement of 48 Nobel Prize winning scientists. It says, "By ignoring scientific consensus on critical issues such as global climate change President Bush and his Administration are threatening the Earth's future."

I am not so upset about a waste of time. We do plenty of that around here. But I am depressed about it because that is what is at stake here, the time that we are spending here. I also just want to say since we are getting into this petty he said, you said, back and forth, the charts that Al Gore said--he talked about this teacher of his, Dr. Roger Ravelle. It was his chart that he first presented. When he showed a chart that looks very similar to our hockey stick it was Dr. Lonnie Thompson's study that he was talking about. These have been repeated over and over again. How many times, 928 peer reviewed articles dealing with climate change published in scientific journals during the previous 10 years, percentage in doubt as to the cause of global warming, 0 percent.

The answer is in. And so it seems to me unless somewhere there, and Dr. Wegman already has told us over and over again, he is nearly a paleoclimatologist, he is not a climate scientist of any sort, unless someone can tell us that the planet is not warming and that it is not that the warming, I am sure no one would do that, that the warming is not at least in part attributable to human activity then what we should be talking about is what we are going to do to address the problem. What do we know? We know Greenland is melting. We know that some of our districts could be under water. We know that human life as we know it could be unsustainable in many ways on this planet. Drought, more severe storms, flooding, all the things, not to mention for my littler grandchildren that polar bears are drowning and different species of trees aren't going to be there.

Look in magazines, the old National Geographics, to look at the changing of the trees in the north. This is happening. So why we would be spending our time in what may be--fight about it. Fine. Let the scientific community do whatever it wants, Dr. Mann and his old study, and let Dr. Mann defend himself, but what we should be sitting down and doing, what are those strategies that we can employ to decrease the effects of global warming so that life as we know--so what if it is normal? So what if--but if human activity is contributing to a greater than normal warming or even an upswing right now and the life that we have established on this planet is in danger then we ought to be thinking about the ways that we address this problem.

And Mr. Waxman talked about the tobacco companies. Well, we have now here on July 27, 2006, ABC News--ever wonder why so many people still seem confused about global warming? This is a quote from the--the answer appears to be that confusion leads to profit especially if you are in some parts of the energy business. One Colorado electric cooperative has openly admitted that it has paid $100,000 to a university academic who prides himself on being a global warming skeptic. Intermountain Rural Electric Association is heavily invested in power plants that burn coal, one of the chief sources of greenhouse gases that scientists agree is quickly pushing Earth's average temperature to dangerous levels.

Scientists and consumer advocates say the co-op is trying to confuse its clients about the virtually total scientific consensus on the causes of global warming. Now virtually totally scientific consensus. Well, maybe we can find one or two more. Maybe we could have a dozen more hearings of individuals who want to come in and challenge what is the scientific consensus. But I am depressed about it and I am worried about it because time is wasting for us to do something constructive about this. I thank you, Mr. Chairman.

MR. WHITFIELD. Thank you. Mr. Bass of New Hampshire.

MR. BASS. Thank you, Mr. Chairman. I wasn't planning to give an opening statement but I am kind of warming up to it here. I have been somewhat amused listening to these opening statements going back and forth like a ping pong ball across the table, and I just have to observe that we could sort of divide this debate into four different categories. We have the don't worry, be happy crowd. We have the crowd that believes that the world is warming but because we can't agree on what to do, we might as well let the good times roll while we can. Then there is the for want of a better word the political crowd that maintains that this whole issue is the fault of George Bush, Halliburton, the tobacco companies, tax cuts, and failure to raise the minimum wage.

Frankly, Mr. Chairman, I think these hearings have been constructive. I think they have been logical. I think Dr. Wegman's testimony's last week was dispassionate, scientific, interesting. I think it is great that we have Dr. Mann here today to present another point of view. I happen to believe personally that there is a problem of global warming in the world and there is a pretty good possibility that that may have been caused by the excessive growth of the use of hydrocarbons over the last century. I don't blame Republicans or Democrats or tobacco companies or any other entity for it. I think it is an issue that we need to address, and we need to address it in a logical fashion, and this is the beginning of that process.

Now I think if I were a member of the general public I would want to have a few questions answered ultimately as a result of this debate. Number one, is there a warming trend going on? Number two, is it caused by natural sources or by man? Are the oceans getting warmer? Are hurricanes getting stronger? Is global warming the reason why hurricanes might be getting stronger? The oceans are a CO_2 sink. Is global warming affecting the ability of the oceans to absorb CO_2 and so forth? I think that is the logical progression that a hearing such as the one that we had last week and what we have today leads to--we don't need to have a hearing that deals with the conclusions before we build the evidence.

So I want to thank you, Mr. Chairman, for this hearing and as one who supports the concept that we need to address this problem I think we are moving in the right direction. I yield back.

MR. WHITFIELD. Thank you. At this time I recognize Mr. Inslee of Washington.

MR. INSLEE. This really is pathetically unworthy of America, the most technologically oriented society in human history, and we are here debating the equivalent of gravity. Literally while America literally burns we fiddle. This hearing makes Nero look like a responsible

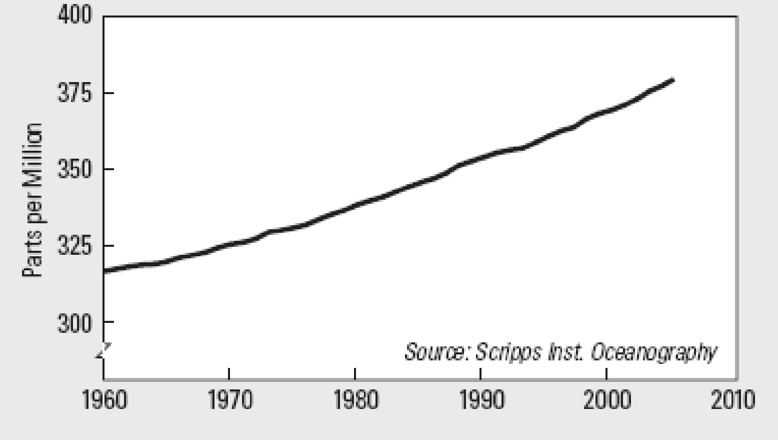
Roman citizen. And we have got to pull our heads out of the sand in that regard. Now the reason is--and I am not depressed like Ms. Schakowsky. I am enraged. Since the last hearing if anybody bothers to read the newspapers an article comes out showing that 80 percent of the mass of the glaciers in my state in North Cascades National Park, one of the jewels in the crown, melted. A study comes out yesterday. Highest heat loss, 50 deaths in California, and we are sitting here fiddling around.

Article comes out yesterday. We have a dead zone in the North Pacific where fish are dying because of change in the circulation patterns in our oceans. And we sit here and fiddle around. This would be the equivalent after the Titanic of the oversight committee having a hearing on how they arranged the deck chairs back in that good old day. Now why is this so ridiculous? It is ridiculous because there is total scientific consensus not only in American but in the world that we are responsible in part for the change in the climatic systems of the globe. I would refer to a science article that studied 928 peer reviewed articles and not a single one of those peer review articles said anything that most of the folks on the Majority side want them to say.

They all said that every single association in the world that has studied this have concluded the climate is changing and humans are partially responsible. That includes the American Meteorological Society, the American Geo-Physical Unit, the Advancement of Science Association, the American Academy of Sciences, and the International Panel of Climate Change. And you know what they got here? They got nothing. They got nothing to say that those things are not true. We are sitting here trying to poke holes in an 8-year old study. You know what it is like to me? It is like at the soccer final championship, and you saw the head butt by Zidane. He head butted, and everybody says he butted him. And they would argue but there was a guy up there in section 23B and it didn’t look like a head butt to him, and maybe his eyesight was a little bad.

The world knows what is going on here, and it is a sham. I want to refer to some of the science of this. They know it is a sham if you look at this graph up here. Atmospheric concentrations of carbon dioxide are going up unassailable.

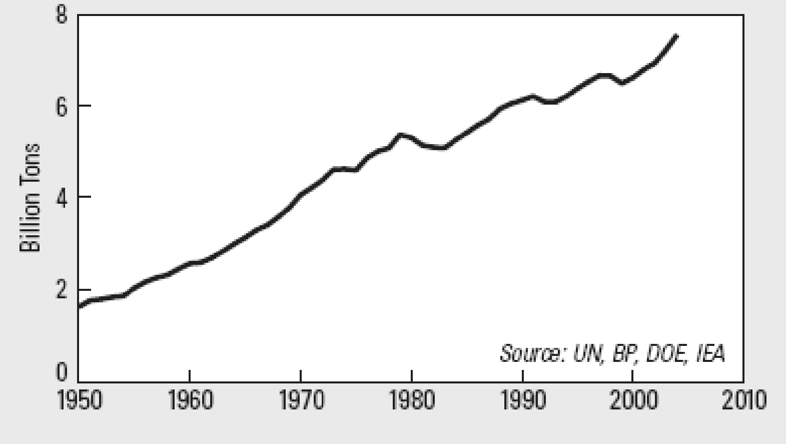
Figure 2. Atmospheric Concentrations of Carbon Dioxide, 1960–2005



Source: Scripps Inst. Oceanography

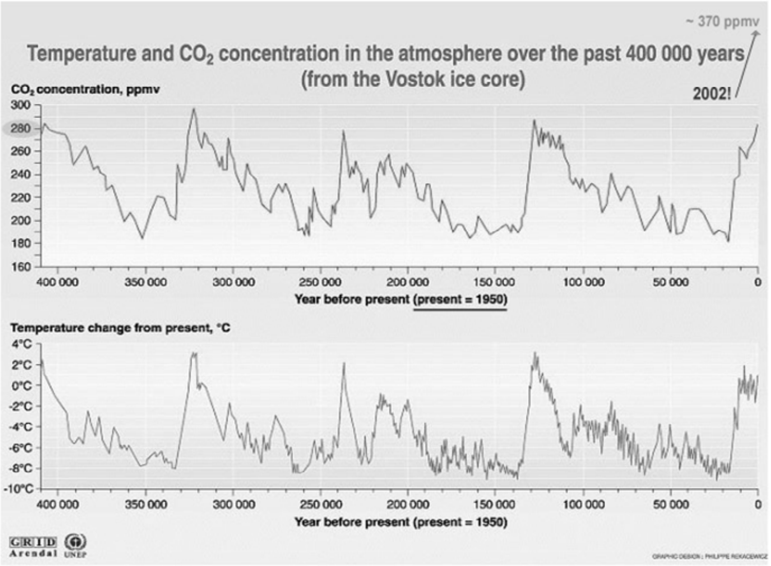
Next slide, please.

Figure 3. Carbon Emissions from Fossil Fuel Burning, 1950–2004



Source: UN, BP, DOE, IEA

Our contributions are going up from fossil fuel burning. No question about that. Next slide, please.

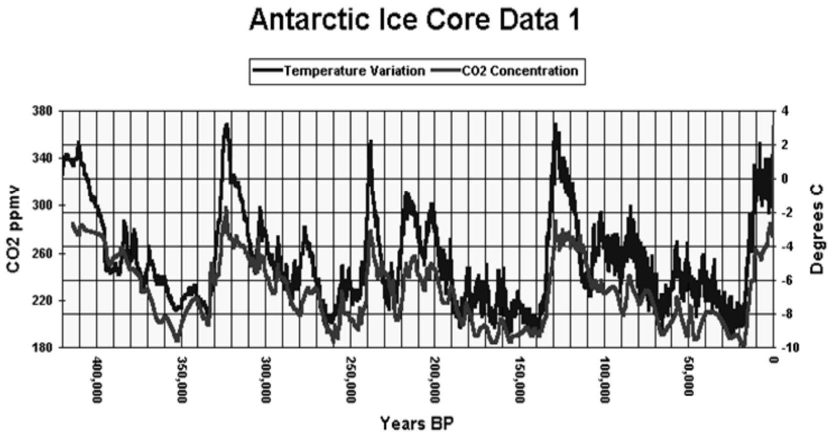


Source: J.R. Petit, J. Jouzel, et al. Climate and atmospheric history of the past 420 000 years from the Vostok ice core in Antarctica, Nature 399 (3JUne), pp 429-436, 1999.

(Note: 2002 information added to diagram)

We see the contributions, the CO_2 levels globally over the last 400,000 years on the top and the temperature at the bottom. What you see is that they are very, very closely related. It is an amazing relationship. And what you will see at the top if I can get a laser pointer to show right up here were 370 PPM. That is higher than any time in the last 400,000 years, and what is scary is it is going through the roof. It will be double preindustrial times in my lifetime and my children's lifetime.

None of this is arguable. All of this is known. And we are going to hear discussion today that we have ice core data that I will talk about that is independent of Dr. Mann's research. We have physical evidence of changes of oxygen isotopes that prove what is going on, which is we are changing the climate of the United States and the world. Next slide, please.

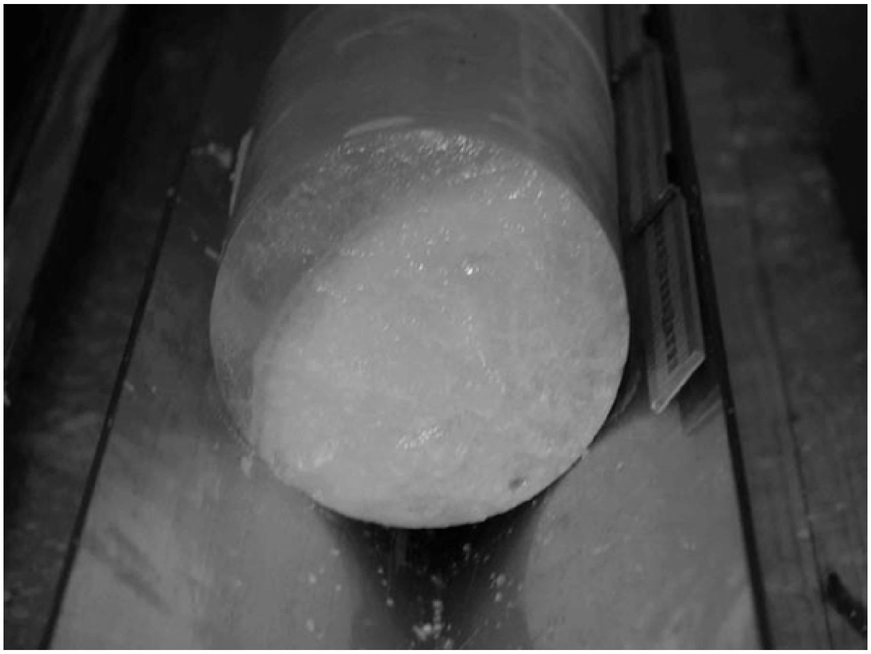


Antarctic Ice Core Data 1

I just want to show you, this is Antarctic ice core data. The blue showing, if I can get my facts straight here, the blue showing temperature, the red showing CO_2 variations. The relationship is incredibly similar.

And again if you look where we are going to be during my lifetime and my children's lifetime, we will be right here. We will be almost off the charts, and we will be double what we were in preindustrial times. I challenge anyone here at this table, and I got an outstanding question for all of you in this hearing, you tell me if you double CO_2 levels for preindustrial levels if you think that is a good idea for America. I want all of you tell me if you think that is a good idea. I think it is a really bad idea. We ought to start being more the American eagle and less the ostrich and we ought to fly with new technology instead of putting our head under the sand on this issue and then this commerce committee will start helping America.

Next slide, please, if I can just show you one more thing.



This is a picture of ice core. We are sitting here talking about some paleoclimatic proxy data, and we are going to spend hours talking about it, but the fingerprints, the DNA evidence, is in the air in that core picture I am showing you because it is 400,000 year old air. We can directly measure the oxygen isotopes that is a direct measurement of the temperature. We know what is going on and it is not a pretty picture. And I look forward to the day that we start doing something about this instead of just having these ridiculous examples and arguing gravity. Thank you, Mr. Chairman.

One more comment too before I leave just briefly. I noted Mr. Barton, my good friend, I congratulate him on the baseball game this year, they whooped us again, and I notice he hadn't seen this movie about climate change. I am going to invite Mr. Barton to go see this movie with me. I am going to buy him as much popcorn as he wants, and I am going to agree to go to any movie he wants to go to from Zorba the Greek to Lawrence of Arabia, anything he wants me to see. I think it would be good for both of us. Thanks very much.

MR. WHITFIELD. Thank you very much. You can see we are a very social group.

CHAIRMAN BARTON. If I go he is going to have to pay.

MR. WHITFIELD. Obviously this is a subject that people feel very strongly about, and we are delighted with our witnesses on the first panel today. Now it is your turn to talk, and we appreciate you being so patient while we talked. Our first witness, and I will introduce all of you, Dr. Michael Mann who is the Associate Professor and Director of the Earth System Science Center at Penn State University, University Park, Pennsylvania; Dr. John Christy, Professor and Director of Earth System Science Center, University of Alabama in Huntsville; Dr. Ralph Cicerone, President of the National Academy of Sciences; Mr. Stephen McIntyre of Toronto, Ontario, Canada; Dr. Jay Gulledge, Senior Research Fellow, Pew Center on Global Climate Change; and Dr. Edward Wegman, Director, Center for Computational Statistics at George Mason University.

We welcome all of you. As you know, this is an Oversight and Investigations hearing, and we do take our testimony under oath, and I would ask any of you do you have any objection to testifying under oath? Under the rules of the House and rules of the committee you also are entitled to legal counsel. I am assuming that none of you have legal counsel with you today, but do any of you have legal counsel today? Okay. Then if you would stand and raise your right hand, I would like to swear you in.

[Witnesses sworn.]

MR. WHITFIELD. Thank you very much. All of you are now under oath. And, Dr. Mann, we will recognize you for 5 minutes for your opening statement.

TESTIMONY OF DR. MICHAEL E. MANN, ASSOCIATE PROFESSOR AND DIRECTOR, EARTH SYSTEM SCIENCE CENTER, THE PENNSYLVANIA STATE UNIVERSITY; DR. JOHN R. CHRISTY, PROFESSOR AND DIRECTOR, EARTH SYSTEM SCIENCE CENTER, NSSTC, UNIVERSITY OF ALABAMA IN HUNTSVILLE; DR. RALPH J. CICERONE, PRESIDENT, NATIONAL ACADEMY OF SCIENCES; MR. STEPHEN MCINTYRE, TORONTO, ONTARIO, CANADA; DR. JAY GULLEDGE, SENIOR RESEARCH FELLOW, PEW CENTER ON GLOBAL CLIMATE CHANGE; AND DR. EDWARD J. WEGMAN, DIRECTOR, CENTER FOR COMPUTATIONAL STATISTICS, GEORGE MASON UNIVERSITY

DR. MANN. Mr. Chairman, members of the committee, thank you for inviting me here to appear before you today. I became a climate scientist because the Earth's climate is a fascinating and complex system and understanding how it works is so important. Part of my research has involved examining preindustrial climate history in order to learn about the natural variations in the Earth's climate. My research in this field, not just the initial work that my colleagues and I published in the late 1990s, but my recent research as well suggests late 20th Century Northern Hemisphere average temperatures are unprecedented over at least the past 1,000 years.

Of course, we have accurate thermometer measurements only back about 100 years, and so we estimate climate prior to that period from indirect sources called climate proxies such as tree rings, corals, and ice cores. This work involves many uncertainties and there are numerous judgment calls that must be made. For that reason we are rarely categorical in the conclusions that we reach. What is important, however, is that the scientific community has reached consensus that recent northern hemispheric average warmth appears to be unprecedented over at least the past 1,000 years, and that this warmth can only be explained by anthropogenic or human influences on the climate.

This conclusion is not based on single studies or isolated research but is confirmed by many studies using different sets of data and independent statistical methods and indeed this conclusion was just echoed weeks ago by a report of the National Academy of Sciences, the most prestigious nonpartisan scientific body in the Nation. So where does my research fit into this? Taken as a whole my own research is in accord with the scientific mainstream reflected in the National Academy report and elsewhere that there has been unprecedented warming in the Northern Hemisphere over the past 100 years.

Exhibit A, if you can take a look at Exhibit A there, that shows that this conclusion is common to a number of similar studies including two I was involved with. This committee is not looking at my work on the whole or on the larger body of science on this issue. It is instead focusing on the first study of this type my colleagues and I published and undertook in 1996 while I was still a graduate student. While there were previous reconstructions based on proxy data our study was the first to estimate global patterns of past temperature change and the first to estimate uncertainties. Our initial study published in the journal, *Nature*, in 1998 was followed by an additional study in the journal, *Geophysical Research Letters*, in 1999. The main conclusion of the 1998 study was that there had been unprecedented warming in the Northern Hemisphere in recent decades. The 1999 study reinforced this conclusion but also reassessed and expanded the uncertainties and added the tentative conclusion that it was likely that the 1990s were the warmest decade over that thousand year time period and that 1998 was the warmest year.

The 1999 study included a graphic depiction of the temperature history over the last millennium, which demonstrated an unprecedented rise during the 20th Century. Some have dubbed this graphic the hockey stick. If the question this committee seeks to answer is whether knowing what I know today, a decade after starting the original study, my colleagues and I would conduct it in exactly the same way, the answer is plainly no. The field of paleoclimate reconstruction has evolved tremendously over the past decade.

Important new proxy data have been developed. Reconstructions have been compared with independent estimates from climate model simulations and confirmed by those simulations. Statistical methods for reconstructing climate from proxy data have been refined and rigorously tested, and I have been actively working in each of these areas. This is important because all the focus of criticism on our work in the late 1990s has been on the statistical conventions we used. My co-authors and I have not used those conventions in our later work.

The critique goes only to our first reconstruction effort. It does not apply to our more recent studies all of which indicate the same basic hockey stick result. Exhibit B demonstrates this point. The green reconstruction does not use principal component analysis at all so the statistical conventions being discussed here have no relevance, and it is the same basic reconstruction, if you will, essentially the same "hockey stick." Now our critics do not confront the fact that our basic conclusion is not an isolated or aberrational finding reached only in one study. Every climate scientist who has performed a detailed reconstruction of the climate of the past 1,000 years using different proxy data and different statistical methods has come up with the same basic hockey stick pattern, that is to say a reconstruction that agrees with our original reconstruction within its estimates uncertainties.

My critics also fail to recognize that even if their criticisms are accepted it has no bearing on the outcome. Dr. Wegman's report argues that the hockey stick pattern derives from the statistical conventions used in our 1998 and 1999 studies. However, using alternative statistical conventions yields the same hockey stick pattern. The hockey stick pattern is intrinsic to the data. That was the conclusion of the National Academy. Page 116 of the National Academy report says the statistical convention my colleagues and I used "does not appear to unduly influence reconstructions of hemispheric mean temperature; reconstructions performed without using principal component analysis are qualitatively similar to the original curves presented by Mann et al."

This was also the conclusion reached by Dr. Hans von Storch who testified here last week, and by four independent teams of scientists who published peer reviewed articles considering and rejecting the conclusion that the statistical methods used in our early studies were responsible for the hockey stick result. Finally, my critics ignore the fact that other scientists have repeated original results using the centered PCA analysis that Dr. Wegman favors and have concluded that the result is basically the same as we originally reported. This is summarized in Exhibit C.

So even if one accepts as valid the criticisms about the statistical conventions used in our early work our results are essentially unaffected. As you can see, the two curves are barely distinguishable within the width of the lines that are shown. And as I have said before our key conclusion that recent hemispheric warmth appears unprecedented over at least the past millennium has been confirmed by every study that has examined the same question.

Finally, it is worth expressing again that paleoclimate reconstructions represent just one of many independent lines of evidence that support the conclusion that human activity is already having a substantial impact on global climate. I appreciate this opportunity to answer the committee's questions. I am sorry I could not be here last week but as I had explained to committee staff, I had to take care of my infant daughter while my wife was attending a conference.

[The prepared statement of Dr. Michael E. Mann follows:]

PREPARED STATEMENT OF DR. MICHAEL E. MANN, ASSOCIATE PROFESSOR AND DIRECTOR, EARTH SYSTEM SCIENCE CENTER, THE PENNSYLVANIA STATE UNIVERSITY

Mr. Chairman, and members of the Committee. My name is Michael Mann. I am an associate professor in the Departments of Meteorology and Geosciences at Penn State University, and Director of the Penn State Earth System Science Center. My research involves the use of climate models, the analysis of empirical climate data, and developing methods for comparing observations and model predictions. The primary focus of my research is understanding the long-term behavior of the climate system, including key climate processes such as the El Nino/Southern, and determining the roles of various potential agents of climate change, both natural and human.

I have served as organizing committee chair for the National Academy of Sciences *Frontiers of Science*, and as the co-author or advisor for several National Academy of Sciences panels related to climate change. I served as a member of the *Committee on Probability & Statistics* of the American Meteorological Society for three years. I have also served as editor for the *Journal of Climate* of the American Meteorological Society and have served as a member of numerous other international and U.S. scientific working groups, panels and steering committees. I have co-authored more than 70 peer-reviewed articles and more than 30 other peer-reviewed contributions and book chapters on climatology and paleoclimatology.

In my testimony here today, I would like to emphasize the following key points:

1) Numerous independent studies using different data and different statistical methods have re-affirmed the most important conclusions of the work my colleagues and I began more than a decade ago. All published studies show that late 20th century average Northern Hemisphere warmth appears to be unprecedented over at least the past 1000 years. Several studies now suggest this holds over an even longer timeframe.

2) Our main conclusions have recently been endorsed by an expert non-partisan report issued by the U.S. National Academy of Sciences (NAS) just weeks ago. The NAS endorsed our conclusion that the late 20th century Northern Hemisphere average warmth was likely anomalous in the context of at least the past 1000 years. "Likely" means having a slightly better than even probability --- i.e., a probability of roughly two-thirds. (See 2001 IPCC Report). In their press conference, the authors of the NAS report stated that they too believe a roughly two-thirds probability can be attached to this conclusion. The NAS report also noted that our conclusions are supported by multiple independent studies and independent lines of evidence.

3) The precise details of our early work have been independently reproduced and confirmed by climate scientists Dr. Eugene Wahl and Dr. Caspar Ammann based on the data used in our study and the algorithm descriptions that have been available in the public domain for years. This work also confirms that my co-authors and I fully adhered to scientific standards by making our data available to other researchers.

4) Climate scientists are not a close-knit "social" group that engages in group think. Hundreds of scientists work in this field and we are a competitive bunch. We compete for scarce research dollars, academic recognition, and professional standing. Every scientific publication that my colleague or I have published has been subject to rigorous and independent peer review. Peer review in my field is anonymous. Authors play no role in selecting peer reviewers. And it is quite possible --- indeed likely --- that a journal will select someone who has expressed skepticism in one's work as a peer reviewer.

5) The evidence for human-induced climate change does not rest solely or primarily on paleoclimatic evidence generally, or on my work in particular.

In order to understand the work that my colleagues and I have performed, it is important to locate it within the larger body of climate studies. Prior to our work in the late 1990s, few scientists had attempted to undertake a detailed spatial reconstruction of the climate during prior centuries, and associated errors were basically unquantified. For that reason, scientists had to rely on cartoon-like schematics, much like the one presented in the 1990 IPCC report. By the mid 1990s, researchers had recognized that is was possible to attempt a more rigorous, quantitative reconstruction of past climate using proxy climate data. Our study was just one of several studies in the late 1990s which attempted to produce such a quantitative reconstruction of past large-scale temperature changes. It was considered by many to be the most comprehensive reconstruction because of its emphasis on spatial patterns of past change, and our quantitative estimation of uncertainties. In the words of the NAS, our work was "groundbreaking." As a result, a number of assumptions had to be made with little guidance available from past work. It was for that reason that our 1999 *Geophysical Research Letters* article describing our reconstruction of Northern Hemisphere temperature changes over the past millennium was titled "Northern Hemisphere temperatures during the past millennium: inferences, ***uncertainties, and limitations.***" (Emphasis added). The article also emphasized both the caveats and uncertainties involved, and the tentativeness of our conclusions. Indeed, the points of our study that have engendered the most interest --- that the 1990s were likely the warmest decade ever; and that 1998 was likely the warmest year in the past millennium --- were all presented as tentative conclusions (e.g., "likely" which, by IPCC standards, means having a roughly 2/3 probability of being correct), not as categorical or unequivocal findings.

Our work is hardly out of the mainstream. The paleoclimate reconstruction studies published by my co-authors and I in the late 1990s represented just one of many independent studies which came to similar conclusions. Our studies represented a significant advance in the field at the time primarily because they provided estimates of spatial patterns of temperature change, something that is of equal or greater scientific interest than simply the average temperature of the Northern Hemisphere. Our study was also the first to attempt to estimate uncertainties. It was unfortunate that only our reconstruction was shown in the 2001 IPCC *Summary for Policy Makers* (SPM) which is far more widely read than the Scientific Assessment report. While I was a co-author of chapter 2 of the Scientific Assessment report, I had no authorship or editorial involvement with the SPM. In chapter 2 of the Scientific Assessment report, three different paleoclimate reconstructions were shown. Each of these reconstructions supported the conclusion that late 20th century Northern Hemisphere average warmth was likely unprecedented as far back as reconstructions were performed, which, in two cases, dated back 1000 years.

Since the publication of our original work in the late 1990s and the publication of the 2001 IPCC report, numerous other Northern Hemisphere average temperature reconstructions have been published in peer-reviewed journals. Each of these reconstructions, using different sets of proxy data (in some cases which are entirely independent of the data we used) and different statistical methods, come to the same key conclusion: That late 20th century warmth is likely anomalous in the context of at least the past 1000 years (see exhibit A). In fact, recent studies extend this conclusion to at least the past 2000 years. *See* Moberg A, Sonechkin DM, Holmgren K, Datsenko NM, Karlen W. 2005. Highly variable Northern Hemisphere temperatures reconstructed from low and high-resolution proxy data. *Nature* 433: 613-617.

At the time my collaborators and I published our original studies, there were no existing methods of combining diverse proxy data to reconstruct past spatial temperature patterns. Our results, as discussed further below, have proved robust. In the decade since our original calculations were performed, new proxy climate records have been developed, statistical methods for reconstructing climate from proxy data have been refined, new methods for using synthetic climate proxy data derived from simulations have been tested, and detailed comparisons between proxy reconstructions and independent estimates from theoretical climate models have been conducted. All of this is aimed at better understanding the workings of the climate system.

I have been actively engaged in these research activities. For more than five years, my collaborators and I have been developing and applying alternative methods that represent a significant refinement to our original methods. We have shown that these methods are not subject to the criticisms that have been raised regarding our original work (*see, e.g.,* Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, *Journal of Climate*, 18, 4097-4107, 2005) and yet they yield results essentially indistinguishable from those reported in our original work. *See* Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, *Journal of Climate*, 18, 2308-2329, 2005.

The report by Wegman, et al., looks only at our earliest work. Wegman's report does not look at any of our more recent studies, or the work of dozens of other climate scholars. That omission is striking because these researchers have found the same basic "hockey stick" temperature history. Nonetheless, Wegman's report does not examine whether the use of statistical conventions he suggests undermine our original work have been employed in more recent work by my co-authors and me, or of the other climate scientists engaged in reconstruction work. Whatever merit one assigns to Wegman's criticisms, they go to the first, and necessarily most tentative, of all of the reconstruction data and ignore the enormous amount of diverse and sophisticated work that has followed.

For this reason, the narrow focus of the Wegman report misses the point. The "hockey stick" is not simply an isolated or aberrational finding my co-authors and I reached only once, in one study. On the contrary, it is a finding that every climate scientist who has performed a detailed examination of the available data has reached, because the hockey stick figure is driven by the data. The Wegman report does not acknowledge the key point made in the recent NAS report, namely, that many researchers have come up with the same basic "hockey stick" reconstruction (i.e., a reconstruction that is within the estimated uncertainties of the Mann et al reconstruction) based on the use of different proxy data sets or different statistical methods (see Exhibit A). Nor does the report acknowledge any of the work that my collaborators and I have been engaged in during recent years seeking to refine the original methods we developed a decade ago. These refined methods, unexamined by Wegman, et al., yield essentially the same result as our original methods, although they use different statistical techniques (see Exhibit B).

Moreover, Wegman has not been careful to represent our findings accurately. For instance, Wegman and his co-authors have stated in their report: *"Overall, our committee believes that Mann's assessments that the decade of the 1990s was the hottest decade of* *the millennium and that 1998 was the hottest year of the millennium cannot be supported by his analysis."* We never made the sort of definitive statements attributed to us by Wegman. In the abstract of our 1999 article in *Geophysical Research Letters* we stated that "[w]e focus not just on the reconstructions, but on the uncertainties therein, and important caveats…expanded uncertainties prevent decisive conclusions for the period prior to A.D. 1400." We concluded by stating that, "…more widespread high resolution data are needed before more confident conclusions can be reached…" It is hard to imagine how much more explicit we could have been about the uncertainties and limitations.

Perhaps the most serious omission in the Wegman report, however, is its failure to acknowledge that its central focus — the conventions used for centering in the Principal Components Analysis used to represent certain tree-ring proxy data — has no significant implications on the results of our analysis. The hockey stick pattern derives from the data, not in the PCA. Nonetheless, Wegman's report claims that the PCA centering convention used to represent the North American tree-ring data network in our 1998/1999 studies is responsible for the "hockey stick" shape of our reconstruction. But the report's conclusion does not follow from its premise. Even accepting that certain statistical conventions that were used in our original studies might not be optimal under some circumstances, the use of alternative conventions yields the same "hockey stick" figure. Nowhere does the Wegman report even acknowledge that its assertion that our reconstruction is a function of PCA centering conventions was *rejected* by the NAS. On page 116 of the NAS Report, the NAS notes that the PCA procedure used by Mann et al. "does not appear to unduly influence reconstructions of hemispheric mean temperature;

reconstructions performed without using principal component analysis are qualitatively similar to the original curves presented by Mann et al."

I understand that Dr. Hans Von Storch made precisely this point at the previous hearing. That is not surprising. Even apart from the NAS, the contention that our initial PCA procedure is responsible for the "hockey stick" figure has been considered and rejected by *four independent teams* of scientists, including one led by Dr. Von Storch: (i) Huybers [Huybers, P. (2005), Comment on "Hockey sticks, principal components, and spurious significance" by S. McIntyre and R. McKitrick, *Geophys. Res. Lett.*, *32*, L20705, doi:10.1029/2005GL023395], (ii) Von Storch and Zorita [Von Storch, H. and E. Zorita (2005), Comment on "Hockey sticks, principal components, and spurious significance" by S. McIntyre and R. McKitrick, *Geophys. Res. Lett.*, 32, L20701, doi:10.1029/2005GL022753], (iii) Wahl and Ammann (2006) [Wahl, E.R. and C.M. Ammann (2006), Robustness of the Mann, Bradley, Hughes Reconstruction of Surface Temperatures: Examination of Criticisms Based on the Nature and Processing of Proxy Climate Evidence, *Climatic Change* (in press); see website: http://www.cgd.ucar.edu/ccr/ammann/millennium/refs/WahlAmmann_ClimChange2006.html] and (iv) Rutherford et al (2005) [Rutherford, S., M.E. Mann, T.J. Osborn, R.S. Bradley, K.R. Briffa, M.K. Hughes, and P.D. Jones (2005), Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, *Journal of Climate*, *18*, 2308-2329].

These studies are significant to the Committee's work because each one shows that the PCA convention simply influences the relative ordering of the leading patterns of variance in the proxy data, and does not lead to the "hockey stick" shape of the reconstruction, nor does it have any significant influence at all on the details of the reconstruction. Exhibit "C" taken from Wahl and Ammann (2006) (on which statistical climatologist Dr. Douglas Nychka was consulted) demonstrates what the Mann et al. (1998—"MBH98") reconstruction looks like if the "centered PCA analysis" is used rather than the PCA convention of MBH98 and, if in addition, one of the less well replicated proxy series contested by McIntyre and McKitrick is removed form the data set. The result is basically the same "hockey stick" reconstruction as MBH98. In fact, if one does not use PCA to represent proxy data networks, and uses a completely different method ("RegEM") to relate the MBH98 proxy dataset to modern instrumental patterns, again essentially the same reconstruction is observed (see Rutherford et al, 2005 referred to above). That is because, as I've said, the hockey stick pattern is in the data. Whether PCA is used, or how it is used, does not make any overall difference. PCA was simply used to encapsulate all of the statistically relevant information in the proxy data, and so whatever patterns are in the data will always influence the final reconstruction. Only when statistically significant information is removed from the dataset is a different result obtained.

The reconstruction work by other scholars like Wahl and Ammann also lay to rest any suggestion that my colleagues and I did not fully disclose our underlying data and therefore hindered replication of our work. Attempts by other climate scientists, such as Wahl and Ammann (2006), have successfully reproduced our results based entirely on our publicly available data and algorithmic descriptions. More significant than this, however, is the fact that numerous studies using different proxy data and methods, or using climate model simulations, have given essentially the same result as our original 1990s work.

Paleoclimate reconstructions produced by various groups, as specifically highlighted in the NAS report, are only one of many independent lines of evidence that have led the world's scientific community to reach a consensus that modern global warming is real and is, in large part, attributable to human activity. All of the climate scientists that testified in the earlier hearing agreed on that point. Greater attention needs to be paid to this pressing issue, including estimating the likely impacts of future climate change, and seeking solutions that will allow us to avert its most detrimental effects.

As climatology has become a more inter-disciplinary field the need for better communication between sub-disciplines and between observationalists, modelers and analysts has become clear. Steps that could facilitate this communication include improved funding for the world data centers so that more effort can be made on data recovery and archiving, and developing systems to ensure that data gatherers can be properly credited for data deposited in public archives.

Thank you.

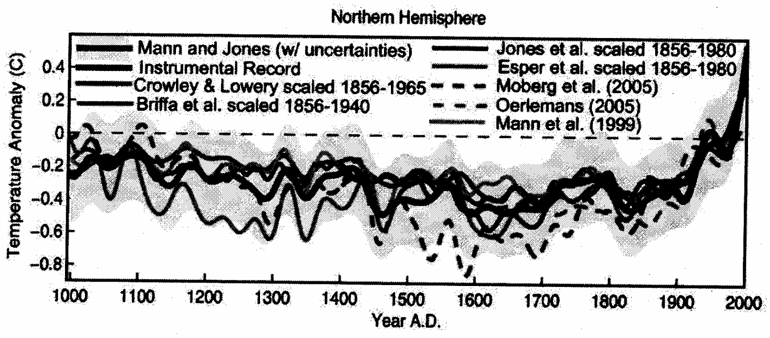


Exhibit A. Comparison of various different reconstruction of Northern Hemisphere average temperatures over the past 1000 years.

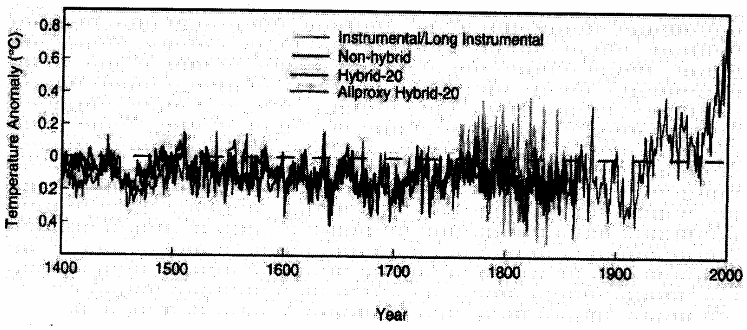


Exhibit B. [from Rutherford et al (2005), Figure 2]. Original MBH98 reconstruction (blue) compared against reconstruction (green) using original MBH98 proxy data set and analternative statistical method which does not represent proxy data with PCA at all.

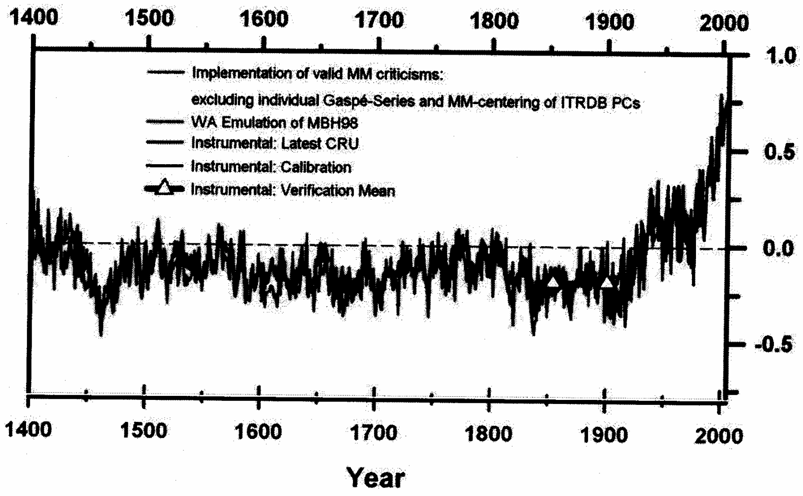


Exhibit C. [from Wahl and Ammann (2006), Figure 5d]. MBH98 reconstruction using centered PC analysis: Red is the original MBH emulation and green is the calculation using centered PC analysis (and additionally removing one of the less well replicated tree ring series).

MR. WHITFIELD. Dr. Mann, thank you. You have heard all the bells going off. We do have a series of four votes on the floor but before we go, Dr. Christy, I am going to ask you to give your opening statement. Then we will recess for probably about 30 minutes and we will come back and take the rest of the testimony.

DR. CHRISTY. Thank you. Chairman Whitfield, Ranking Member Stupak, and committee members, I am John Christy, Director of the Earth System Science Center at the University of Alabama in Huntsville, and the Alabama state climatologist. I served as a lead author of IPCC 2002 chapter on observations with Dr. Mann, and as panelist on the NAS report on temperature reconstructions. As the lead author of the IPCC, I helped craft the now infamous statement about the 1990s and 1998 being the warmest decade and year.

Our confidence was described as likely rather than very likely or virtually certain. In other words, we chose a relatively low level of confidence because of the following concerns known at that time. First, that the hockey stick was new and had not had time for independent analysis for confirmation or revision. Two, a key factor or a key anchor for that early part of the record was a western tree ring series that explained only about 5 percent of the overall variability. And, three, that the unavoidable constraints on the length of the calibration and validation periods really prevented confident knowledge of the relative warmth of different centuries.

A more disappointing aspect of the IPCC regarding temperatures over the last millennium was that some important work was not included, specifically the work of Dahl-Jensen et al., 1998, which I recommended for inclusion many times, was completely missing from this section. These borehole temperatures from Greenland represented probably the most reliable regional temperatures over the last millennium. Thus, in at least one location we had high confidence that it was warmer 1,000 years ago, and though Greenland's temperature may not be tightly connected to hemispheric averages, Greenland is important for sea level averages. If Greenland were warmer in the relatively recent past were its edges also melting as they appear to be now under cooler conditions? I believe the IPCC missed an opportunity to demonstrate climate complexity by excluding this information in 2001.

Dr. Roy Spencer and I created the first satellite-based data set temperature back in 1990. We are now working on improvements to the 8th revision brought about by the divergence of the two most recent satellites.

When asked by others, we provided sections of our code and relevant data files. By sharing this information, we opened ourselves up to exposure or a possible problem which we had somehow missed, and frankly this was not personally easy. On the other hand, if there was a mistake we wanted it fixed. Not knowing the outcome of the work done by scientists at Remote Sensing Systems they asked if they could publish what we had sent them. In my formal scientific response, I wrote, “Oh, what the heck. I think it would be fine to use and critique, that is sort of what science is all about.”

And so it was that in August 2005 RSS published a clear example of an artifact which created errors in the tropics in our data. In *Science* magazine the following November we published the information about our now-corrected temperatures and expressed our gratitude to RSS for discovering our error. While a bit painful, this process as recommended in the National Academy's report, resulted in progress and better scientific information.

Finally, greenhouse gas concentrations are increasing, and therefore the radiation budget of the atmosphere will be altered. In response, the surface temperatures will or should rise. Our observational work, however, has not been able to show clear support for the manner or magnitude of this response as depicted by current climate models. For policy makers this is important. For example, we cannot reliably reproduce or predict the climate for large regions within the United States. It would be a far more difficult task to reliably predict the effects of a policy that reduce greenhouse gas emissions.

Simply put, we cannot say with any confidence to you or to the American taxpayer that by adopting policy X we will cause an impact Y on the weather of the climate system.

What I really find disturbing today is the demonization of energy and its most common byproduct, carbon dioxide, CO_2. I cannot call CO_2 a pollutant when it is a source of life on the planet. CO_2 is plant food. But, as importantly, the extra CO_2 we have put in the air represents astounding improvements in the health, longevity, and quality of human life. I suspect half of us in this hearing room would not be here but for the benefits wrought by affordable energy. Energy use is not evil.

I believe my experience in Africa is important in this whole discussion of energy and climate. In the 1970s I taught science and math as a missionary teacher, and I saw the energy system there. The energy source was wood chopped from the forest. The energy transmission system was the backs of women and girls hauling wood an average of three miles each day. The energy use system was burning the wood in an open fire indoors for heat and light. The consequence of that energy system was deforestation and habitat loss while for people it was poor respiratory and eye health. The U.N. estimates 1.6 million women and children die each year from the effects of this indoor smoke.

Energy demand will grow, as it should, to allow these people to experience the advances in health and quality of life that we enjoy. They are far more vulnerable to the impacts of poverty, water pollution, and political strife than whatever the climate does. I simply close with a plea, please remember the needs and aspirations of the poorest among us when energy policy is made. Thank you very much.

[The prepared statement of Dr. John R. Christy follows:]

PREPARED STATEMENT OF DR. JOHN R. CHRISTY, PROFESSOR AND DIRECTOR, EARTH SYSTEM SCIENCE CENTER, NSSTC, UNIVERSITY OF ALABAMA IN HUNTSVILLE

House Committee on Energy and Commerce
Subcommittee on Oversight and Investigations

27 July 2006
John R. Christy, Ph.D.
University of Alabama in Huntsville

Chairman Whitfield, ranking member Stupak, and committee members, I am John Christy, professor of Atmospheric Science and Director of the Earth System Science Center at the University of Alabama in Huntsville. I am Alabama's State Climatologist. I also served as a Lead Author of the chapter on Observations of the Intergovernmental Panel on Climate Change 2001 Assessment, a Lead Author of the Climate Change Science Program's report on temperature trends and as a Panelist on the National Academy of Sciences report on temperature reconstructions over the past 2000 years.

This written testimony covers a wide range of topics. I will discuss the idea of "consensus" in climate reports and how scientific results may be convoluted by that process. I will examine the issue of sharing computer code and data, and the way it led in our experience to a more reliable dataset. The issue of relative temperatures of the past 1000 years as stated in the IPCC 2001 will be addressed from my perspective as of one of the Lead Authors explaining that we chose words signifying a relatively low level of confidence. I also will note my disappointment with the exclusion of information that pointed to a more complex picture of temperature variability over the last millennium. I touch on the imperceptible climate impacts of energy policy options being considered nowadays and close with some comments about the unfortunate demonization of energy, the resource that has produced uncountable benefits in human health, longevity and freedom from deprivation.

Consensus Reports and Science

In describing the process of generating scientific reports by consensus I was quoted in the *New York Times* as saying it was the worst way to gather scientific information except for all the others.

Consensus at its heart is a political notion. It is a process of selecting words that don't offend the combined sensibilities of a particular set of the authors and reviewers, and is often done grudgingly. It is almost certain that a different set of authors and reviewers would select a different set of words and interpretation even if given the same scientific material.

One example from the first report of the Climate Change Assessment Program's (CCSP) on surface and atmospheric temperature trends comes to mind. This may provide a window into the "science-by-consensus" process. The report's main task was to reach conclusions about temperature trends measured at the surface and those measured in the lower atmosphere. Projections from theoretical climate models indicated atmospheric trends should be warming faster than the surface, especially in the tropics. However, several observational datasets did not support the models, suggesting flaws in the way greenhouse theory was being expressed in those models. Was this discrepancy real?

The original headline was made public in the near-final drafts as, "There is no longer evidence of this discrepancy." This was constructed in a rather busy Chicago meeting in which various authors were working on finalizing their own chapters as well as dealing with this punchline. After sitting with this characterization for a few days I could not agree with its dogmatic tone of finality. The problem was that there was evidence for discrepancies within the report itself.

In terms of strict scientific defensibility the statement should have said, "The magnitude of the global discrepancies in trends is not significant." I made known my view and our lead Editor, Dr. Tom Karl, instigated a special, last minute conference call with the authors to let me make my case. I was basically unsuccessful at persuading the others.

At one point I offered to have a footnote inserted that stated something like, "One author, John Christy, recommends the following version..." I didn't mind being singled out in print as having a different view. That idea was not accepted because, I presume, it violated the notion of consensus. Rather, the punchline statement was massaged a bit to give a little less dogmatism in its meaning to, "This significant discrepancy no longer exists."

The problem still for me is that discrepancies do indeed exist as clearly indicated in the raw numbers provided in the body of the report. However, error margins of the datasets included the *possibility* (not the *proof*) that there were no discrepancies. The difference in meaning of these two statements was apparent to me; rather than promoting a certainty of knowledge as does the first, my proposal acknowledged the uncertainty in our observations, and thus in model evaluation.

This example doesn't cast doubt on the credibility of the body of the report and the considerable information it provides. The many tables and figures display the real currency of science: numbers. The interpretation of those numbers, especially in the high profile Executive Summary, represents the political art of consensus, with the underlying knowledge that from this the headlines burst forth.

I often wonder what conclusions a completely different group of authors would have reached in the Executive Summary given the same scientific information. [That would be a very interesting experiment to perform!] My basic point is that one should recognize that scientific material *and* interpretation of that material are contained in these reports. The interpretation is difficult to test for accidental or even subtle bias. Specific statements may arise from the dogged advocacy of a small group and the fatigue of the remaining writers, but in the end is blanketed by the notion of "consensus." This leaves a murky path of accountability where "all" authors are accountable but at the same time "none" are.

I am risking something here. What future committee would ask me to serve if I might be tempted to later expose some deliberation to the public after all was said and done? I have been careful here to limit this example to one that involved only me, and that was made fully public in the process of final review. But, would the idea of public exposure and potential accountability constrain the typically free-wheeling discussions we as scientists enjoy in trying to reach conclusions? In any case, I hope this example will not threaten future opportunities for me while giving the committee a sense of the limitations of scientific consensus.

Consensus reports are not inerrant, nor infallible. And, as time goes on, new discoveries will demonstrate how science evolves and understanding improves. In the science of climate change we will never have the "Final Answer". I wish every one of these reports began with the line my high school physics teacher drilled into us, "At our present level of ignorance we think we know …"

IPCC 2001, NAS and MBH99 (i.e. the "Hockey Stick")

The National Academy of Sciences (NAS) recently released a report about surface temperature reconstructions of the past 2000 years. Regarding the NAS statement which evaluated MBH99 and how it was expressed in the IPCC 2001, I specifically recused myself from discussing that one paragraph since I was an author of the original IPCC statement. I did not want to be seen as having a conflict of interest and of opening the NAS to such a claim.

As one of the Lead Authors of the Observations chapter in IPCC 2001, I helped craft the now infamous statement regarding the relative warmth of the temperature of the decade of the 1990s and the single year 1998 in the past millennium. We selected the qualifying term "likely" warmest rather than "very likely" or "virtually certain". In other words we chose the term which represented a relatively low level of confidence, being two thirds chance of being correct. "Very likely" meant 90% confidence while "virtually certain" demanded 99% confidence.

Through consensus, and I've indicated the dangers of applying consensus, we settled on "likely", meaning the evidence indicated to several of us that there still remained considerable uncertainty surrounding proxy temperature reconstructions and their errors.

Some IPCC authors were concerned that MBH99 was new and had not had time to be exposed to independent analysis to confirm or revise the result. We also learned at that time that a key anchor for the early part of the record was a western tree ring series that explained only 5% of the overall temperature variability. I was specifically concerned that the unavoidable constraints on the length and certainty of the calibration and validation periods prevented confident assignment of the relative warmth of century-scale temperatures. We eventually chose “likely” based on such concerns. I also remember that we casually discussed the possibility that this figure would become a prominent result of our chapter, but had no idea that it would receive the level of notoriety it eventually did. I think the wide but improper use of the figure promoted an idea that nothing happened for 900 years, then all of the sudden everything happened, giving a false impression of how climate varies over time.

A more disappointing aspect of IPCC 2001 regarding temperatures of the last 2000 years was that some important work was not included. Specifically, the work of Dahl-Jensen 2000 et al., which I recommended to be included on a number of occasions, was completely missing in this section. At that time, this particular analysis of borehole temperature records from Greenland was probably the most confident assessment of relative regional temperature values over the last millennium. Thus, in at least one location of the northern hemisphere we had high confidence that 1000 years ago there was a relatively long period of warmer temperatures than observed in the most recent decade. And, though Greenland’s temperature may not be tightly connected to that of the entire northern hemisphere, Greenland in and of itself is important in dealing with claims of melting ice and sea level rise.

If Greenland were indeed warmer in the relatively recent past, as several proxy records indicated, what was its condition then? Was it melting around the edges in those earlier, warmer centuries as it appears to be melting now in our present cooler temperatures? I believe the IPCC 2001 missed an opportunity to show a more complex picture of climate variability on the planet by excluding this information in 2001.

Sharing data and computer code

Dr. Roy Spencer and I created the first satellite-based temperature dataset for climate studies in 1990. At present we are working on improvements for the 8th adjustment to the dataset brought about by the divergence of the most recent two satellites. Of the 7 previous changes in methodology, two were discovered by other scientists while the other 5 were discovered by us. Satellite instruments and data are complicated and affected by processes which no one really understands completely. Since we cannot go back in time with better instruments, we have to study the ones that were in orbit then and do the best we can to understand how confounding influences affect the measurements.

The computer code we employ consists of 6 complicated programs which at times run sequentially on 3 different machines. The raw datafiles are enormous. When asked, we have shared with others parts of the computer code that were important to understanding how our methodology worked as well as intermediate products which served as a test to check that are methodology was doing what it was intended to do.

When asked, we provided Remote Sensing Systems (RSS) a section of our code which calculated part of the adjustment for the satellites' east-west drift as well as files with the actual values of the adjustment to be sure that our intention in the code and the output matched. They believed our accounting of this particular adjustment was incorrect. Frankly, this was a difficult process from a personal standpoint. By sharing this information, we opened ourselves up to exposure of a possible problem in the code which we had somehow missed. Or worse, a simple disagreement which would lead to arguments about obscure technical aspects of the problem might arise for which there was no simple answer. However, and more importantly, if there was a problem, we certainly wanted to know about it and fix it.

Not knowing the outcome of their work, I received a request from RSS for permission to publish one of the files that we had sent to them. In my formal scientific response I wrote, "Oh what the heck" … " I think it would be fine to use and critique … that's sort of what science is all about."

And so it was that in August 2005 RSS published a clear example of an artifact in our adjustment procedure which created erroneous values in our tropical temperature trend (Mears and Wentz 2005). In *Science* magazine the following November we published information about our now-corrected temperatures and expressed our gratitude to RSS for discovering our error (Christy and Spencer

2005, below). The UAH dataset is better as a result. RSS has also generated a set of satellite temperature products which still differ from ours in some aspects and explanations of those differences are being explored and documented in soon-to-be published material.

The NAS report on temperature reconstructions made the point that when datasets and methods are fully exposed to independent eyes the results will carry more confidence within the scientific community. As best I can tell, this practice was not followed in the MBH99 situation, leading to the conflicts of the past few years.

This brings me back to the CCSP and the evaluation of climate model projections. It was a requirement in the CCSP that all observational datasets used in the report be publicly available in easy-to-access format. Some of us thought the same requirement should be applied to the time series of the global and tropical averages from the climate model simulations, especially since those results had already been published the year before.

In a curious email debate, those who did not want public access given to the climate model averages prevailed. I've encountered this asymmetry before in the field of climate science in which it has typically been very difficult to obtain climate model output in a useful format if at all. Progress has been made with the archiving of the "Climate of the 20th Century" model output at the Dept. of

Energy's Lawrence Livermore Laboratory, but the effort required to retrieve commonly used climate variables is still almost Herculean. Most investigators do not have the infrastructure and personnel to spend time acquiring the huge raw datafiles and then climb a very steep learning curve to process those files into the something useful.

Further, it appears to me that climate model evaluation to this point has been performed mostly by the modelers themselves. It is my view and recommendation that policymakers would learn much from independent, hard-nosed assessments of these model simulations by those who are not directly vested in the outcome. Some of this is going on, but the level of support is minimal.

Science Panel Members – Vested or Non-Vested?

This leads me to another point regarding the CCSP and the NAS reports. In the case of the CCSP report, we as authors were ourselves the builders of the datasets or those who directly performed climate model simulations and evaluations. The process of selecting words to describe the conclusions sprang from those who arguably had strong vested interests. On the other hand, the NAS report of surface temperature reconstructions was written by experts in climate, but who, as we say in Alabama, did not "have a dog in that fight."

After experiencing both situations in the past year, I prefer the approach of the National Academy of Sciences where, in theory, a better chance of producing unbiased and more critical statements is likely.

Global Warming

That greenhouse gases are increasing in concentration is clearly true and therefore the radiation budget of the atmosphere will be altered. In response, the surface temperature should rise due to this additional forcing. In our observational work however, we have not been able to show clear support for the manner or magnitude of this response as has been depicted by the present set of climate models (Christy, 2002, Christy et al. 2006a, Christy and Norris 2006, Christy et al. 2006b).

For policy makers this is an important point, as detailed in my testimony last week (20 July 2006) before the House Committee on Government Reform. We cannot reliably project the trajectory of the climate for large regions within the U.S. for example. It would be a far more difficult task to reliably predict the effects of a policy that altered by a tiny amount the emissions which act to enhance the greenhouse effect. Simply put, we cannot say with any confidence to you or to the American tax payer that by adopting policy X we will cause an impact Y on the weather of the climate system. The basic problem is that if policy X is similar to those being proposed today, the impact on emissions will be essentially imperceptible and thus the attempt to measure or predict its consequence on the climate will be essentially impossible.

To understand the scale of what we are dealing with the following serves as a rough example. We know that we on Earth benefit from 10 terawatts of energy production today. To achieve a reduction of the CO2 representing 10% (1 terawatt) of that production we would need 1,000 nuclear power plants now (1 gigawatt each). Massive implementation of wind and solar does not achieve this result and would not provide the baseload power needed by economies today in any case. (They of course are worthy of investment if costs are reasonable.) Thus, to have a 10% impact on emissions from energy (that is growing at the same time) will require a tremendous and difficult and expensive restructuring of energy supplies.

I believe we will slowly decarbonize energy production and eventually this issue will fade away. But that path of decarbonization should be done with care, being aware of where we are in human economic development as described below.

(However, there are other reasons, such as energy security, which may drive the nation to a different mix of energy sources for which economic outcomes may be more confidently predicted.)

Energy Policy

What I find disturbing in the policy sphere is the demonization of energy and its most common by-product, carbon dioxide (CO2). It is difficult for me to call CO2 a pollutant when as an atmospheric gas it is the source of life on the planet. The long history of CO2 decline over the last millions of years is thought to have been leading to a slow starvation of the biosphere because CO2 is, simply put, plant food.

But, as importantly, the extra CO2 we have put in the atmosphere represents tremendous improvements in health, longevity and quality of human life. I suspect half of us in this Hearing room would not be here but for the benefits wrought by affordable and accessible energy. Energy has delivered to us longer and better lives. Energy use is not evil.

I feel I have some expertise not common to the average scientist that I believe is important in this whole discussion of energy and climate change. In the 1970's I taught science and math in Africa as a missionary teacher. I saw the energy system there. The "energy source" was wood chopped from the forest. The "energy transmission" system was the backs of women and girls, hauling the wood a U.N.-estimated average of 3 miles each day. The "energy use" system was burning the wood in an open fire indoors for heat and light. The consequence of that energy system was deforestation and habitat loss while for people it was poor respiratory and eye health. The U.N. estimates 1.6 million women and children die each year from the effects of this indoor smoke.

Energy demand will grow, as it should, to allow these people to experience the advances in health and quality of life that we in the U.S. enjoy. They are far more vulnerable to the impacts of poverty, water and air pollution, and political strife than whatever the climate does. I simply close with a plea, please remember the needs and aspirations of the poorest among us when energy policy is made.

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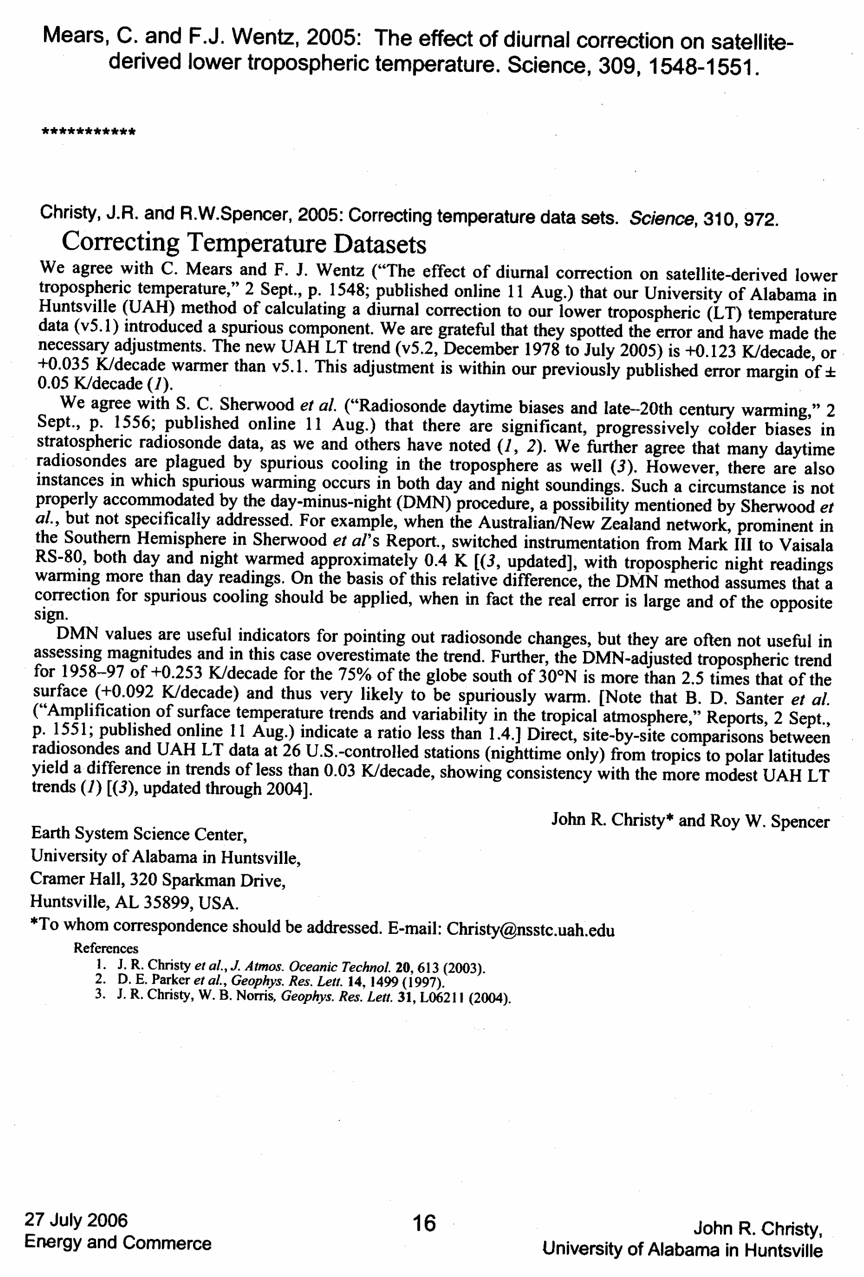

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Correcting Temperature Datasets

We agree with C. Mears and F. J. Wentz ("The effect of diurnal correction on satellite-derived lower tropospheric temperature," 2 Sept., p. 1548; published online 11 Aug.) that our University of Alabama in Huntsville (UAH) method of calculating a diurnal correction to our lower tropospheric (LT) temperature data (v5.1) introduced a spurious component. We are grateful that they spotted the error and have made the necessary adjustments. The new UAH LT trend (v5.2, December 1978 to July 2005) is +0.123 K/decade, or +0.035 K/decade warmer than v5.1. This adjustment is within our previously published error margin of ± 0.05 K/decade (*1*).

We agree with S. C. Sherwood *et al.* ("Radiosonde daytime biases and late–20th century warming," 2 Sept., p. 1556; published online 11 Aug.) that there are significant, progressively colder biases in stratospheric radiosonde data, as we and others have noted (*1*, *2*). We further agree that many daytime radiosondes are plagued by spurious cooling in the troposphere as well (*3*). However, there are also instances in which spurious warming occurs in both day and night soundings. Such a circumstance is not properly accommodated by the day-minus-night (DMN) procedure, a possibility mentioned by Sherwood *et al.*, but not specifically addressed. For example, when the Australian/New Zealand network, prominent in the Southern Hemisphere in Sherwood *et al*'s Report., switched instrumentation from Mark III to Vaisala RS-80, both day and night warmed approximately 0.4 K [(*3*, updated], with tropospheric night readings warming more than day readings. On the basis of this relative difference, the DMN method assumes that a correction for spurious cooling should be applied, when in fact the real error is large and of the opposite sign.

DMN values are useful indicators for pointing out radiosonde changes, but they are often not useful in assessing magnitudes and in this case overestimate the trend. Further, the DMN-adjusted tropospheric trend for 1958–97 of +0.253 K/decade for the 75% of the globe south of 30°N is more than 2.5 times that of the surface (+0.092 K/decade) and thus very likely to be spuriously warm. [Note that B. D. Santer *et al.* ("Amplification of surface temperature trends and variability in the tropical atmosphere," Reports, 2 Sept., p. 1551; published online 11 Aug.) indicate a ratio less than 1.4.] Direct, site-by-site comparisons between radiosondes and UAH LT data at 26 U.S.-controlled stations (nighttime only) from tropics to polar latitudes yield a difference in trends of less than 0.03 K/decade, showing consistency with the more modest UAH LT trends (*1*) [(*3*), updated through 2004].

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MR. WHITFIELD. Dr. Christy, thank you. We are going to go vote. It is now 15 after 3:00 so we will reconvene about 15 till 4:00. Down in the basement there is a little snack center and if you go out the main first floor of the Rayburn Building and walk over to Longworth there is a wonderful ice cream shop so whatever you decide to do.

[Recess.]

MR. WHITFIELD. The hearing will reconvene, and we apologize for the delay. We are about 35 minutes later than we said. But, Dr. Cicerone, you are recognized for your 5-minute opening statement.

DR. CICERONE. Thank you, Chairman Whitfield and members of the committee. My name is Ralph Cicerone. I am President of the National Academy of Sciences and Chairman of the National Research Council. Prior to this year, I was Chancellor of the University of California at Irvine where I was Aldrich professor in Earth System Science and also professor of chemistry. This afternoon I will summarize the state of scientific understanding on climate change very briefly, based on findings and recommendations in NAS and NRC reports and in some recent refereed publications. Our reports, quite often written with the National Academy of Engineering and the Institute of Medicine, go through a peer review process and although we are not part of the Government, we were chartered by Congress and President Lincoln to provide advice on matters of science and technology.

I would like to first start with how is it that humans can influence the climate of an entire planet. The strongest answer is the greenhouse effect which is a natural phenomenon. Without the natural greenhouse effect, the Earth would be much colder than it is right now. We can test that prediction by looking at Mars and Venus, for example. Now humans are amplifying the natural greenhouse effect. Just to give you one major, the extra energy trapped near the earth's surface by a variety of greenhouse gases is about 2-1/2 watts per square meter now, which is about 100 times larger than all the energy usage by humans worldwide on the entire planet from all sources, fossil fuels, nuclear wind, hypothermal, you name it. It is a big number. This is what gives humans leverage to influence an entire planetary climate.

There is no doubt that the Earth is warming. Weather station records and ship-based observations show that the global average surface temperature in the air has increased by about 1.2 degrees Fahrenheit since the beginning of the last century, more than half of the increase since 1975. Scientists have also measured upward temperature trends in the lower atmosphere and in the upper oceans, and this continuing warming has been accompanied by worldwide changes and many other indicators, such as decreases in Arctic sea ice thickness and extent, and shifts in ecosystems.

What is the primary evidence for this widely accepted view that global warming is occurring, that human beings are responsible at least in part for the warming and that the Earth's climate will continue to change during this current century. There are many lines of evidence. Let me summarize them briefly. First, measurements show large increases in carbon dioxide and other greenhouse gases, such as methane and nitrous oxide, beginning in the middle of the 19th Century. These increases in greenhouse gases are due to human activities such as burning fossil fuel for energy, agricultural and industrial processes, and so forth. The concentration of carbon dioxide is now at its highest level shown by actual measurements in the last 650,000 years. The record has been extended back that far now.

Second, we understand how carbon dioxide and other greenhouse gases physically affect global temperature. Rigorous radioactive transfer calculations of the temperature changes due to increasing greenhouse gas concentrations, together with reasonable assumptions about climate feedbacks provide a physically based mathematically sound explanation for the observed warming.

Third, state-of-the-art mathematical climate models are able to reproduce the warming of the past century, but only if human-caused greenhouse gases are included. Fourth, and I did not have this in my written testimony, but simulations of the stratospheric penetrating volcano, Mount Penatubo, in mid-June 1991 were able to show the exact timing of the cooling that took place afterwards based on the sulphate particles and got the magnitude of the cooling almost right. And these were primitive models at the time. Models have improved a great deal since.

Fifth, analysis of high-quality, precise measurements of the sun's total brightness over the past 25 years show little, if any, change in the long-term average of solar output over this time period. Thus, changes in the sun, the best explanation for a natural explanation cannot explain the warming over the past 25 years.

Six, the oceans have warmed in recent decades and the stratosphere has cooled. Land masses north of the tropical region in the Northern Hemisphere have warmed even more than the oceans. All of these large scale changes, their sizes and patterns are consistent with the predicted geographical and temporal pattern of greenhouse surface warming.

Seventh, ice covered regions of the Earth have experienced significant melting. For example, the average annual sea ice extent of the Artic has decreased by about 8 percent or nearly a million square kilometers over the past 30 years. Sea ice thickness measured, for example, by the United States Navy has decreased over the period. Measurements from Earth orbiting satellites from synthetic aperture radars and from Earth's gravity sensors over the last few years have shown that both Greenland and West Antarctic ice sheets are losing ice.

Eighth, several publications in the last 2 years show that hurricane intensities have increased in some parts of the world in lock step with sea surface temperatures.

While we are quite certain that the Earth's surface has heated up during the last 30 years, and that it is hotter now than at any time during the last 400 years, predicting what will happen to important climate variables besides temperature is more difficult.

As we stated in our 2001 report climate change simulations yield a globally averaged surface temperature increase by the end of this century of maybe 2-1/2 to 10 degrees Fahrenheit. As I said, temperature is easier to predict than other changes such as rainfall, storm patterns, and ecosystems, and the prediction of extreme events, which is what probably humans and other biological creatures respond to the most are very difficult.

While these future climate changes and their impacts are inherently uncertain, they are far from unknown. We can paint useful broad brush pictures now of how global warming may affect certain regions of the world. For example, these mathematical models generally project more warming in continental regions than over the oceans and in polar regions rather than near the equator. Precipitation is expected to increase in the tropics, decrease in the subtropics, and increase in the midlatitudes. Rainfall is expected to increase in monsoon regimes. We can give a lot of broad brush predictions like that that are difficult to prove, but that is the state of the science now.

Even if no further increases in the atmospheric concentrations of greenhouse gases occur, which would be a difficult to achieve scenario, we are very likely to experience additional warming of about 7/10ths of a degree Fahrenheit in the coming decades. In colder climates such warming could bring less severe winters and longer growing seasons if soil moisture is adequate. Several studies, quite credible, have projected that summertime ice in the Arctic could disappear in this century, the end of the century.

The combined effects of ice melting and sea water expansion from ocean warming will likely cause the global average sea level to rise by anywhere between 1/10th and 9/10ths of a meter in this century. So coastal communities will experience increased flooding due to seal level rise and are likely to experience more severe storms and storm surges. And of course increased acidification of the surface ocean due to the added carbon dioxide from the atmosphere is occurring. It will continue and it will harm marine organisms such as corals and some plankton species.

In summary, there are multiple lines of evidence supporting the reality of and human roles in global climate change. I think I will stop there to be as brief as possible. With your permission, Mr. Chairman, I have submitted two appendices.

[The prepared statement of Dr. Ralph J. Cicerone follows:]

PREPARED STATEMENT OF DR. RALPH J. CICERONE, PRESIDENT, NATIONAL ACADEMY OF SCIENCES

Good morning, Mr. Chairman and members of the Committee. My name is Ralph Cicerone, and I am President of the National Academy of Sciences. Prior to this position, I served as Chancellor of the University of California at Irvine, where I also held the Daniel G. Aldrich Chair in Earth System Science and was Professor of Chemistry. In addition, in 2001 I chaired the National Research Council committee that wrote the report, *Climate Change Science: An Analysis of Some Key Questions*, at the request of the White House.

This afternoon I will summarize the state of scientific understanding on climate change, based on the findings and recommendations in National Academy of Sciences and National Research Council reports and in recent refereed scientific publications. Although not part of the government, the National Academy of Sciences was chartered in 1863 to advise the government on matters of science and technology. Our reports, often written with the National Academy of Engineering and the Institute of Medicine, are the products of a study process that brings together leading scientists, engineers, public health officials, and other experts to provide consensus, peer-reviewed advice to the nation on scientific and technical questions.

The greenhouse effect is a natural phenomenon. Without greenhouse gases, the surface of the Earth would be about 60°F (33°C) colder than it is today. Now, humans are amplifying the greenhouse effect by increasing the concentrations of many greenhouse gases (carbon dioxide, methane, nitrous oxide, synthetic chlorofluorocarbons and other fluorocarbons, and tropospheric ozone) in the atmosphere. The extra energy trapped near

Earth's surface by the human-amplified greenhouse effect is presently about 2.5 Watts per square meter, which is about 100 times larger than all human energy usage.

There is no doubt that the Earth is warming. Weather-station records and ship-based observations show that global average surface air temperature has increased by about 1.2°F (0.7°C) since the beginning of the 20th century, more than half of it since 1975. Scientists have also measured upward temperature trends in the lower atmosphere and in the upper oceans, and this continuing warming has been accompanied by worldwide changes in many other indicators, such as shifts in ecosystems and decreases in Arctic sea ice thickness and extent.

Last week you heard testimony from Dr. Gerald North, chair of the National Research Council committee that examined surface temperature reconstructions for the last 2,000 years derived from tree rings, boreholes, ice cores, glacier length records, and other types of proxy evidence. The committee concluded that the Earth was warmer during the last few decades of the 20th century than at any other time during at least the last 400 years, and potentially the last several thousand years. These temperature reconstructions provide a useful context for evaluating late 20th century warming. However, they are not the primary evidence for the widely accepted view that global warming is occurring, that human beings are responsible, at least in part, for this warming, and that the Earth's climate will continue to change during the next century.

Many additional lines of evidence demonstrate that climate is changing:

- Measurements show large increases in carbon dioxide and other greenhouse gases (methane and nitrous oxide, for example) beginning in the middle of the 19th century. These increases in greenhouse gases are due to human activities such as burning fossil fuel for energy, industrial processes, and transportation. The concentration of carbon dioxide in the atmosphere is now at its highest level in 650,000 years and continues to rise.

- We understand how carbon dioxide and other greenhouse gases affect global temperature. Rigorous radiative transfer calculations of the temperature changes associated with increasing greenhouse gas concentrations, together with reasonable assumptions about climate feedbacks, provide a physically based theoretical explanation for the observed warming.

- State-of-the-art mathematical climate models are able to reproduce the warming of the past century only if human-caused greenhouse gases are included.

- Analysis of high-quality, precise measurements of the Sun's total brightness over the past 25 years shows that there has been little if any change in the long-term average of solar output over this time period. Thus, changes in the Sun can not explain the warming observed over the past 25 years.

- The oceans have warmed in recent decades and the stratosphere has cooled. Extratropical land masses in the Northern Hemisphere have warmed even more than the oceans. These large-scale changes are consistent with the predicted spatial and temporal pattern of greenhouse surface warming.

- Ice covered regions of the Earth have experienced significant melting. For example, the annual average sea-ice extent in the Artic has decreased by about 8%, or nearly one million square kilometers, over the past 30 years. Measurements from Earth-orbiting satellites (from synthetic aperture radars and from Earth's gravity sensors) over the last few years have shown that both the Greenland and West Antarctic Ice Sheets are losing ice.

- Several publications in 2005 and 2006 show that hurricane intensities have increased in some parts of the world, in lock step with oceanic warming.

While we are quite certain that the Earth's surface has warmed rapidly during the last 30 years and that it is warmer now than at any other time during at least the last 400 years, projecting what will happen to important climate variables in the future is more difficult. As stated in the 2001 NRC report, "climate change simulations… yield a globally averaged surface temperature increase by the end of the century of 2.5 to 10.4°F (1.4 to 5.8°C) relative to 1990." Since 2001, we have continued to make advances in our knowledge of the climate system and in our ability to model it mathematically. Yet, pinpointing the magnitude of future climate changes is hindered both by remaining gaps in our ability to simulate scientific phenomena, and by the fact that it is difficult to predict society's future actions, particularly in the areas of population growth, energy consumption, and energy technologies. In general, temperature is easier to predict than changes such as rainfall, storm patterns, and ecosystems.

While future climate change and its impacts are inherently uncertain, they are far from unknown. A broad-brush picture of how global warming may affect certain regions of the world is starting to emerge from climate modeling efforts. Models generally project more warming in continental regions than over the oceans and in polar regions than near the equator. Precipitation is expected to increase in the tropics, decrease in the subtropics, and increase in the midlatitudes. Rainfall is also expected to increase in the monsoon regimes in South Asia, West Africa, and South America; these changes may create the potential for stronger El Niño events. Some models indicate that midlatitude continents will likely be drier during the summer in a warmer climate, leading to an increased chance for summer drought conditions.

Even if no further increases in the atmospheric concentrations of greenhouse gases occur, we are very likely to experience additional warming of 0.7°F (0.4°C). In colder climates, such warming could bring less severe winters and longer growing seasons (if soil moisture is adequate). Several studies have projected that summertime ice in the Arctic could disappear by A.D. 2100. The combined effects of ice melting and sea water expansion from ocean warming will likely cause the global average sea level to rise by between 0.1 and 0.9 meters between 1990 and 2100. Those in coastal communities, many in developing nations, will experience increased flooding due to sea level rise and are likely to experience more severe storms and surges. Increasing acidification of the surface ocean (due to added carbon dioxide from the atmosphere) will harm marine organisms such as corals and some plankton species.

In summary, there are multiple lines of evidence supporting the reality of and human roles in global climate change. The task of mitigating and preparing for the impacts of climate change will require worldwide collaborative inputs from a wide range of experts, including natural scientists, engineers, social scientists, medical scientists, those in government at all levels, business leaders and economists. For example, researchers and resource managers have only begun to address how climate change will impact future demands for electricity and water. Society faces increasing pressure to decide how best to respond to climate change and associated global changes, and applied research in direct support of decision making is needed.

Appendixes

- Joint science academies' statement: Global response to climate change
- *Understanding and Responding to Climate Change: Highlights of National Academies Reports* (2005)

MR. WHITFIELD. Well, thank you so much, and your entire statement is part of the record, and we appreciate your being here. Mr. McIntyre, you are recognized for 5 minutes.

MR. MCINTYRE. Thank you. Good afternoon, Mr. Chairman, and members of the committee. My name is Stephen McIntyre. I appreciate the invitation to appear before you once again. I will recap my testimony from last week, referring to the NAS and Wegman reports. The Wegman report drew attention to a remarkable lack of independence in data used in supposedly independent studies. Some proxies are used in nearly every such study. This raises the spectre that problems in one proxy can spill over to multiple reconstructions.

One such problem has already been identified. The NAS panel agreed that strip bark bristlecones should be avoided in temperature reconstructions but they did not assess the potential impact of this conclusion. Last week I showed that this recommendation reversed the estimates of medieval modern levels in the Crowley and Lowery reconstruction. Here we show the impact of this on the Mann study, where the conclusion of 20th Century uniqueness does not withstand removing the bristlecones. Every reconstruction using bristlecones will have to be reconsidered in the light of thee NAS recommendation.

By coincidence the key bristlecone sites are located in an area recently studied by Dr. Christy where he recompiled high altitude temperature data. There is actually a slight negative correlation between Christy's temperature data and Mann's key principal component series. You can readily see why the NAS panel said that bristlecones should be avoided as a temperature proxy. Further grounds for concern about the use of this data comes from fossil trees located well above modern tree lines in this area, dated to the Medieval Warm Period. Recent ecological niche studies have concluded that the annual minimum temperatures in this area were 3.2 degrees Centigrade warmer, that is 6 degrees Fahrenheit warmer, than at present.

Dr. Mann likes to say that any problems do not arise simply, and I emphasize simply, from the flawed PC method. If the proxies were ideal, such as the synthetic data studied by von Storch and Zorita, the bad method may not make a difference. But in such circumstances a simple average would also have a hockey stick shape which were not observed in the simple average of the Mann proxies. The real problem, and the one observed by Wegman, is that the PC method as applied to low quality data caused a minor pattern, in this case bristlecones, to be exaggerated as a dominant pattern in worldwide climate.

Notably, Dr. Mann's testimony does not mention bristlecones but in his data, the hockey stick shape is dependent on them. The graph here shows in red the contribution to his reconstruction for bristlecones. The other colors show the contribution from other classes of proxies. As you can see, there is very little information from the other proxies.

Dr. Mann has also said that he can get a hockey stick shape in another way. There are many ways of processing Mann'a data set. Some result in hockey stick shape series, some do not. Burger and Cubasch in 2005 showed a bewildering variety of outcomes based on a slight variation in methodology. Sometimes you are told that scientists have moved on, and that the criticized methods are no longer used. This is not the case. All of Dr. Mann's more recent work used his disputed PC1. Mann's PC1 was used in the prominent article, Osborne and Briffa 2006, and even occurs illustrated as a temperature proxy in one of the NAS illustrations.

An important control on any statistical study is reporting of adverse results. The verification r2 statistic is commonly used in paleoclimate studies and was said in the original article to have been considered. However, early periods of the reconstruction failed the significance test, a fact which was never reported. At the NAS press conference, Dr. Bloomfield said that he found nothing unusual about MBH reporting. If paleoclimate research practices do not require scientists to disclose results adverse to their claims, then this reduces the ability of policy makers to rely on these studies.

Last week I pointed out many problems with data and code access. Much relevant Mann data did not become available until 2004, 6 years after the original study, and then only after a formal complaint to *Nature*. Mann's archiving practices are by no means the worse in the community. Much of Lonnie Thompson's data remains unarchived 20 years after it was collected. The efforts of your committee led to Dr. Mann disclosing a considerable amount of source code. Unfortunately, the source code does not operate with the data as archived and it does not include code for important steps such as the calculation of confidence intervals or PC retention rules.

Wahl and Ammann have been described as independent studies but they are co-authors and collaborators with Dr. Mann and their efforts, whatever their merit, can hardly be described as independent. To the credit of Wahl and Ammann, they have archived their code for their study following a practice that we followed. Their code reconciles to ours and any differences between the studies do not arise from differing arithmetic.

The interest of this committee in reconstruction seems to have been prompted in part when Dr. Mann was quoted by the *Wall Street Journal* as saying that he would "not be intimidated into disclosing his algorithm." Such attitudes are inconsistent with the requirement of policy makers if they are to rely on such studies. If you are to rely on paleoclimate studies you should be concerned about disclosure, data access, and replication because, first, peer review at journals is very limited and does not constitute sufficient due diligence for policy reliance.

Second, IPCC does not carry out independent testing or verification. Third, to enable and facilitate independent testing, paleoclimate research needs to achieve dramatically improved standards for archiving data and code. Fourth, because much of the work is funded by the U.S. Federal government, improved administrative practices by NSF and DOE could make a direct and immediate impact and improvement. Thank you.

[The prepared statement of Stephen McIntyre follows:]

PREPARED STATEMENT OF STEPHEN MCINTYRE, TORONTO, ONTARIO, CANADA

SUMMARY

1. little reliance can be placed on the original MBH reconstruction, various efforts to salvage it or similar multiproxy studies, even ones which do not use Mann's principal components methodology;
2. peer review as practiced by academic journals is insufficient due diligence for policy reliance. IPCC reports are only a literature review rather than independent due diligence.
3. to enable and facilitate independent testing, paleoclimate research practices need to achieve dramatically improved standards for archiving data and code.
4. administrative policies governing work directly funded by the U.S. government can make a direct and immediate difference.

Good morning, Mr Chairman and members of the Committee.

My name is Stephen McIntyre. I appreciate the invitation to appear before you once again. I will recapitulate my testimony from last week, making further reference to the NAS and Wegman reports.

The Wegman report drew attention to a remarkable lack of independence in the proxies used in supposedly "independent" studies. Some sites are used in nearly every study. This raises the spectre that problems with one proxy can spill over to multiple studies. One such situation has already been identified. The NAS panel agreed that strip-bark bristlecones should be "avoided in temperature reconstructions". Last week, we showed that this reversed medieval-modern levels in the Crowley and Lowery 2000 reconstruction. Figure 1 below shows the impact on MBH, where conclusions of 20th century uniqueness do not withstand removing the bristlecones. Wegman showed that bristlecones were used in multiple studies and each one will have to be reconsidered in light of the NAS recommendation.

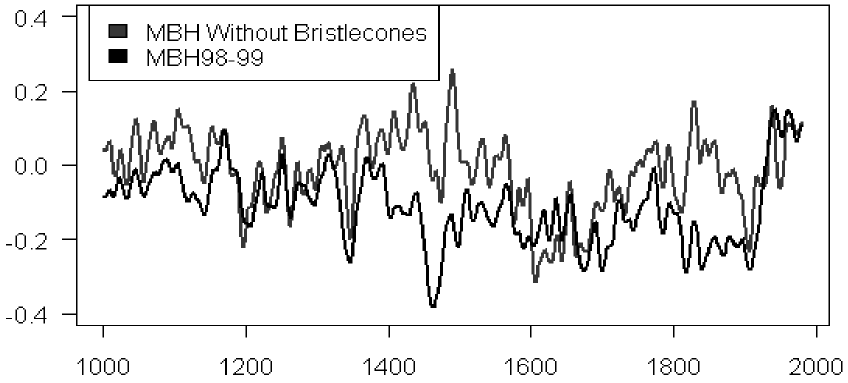


Figure 1. *MBH99 reconstruction and estimate of MBH99-type reconstruction without bristlecones. 20-year gaussian smooth.*

By coincidence, the key bristlecone and foxtail proxies that establish the pattern in Mann's critical PC series are located in almost the exact area studied by Christy, as shown in the location map on the left. As you see, there is little correlation on either a smoothed or unsmoothed basis – actually a slight negative correlation – between temperature and Mann's PC1. You can readily see why the NAS panel said that this data should be avoided as a temperature proxy.

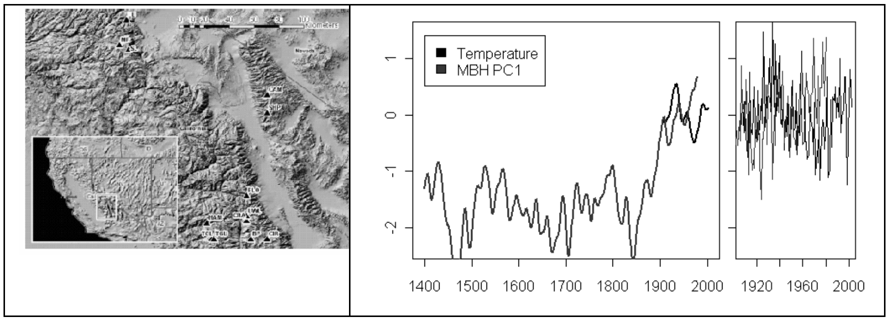


Figure 2. *Left - location of foxtail and bristlecone sites in the Sierra Nevada and White Mountains. Right - Black – annual mean of maximum and minimum temperatures (data, Christy, pers. comm.); red – MBH98 NOAMER PC1.*

Further grounds for concern about using Mann's PC1 as a temperature proxy comes from the evidence of fossil trees well above modern tree lines, dated to the Medieval Warm Period. Millar et al. 2006 concluded that annual minimum temperatures in this area were then significantly warmer (+3.2 °C) than at present.

Figure 3. *A dead trunk above current treeline from a foxtail pine that lived about 1000 years ago near Bighorn Plateau in Sequoia National Park.*

Dr Mann likes to say that any problems do not arise *simply* from the flawed PC method. However, it's not true that the flawed PC method has nothing to do with the problems. A simple average of Mann's proxies does not yield a hockey stick shaped series, as shown in Figure 4 below. If you have proxies of ideal quality, even a bad PC method can yield meaningful results – which is what von Storch and Zorita observed, using idealized data generated in a climate model. However, the problem is that Mann's PC method was applied to low-quality data, where the flawed method caused a minor pattern in bristlecones to be exaggerated as a "dominant pattern" in worldwide climate.

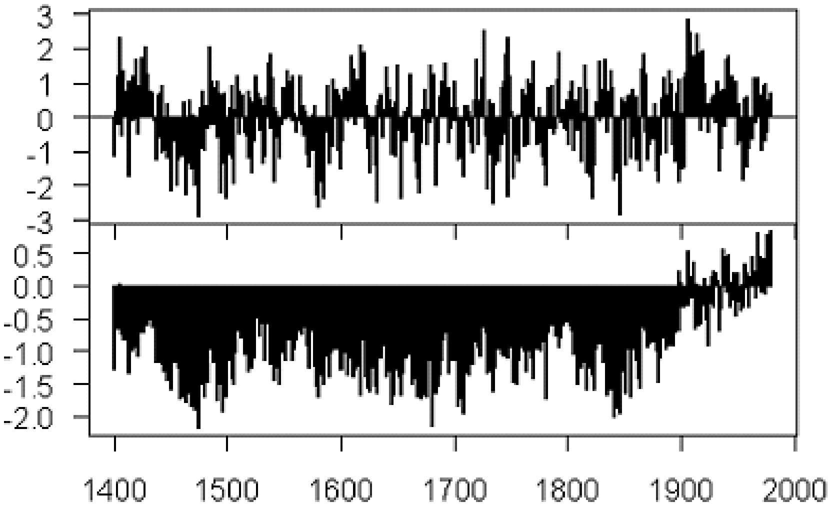


Figure 4. *Left: Top – Average of all 415 MBH proxies; bottom – MBH reconstruction. Both in standard deviation units.*

In the MBH data set, the hockey stick shape is dependent on the bristlecones. All the statistical salvage jobs Dr. Mann cites are variations on schemes to load the final weight on the very data the NAS panel said should not be used.

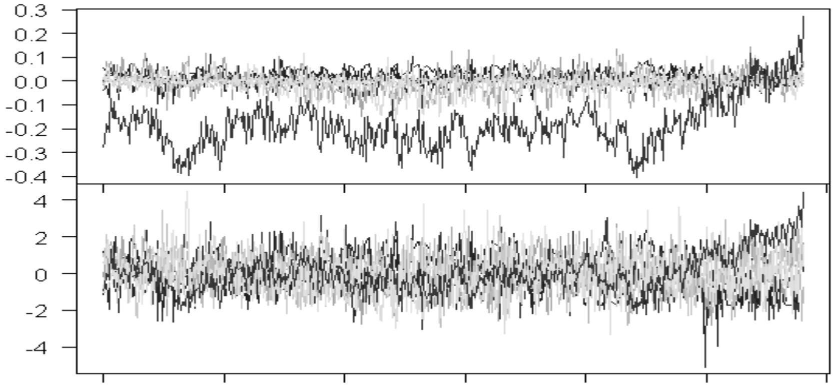


Figure 5. *Top – Contribution (deg C) of proxy groups (proxy type x continent e.g. Asian tree rings; South American ice cores) to the MBH reconstruction, with bristlecones and foxtails in red. Bottom – Same series in standard deviation units. The bristlecone contribution closely matches the final MBH reconstruction.*

There are many ways of processing the MBH data – some result in hockey-stick shaped series; some do not. Bürger and Cubasch 2005 showed a bewildering variety of outcomes based on slight variations in MBH methodology.

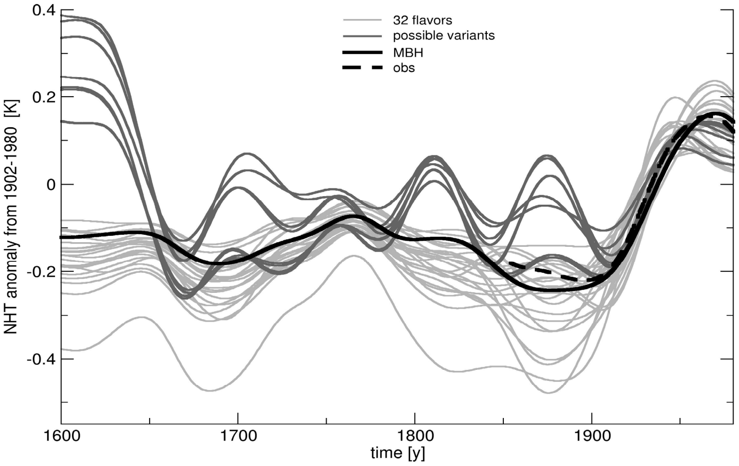


Figure 6: *Different MBH-type results from slight methodological differences from Burger and Cubasch [2005] SI Figure 1.*

Sometimes you're told that scientists have "moved on" and that the methods criticized by Wegman and the NAS panel are no longer used. However, this is not the case. Rutherford et al., coauthored by Dr Mann and published in late 2005, used the identical PC method as the 1998 paper.

> Although 415 individual proxy series were used, data reduction by using leading PCs of tree-ring networks results in a smaller set of 112 indicators in the multiproxy–PC network available back to 1820 (Fig. 1a), with a decreasing number of indicators available progressively further back in time. Twenty-two of the indicators (representing 95 individual proxy series) extend back to at least A.D. 1400.

Mann's PC1 was also used in Osborn and Briffa 2006. And despite criticisms of the PC methodology by the NAS panel, they themselves used it, perhaps inadvertently, in one of their illustrations as a temperature proxy – see the top panel of Figure 6 of the NAS report.

An important control on any statistical study is reporting of adverse results. The verification r^2 statistic is commonly used in paleoclimate studies and was said to have been considered in MBH98. However, its early periods had insignificant values of this statistic, a fact that was never reported. At the NAS press conference, Dr Bloomfield said that he found nothing unusual about reporting of results in MBH. If paleoclimate research practices do not require scientists to disclose results adverse to their claims, then this reduces the ability of policy-makers to rely on these studies.

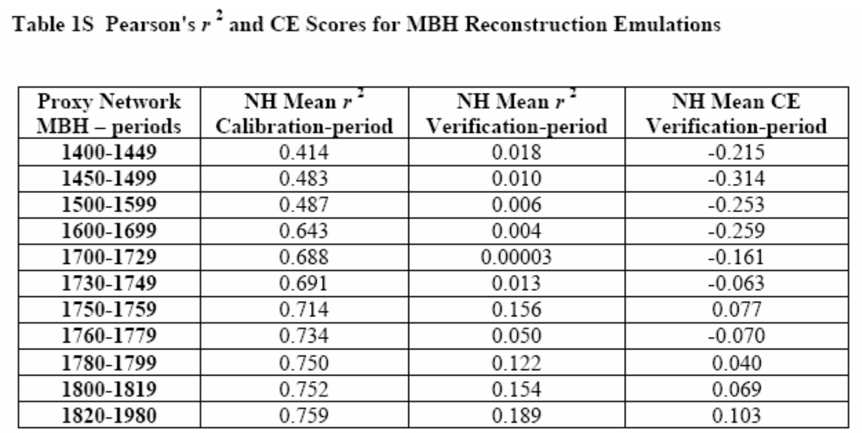


Table 1S Pearson's r^2 and CE Scores for MBH Reconstruction Emulations

Proxy Network MBH – periods	NH Mean r^2 Calibration-period	NH Mean r^2 Verification-period	NH Mean CE Verification-period
1400-1449	0.414	0.018	-0.215
1450-1499	0.483	0.010	-0.314
1500-1599	0.487	0.006	-0.253
1600-1699	0.643	0.004	-0.259
1700-1729	0.688	0.00003	-0.161
1730-1749	0.691	0.013	-0.063
1750-1759	0.714	0.156	0.077
1760-1779	0.734	0.050	-0.070
1780-1799	0.750	0.122	0.040
1800-1819	0.752	0.154	0.069
1820-1980	0.759	0.189	0.103

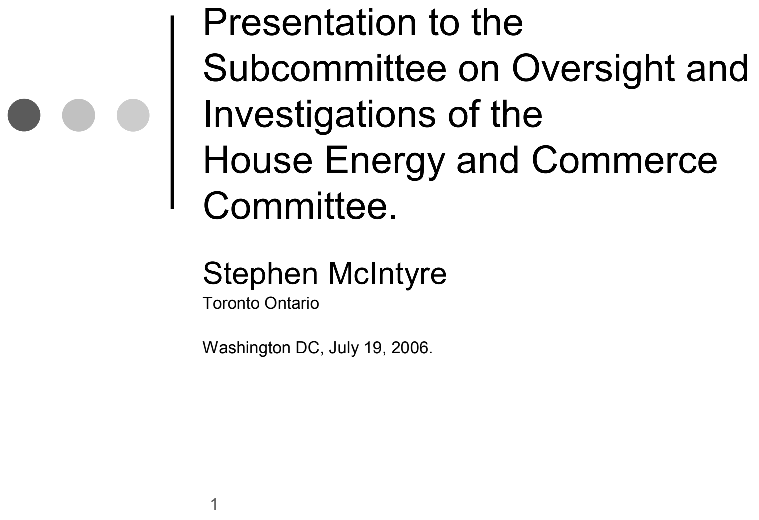
Source: Wahl and Ammann 2006.

Last week, we pointed out many problems with data and code access in paleoclimate. In the MBH case, much relevant data did not become available until the 2004 corrigendum, 6 years after the original study, and only then after a formal complaint to Nature. The efforts of your committee led to Dr Mann disclosing a considerable amount of source code. Unfortunately, as Dr Wegman reported to you, the source code does not work with any data sets presently archived and is inoperable. It also does not include code for some important steps, such as MBH99 confidence intervals or PC retention rules, which neither ourselves nor Wahl and Ammann have been able to replicate. Since Wahl and Ammann are recent coauthors and collaborators with Mann, their efforts hardly can be described as "independent" replication.

Dr Mann and his associates are by no means the worst in the paleoclimate field in archiving data. It is undoubtedly frustrating for Dr Mann to be the center of attention when many of his colleagues are much worse. For example, despite over 2 years of effort, I have been unsuccessful in learning what sites were used in one of three paleoclimate studies illustrated in the IPCC Third Assessment Report (Briffa et al 2001). These sites were recently been used by Mann and coauthors, who have also failed to even disclose the location of the sites.

The reason why data access and replication should be of concern to you is that:

(1) peer review at journals is very limited and does not constitute sufficient due diligence for policy reliance;
(2) IPCC does not carry out due diligence on articles.
(3) In order to properly assess a study, it needs to be replicated. Placing obstacles in the way of access to data and code makes this either impossible or simply impractical for people with less than infinite patience.
(4) Because much of the work is funded by the U.S. federal government, there are direct and practical steps that can be taken with NSF and DOE that would have an immediate impact in improving the quality of due diligence in this field.



Presentation to the Subcommittee on Oversight and Investigations of the House Energy and Commerce Committee.

Stephen McIntyre
Toronto Ontario

Washington DC, July 19, 2006.

NAS Panel Recommendation on Bristlecones Affects MBH

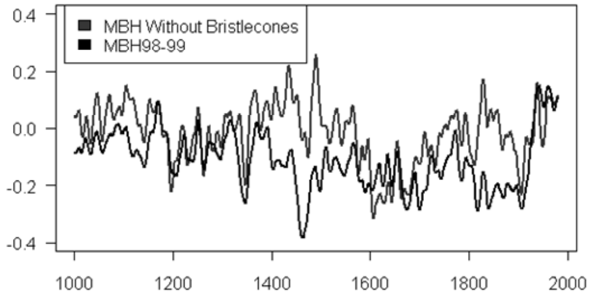
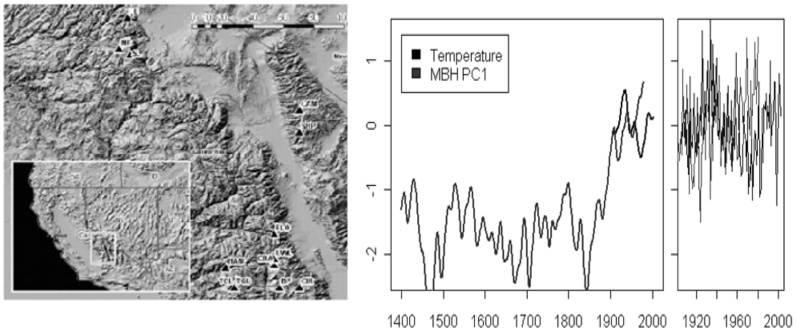


Figure 1. MBH99 reconstruction and estimate of MBH99-type reconstruction without bristlecones. 20-year gaussian smooth.

Up-to-date temperature data from Sierra Nevadas shows no correlation of key MBH PC indicator to temperature

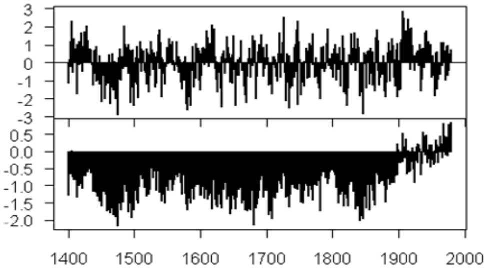


Left - location of foxtail and bristlecone sites in the Sierra Nevada and White Mountains, California. Sheep Mountain is denoted SHP. Right - Black – annual mean of maximum and minimum temperatures (data, Christy, pers. comm.); red – MBH98 NOAMER PC1.

MWP annual minimum temperature in the California Sierra Nevadas estimated to be +3.2 °C warmer than at present

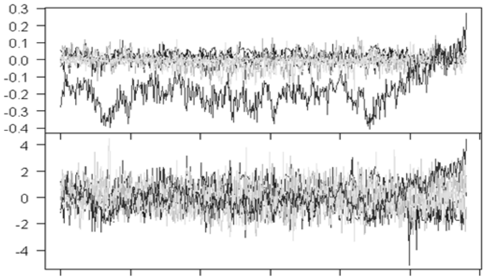
Left: A dead trunk above current treeline from a foxtail pine that lived about 1000 years ago near Bighorn Plateau in Sequoia National Park.

PCs matter: a simple average of MBH proxies does not yield a hockey stick



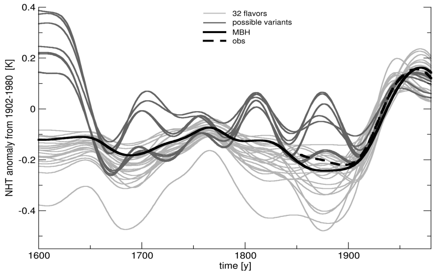
Top: average of MBH proxies. Bottom: result of MBH method.

MBH Methods enhance weighting of bristlecones – a proxy rejected by NAS



Top – Contribution (deg C) of 10 proxy groups (proxy type x continent e.g. Asian tree rings; South American ice cores) to the MBH reconstruction, with bristlecones/foxtails in red. The bristlecone contribution closely matches the final MBH reconstruction.
Bottom – Same series in standard deviation units.

New research in late 2005 showed that a bewildering variety of results can be obtained from MBH proxies.



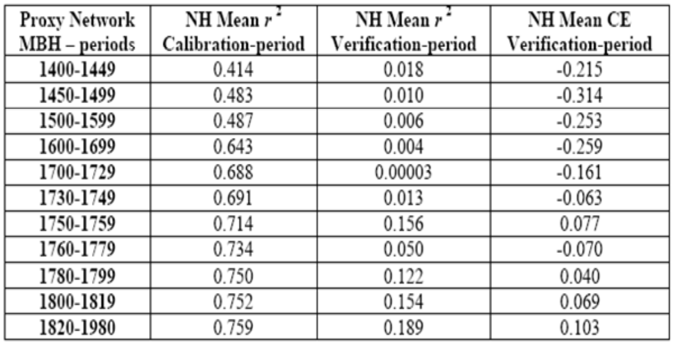
Source: Bürger and Cubasch SI Figure 1

Moving On?: Precisely the same PC series were used late last year in Rutherford et al 2005

> “Although 415 individual proxy series were used, data reduction by using leading PCs of tree-ring networks results in a smaller set of 112 indicators in the multiproxy–PC network available back to 1820 (Fig. 1a), with a decreasing number of indicators available progressively further back in time. Twenty-two of the indicators (representing 95 individual proxy series) extend back to at least A.D. 1400.”

MBH Results Failed Verification r^2 Test

Table 1S Pearson's r^2 and CE Scores for MBH Reconstruction Emulations



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1820-1980	0.759	0.189	0.103

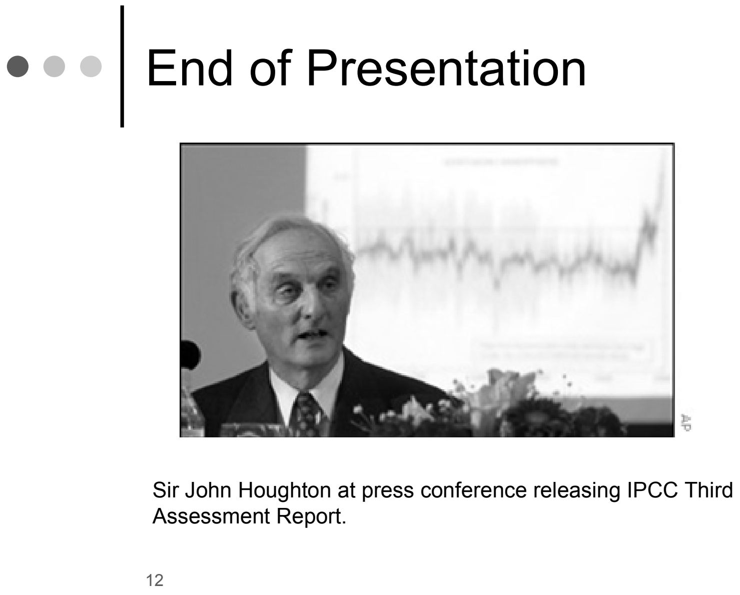
Source: Wahl and Ammann 2006

Replication Problems Persist

- MBH source code provided to Committee does not work with any data sets presently archived and is inoperable.
- code for some important steps, such as MBH99 confidence intervals or the application of Preisendorfer's Rule N, was not provided
- Other authors are even worse: identity of sites in Briffa et al 2001 (also used in Rutherford et al 2005) remain unreported and unavailable

You can make a difference …

- peer review at journals is very limited and is insufficient due diligence for policy reliance;
- IPCC does not carry out due diligence on articles;
- to enable and facilitate independent testing, paleoclimate research practices need to achieve dramatically improved standards for archiving data and code;
- Because much of the work is funded by the U.S. federal government, administrative changes at NSF and DOE would have an immediate impact on paleoclimate due diligence.



End of Presentation

Sir John Houghton at press conference releasing IPCC Third Assessment Report.

MR. WHITFIELD. Thank you. Dr. Gulledge, you are recognized for 5 minutes.

DR. GULLEDGE. Thank you, Mr. Chairman, Ranking Member, and members of the committee. I am Jay Gulledge. I am a Senior Research Fellow with the Pew Center on Global Climate Change, and an Adjunct Assistant Professor at the University of Louisville, where I conduct research on the carbon cycling. I just want to try to provide a little bit of context here today. I am not a paleoclimatologist or a statistician, but I am a professional scientist observing--I am a generalistic climate change scientist through my duties at the Pew Center on Global Climate Change.

[Slide]

Next slide, please. I just want to reiterate, now Dr. Cicerone mentioned most of these things, but this is not about the fundamentals of climate change science and the hockey stick reconstruction is not a foundation. Chain activities are increasing greenhouse gases. The Earth is warming. These are unequivocal facts. The warming over the past 5 decades has been attributed through sound science to human activities associated with greenhouse gases. The effects of warming are being seen today all over the globe, and this warming is going to continue for a long time even if we stabilize greenhouse gases in the atmosphere today. Next slide.

[Slide]

Now the main points I want to make today, the so-called hockey stick controversy is not a scientific construct. The controversy is in science and that is because debate is normal in science and people re-examine each other's methods and so forth. This is not controversial. It is just not controversial in science. The criticisms of the hockey stick scientifically speaking do not undermine the climate, the science of climate change. It is just not central to our understanding of it.

The results of the hockey stick actually represent a gradual development in the understanding in the paleoclimate community of past climate, not any kind of step change in the understanding. This is readily demonstrated from the scientific literature over the past 20 years. And in my opinion climate change assessments are working well under the supervision of climatologists.

[Slide]

The next slide, I just want to point out the bottom quote here from the NAS report that says the surface temperature reconstructions I have included such as the hockey stick are consistent with other evidence of global climate change and can be considered, and this is the operational phrase here, my point, as additional supporting evidence. It is not central to climate science. Next slide.

[Slide]

This is the hockey stick as presented in the 2001 IPCC. It is a reconstruction of the average northern hemispheric temperature over the last thousand years. Next slide.

[Slide]

And the main conclusions as you have heard over and over again the 20th Century is the warmest in the past thousand years. The 1990s were the warmest decade, and even 1998 being the warmest year as represented by the blade of the hockey stick here. Next slide.

[Slide]

Now the criticisms that have been discussed in this hearing as leveled by McIntyre and McKitrick have to do with statistical methodology and whether they were applied properly, inappropriate use of data, and a general complaint that this has resulted in an incorrect elimination of the Medieval Warm Period which would show where the red oval is here. Next slide.

[Slide]

Now as a result of these criticisms this committee has asked Mr. Wegman to produce a report along with his colleagues to examine these criticisms. And the primary objective of this report, as quoted from the report, is to "reproduce the results of McIntyre-McKitrick nor to determine whether the criticisms were valid and have merit." I put in red the last phrase. I think this has not been accomplished by the Wegman report at all, and I will illustrate why. Next slide.

[Slide]

It just seems reasonable that you got to look at what has happened since this because you are trying to find out the reliability of the science here. Second, Mann's claims that McIntyre and McKitrick didn't apply his method correctly are not addressed in the Wegman report at all but they certainly are germane. If those criticisms are being used to question the work then that has to be examined. Corroborating evidence wasn't looked at. That was the strength of the NAS report, I would say. And finally in red here a very important report with regard to the questions of this committee was really overlooked by this report showing up only in a footnote on a later page or on a middle page.

But this thing, this study by Wahl and Ammann from the National Center of Atmospheric Research, actually looked at all the main criticisms of the McIntyre-McKitrick papers, and whether they are correct or not, this should have been examined by any investigation wanting to look into the merits of the McIntyre-McKitrick criticisms. Next slide.

[Slide]

Now what they are showing is that they are able to reproduce extremely closely the original Mann 1998 hockey stick. Here in gray is the original Mann result, and if you can't see it it is because it is under the red line, which is their emulation. They did this writing their own code in the R programming language, and they made a very faithful reproduction. Next.

[Slide]

Now using their reproduction they then tested whether or not the McIntyre-McKitrick criticisms had an effect on the result of the reconstruction. In this figure they have corrected for the de-centering problem prior to the PC analysis, and also they removed the gaspe tree ring series that was questioned by McIntyre and McKitrick. And the result is the only change that occurred that has any significance is in the 14th Century. You see the red line is their emulation of Mann and the blue, which is sticking up a little bit on the very left hand of the graph, is the effect of the corrections.

Now this really just doesn't change--and these green and magenta are the 95 percent confidence intervals. This really just doesn't change the picture of the 20th Century being unique. Now it does leave the impression that perhaps there is a trajectory of warming as you move back in time. Maybe that continues to go up and the Medieval Warm Period, which isn't even shown here, maybe got warm. Next slide, please.

[Slide]

I asked Dr. Ammann yesterday whether or not he had used these corrections and taken them back in time. He said that he had, that he has a paper that is submitted for review on this, and I want to make clear that this hasn't been peer reviewed yet. It is the same correction applied to the data going back a thousand years, and this is the result. The blue line is the emulation of Mann 1999. The red line is the result. And in fact it does not continue to go up. And this is going to be my last slide so don't be concerned. There are a lot more slides in your handout. I want to point out here that if you look at the medieval times here which would be the first couple of frames from the left in that graph it is warmer than what you see to the right of that. There is a Medieval Warm Period on this graph. It is just weak, and that is completely consistent with the scientific examination of paleoclimate over the last 20 years. There has been a consistent trajectory and this is completely consistent with that. Thank you.

[The prepared statement of Dr. Jay Gulledge follows:]

PREPARED STATEMENT OF DR. JAY GULLEDGE, SENIOR RESEARCH FELLOW, PEW CENTER ON GLOBAL CLIMATE CHANGE

Mr. Chairman, Ranking Member, and Members of the Committee:

Thank you for the opportunity to speak today. I am Jay Gulledge, Ph.D., Senior Research Fellow for Science and Impacts at the Pew Center on Global Climate Change. I am also an Adjunct Assistant Professor at the University of Louisville, which houses my academic research program on carbon cycling.

The Pew Center on Global Climate Change is a non-profit, non-partisan and independent organization dedicated to providing credible information, straight answers and innovative solutions in the effort to address global climate change. In our eight years of existence, we have published almost seventy reports by experts in climate science, economics, policy and solutions, all of which have been peer-reviewed and reviewed as well by the companies with which we work.

Forty-one major companies sit on the Pew Center's Business Environmental Leadership Council, spanning a range of sectors, including oil and gas (BP, Shell), transportation (Boeing, Toyota), utilities (PG&E, Duke Energy, Entergy), high technology (IBM, Intel, HP), diversified manufacturing (GE, United Technologies), and chemicals (DuPont, Rohm and Haas). Collectively, the 41 companies represent two trillion dollars in market capitalization and three million employees. The members of the Council work with the Pew Center to educate the public on the risks, challenges and solutions to climate change.

If you take nothing else from my testimony, please take these three points:

1. The scientific evidence of significant human influence on climate is strong and would in no way be weakened if there were no Mann hockey stick.
2. The scientific debate over the Medieval Warm Period (MWP) has been gradually evolving for at least 20 years. The results of the Mann hockey stick simply reflect the gradual development of thought on the issue over time.
3. The impact of the McIntyre and McKitrick critique on the original Mann paper, after being scrutinized by the National Academy of Science, the Wegman panel and a

number of meticulous individual research groups, is essentially nil with regard to the conclusions of the Mann paper and the 2001 IPCC assessment.

The science of climate change is an extraordinary example of a theory-driven, data-rich scientific paradigm, the likes of which, arguably, has not occurred since the development of quantum mechanics in the first half of the twentieth century. The product of this strong scientific framework is a body of strong, multifaceted evidence that man-made greenhouse gases are causing contemporary global warming, and that this warming trend is inducing large-scale changes in global climate. The primary evidence is based on physical principles and observational and experimental analysis of contemporary climate dynamics, as opposed to analyses of past climates, which are the subject of this hearing. We can now say with confidence that the evidence of human influence on climate is strong, as described by Dr. Cicerone.

Although paleoclimatology – the study of ancient climates – is an important part of the climate science frame work, reconstructions of temperature over the past millennium play a secondary, expendable role in the larger body of evidence, as stated in the recent NAS report titled, *Surface Temperature Reconstructions for the Last 2,000 Years*: "**Surface temperature reconstructions are consistent with other evidence of global climate change and can be considered as additional supporting evidence**" (National Research Council 2006, p. 23; hereafter referred to as the NAS report). Dispensing with such reconstructions entirely or proving them fundamentally flawed would have little, if any, impact on our understanding of contemporary climate change. This statement does not imply that millennial climate reconstructions are unimportant, but their main influence will be in the future, when their potential to reveal how climate varied across the earth's surface from year-to-year in the past (i.e. an annual record of spatially explicit climate dynamics) is fully realized. At that point, such reconstructions will be used in a manner parallel to thermometer records today. This capability would contribute significantly to resolving the current genuine debate in climate science, which is not about whether humans are changing the climate—a point over which there is no scientific controversy—but is about how much human influences will change the climate in the future as a result of greenhouse gas accumulation and other forcings we apply to the climate system. In other words, the goal of spatially explicit paleoclimate reconstructions is to help climatologists determine how physical forcings, such as solar radiation, volcanic eruptions, land-use changes, and changes in atmospheric greenhouse gases, have affected the planet in the past, so that we can improve estimates of how they will do so in the future.

The early MBH reconstructions (Mann et al. 1998; Mann et al. 1999; hereafter referred to as MBH98 or MBH99 or, collectively, MBH) were the first to offer spatially explicit climate reconstructions and therefore represented a breakthrough in climate change science that continues to develop and promises to further our understanding of climate physics in the future. The Wegman report's conclusion that paleoclimatology "does not provide insight and understanding of the physical mechanisms of climate change" (p. 52), fails to appreciate that the purpose of Dr. Mann's research is to improve our knowledge of physical mechanisms of climate change by examining how they operated in the past.

Turning our attention to the methodological issues this hearing seeks to investigate, in my opinion, the Wegman report failed to accomplish its primary objective, which was "**to reproduce the results of [McIntyre & McKitrick] in order to determine whether their criticisms are valid and have merit**" (p. 7). Although the panel reproduced MM's work—verbatim—it only partially assessed the validity, and did not at all assess the merits, of the criticisms directed toward the MBH reconstructions. For instance, MM (McIntyre and McKitrick 2003; McIntyre and McKitrick 2005; heafter referred to collectively as MM) allege that the so-called MBH "hockey stick" result is biased by methodological errors that undermine the conclusion that the late 20th century was uniquely warm relative to the past 1000 years. This critique only has merit if, after correcting for the errors pointed out by MM, the resulting reconstruction yields results significantly different from the original result that can no longer support the claim of unusual late 20th century warmth. However, the Wegman Report takes no steps to make such a determination.

Fortunately, a different group, one well qualified both statistically and climatologically to tackle this question of merit, had already performed the task several months before the Wegman Report was released. The study by Wahl & Ammann (In press; hereafter referred to as WA06), was peer-reviewed and accepted for publication in the journal, *Climatic Change*, early last spring, and has been publicly available in accepted form since last March (http://www.cgd.ucar.edu/ccr/ammann/millennium/refs/WahlAmmann_ClimChange2006.html). This study, titled, **Robustness of the Mann, Bradley, Hughes Reconstruction of Northern Hemisphere Surface Temperatures: Examination of Criticisms Based on the Nature and Processing of Proxy Climate Evidence**, carefully reproduced the MBH98 reconstruction and then used their faithful reproduction to test MM's suggested corrections. They tested each of the criticisms raised by MM in all of their published papers, including both the peer-reviewed and non-peer-reviewed papers. Given that this report specifically examined MM's criticisms, including the decentering issue that was the main focus of the Wegman report, it is unfortunate that the Wegman report dismissed it in a footnote (p. 48) as "not to the point."

WA06 have performed a meticulous and thorough evaluation of MBH98, and the answers that this committee seeks about the MBH reconstructions are to be found within this report. After examining each of MM's three methodological criticisms, WA06 accepted two of them as valid, and have used them to correct the MBH98 reconstruction. I will now show you what effect these corrections have on the MBH98 reconstruction, and then reconsider the uniqueness of the late 20th-century warming trend in the light of these corrections.

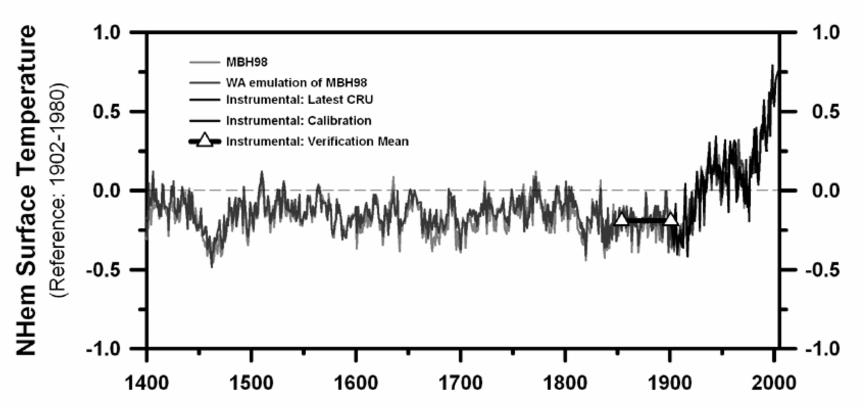


Fig. 1. WA06 reproduction of MBH98 reconstruction. The gray line is the original MBH98 reconstruction. The red line is the WA06 emulation. The black line is the original 1992 instrumental data used for calibration. The blus line is the latest instrumental data. The black bar with triangles represents the mean of the instrumental mean of the verification period.

The original MBH98 "hockey stick" is shown as a gray line (Fig. 1). The WA06 reproduction of MBH98 is shown in red (Fig. 1). Except for a couple of minor simplifications, WA06 remained faithful to the original MBH method and retained all of the original MBH data, including the original instrumental temperature series from 1992. They wrote their own computer code to perform the calculations, using the R programming language, as recommended by the MM and the Wegman report, rather than the original Fortran language used by Dr. Mann. As you can see, the two reconstructions are materially the same. This result demonstrates that MBH98 can be reproduced based on information available in the original MBH papers and supplemental information and data available on the Internet.

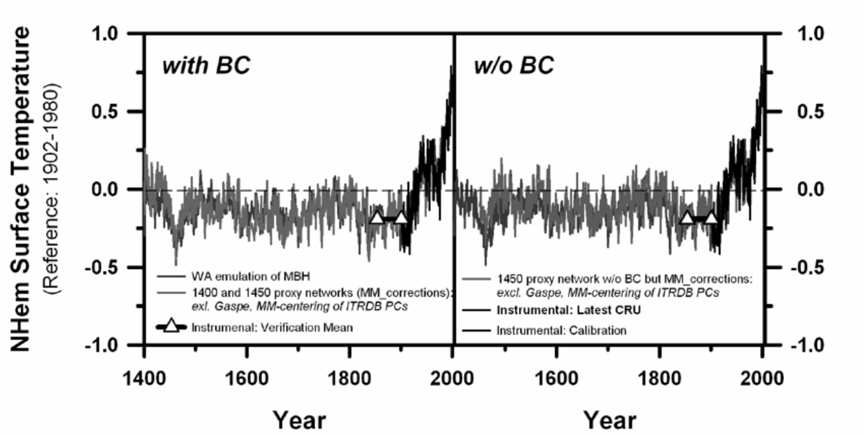


Fig. 2. WA06 corrections of MBH98 for accepted MM corrections. The left frame shows original WA06 emulation of MBH in red and the corrected reconstruction accounting for decentering and excluding the Gaspe tree-ring series in blue. The right frame shows the same but with the bristle cone pine series removed (green line). Instrumental data are shown in black.

With this successful reproduction in hand, WA06 were able to test the effects of each of MM's criticisms on the outcome of the MBH98 reconstruction. After carefully considering the validity of MM's three criticisms of MBH's reconstruction methodology, WA06 agreed that 1) decentering the proxy data prior to Principle Component analysis and 2) including the poorly replicated North American Gaspé tree-ring series from 1400-1449 both affected the MBH results. After correcting for these effects, WA06 obtained the results shown in blue (Fig. 2, left frame). The result is a slightly warmer (0.1 °C) early 15th century, with no other time period affected. MM's third methodological criticism surrounding the inclusion of the bristlecone/foxtail pine series was rejected for several reasons. The right frame in Fig. 2 illustrates that excluding these series has little effect on the MBH98 reconstruction, except to force it to begin in 1450 instead of 1400, because of lack of a data. Since the exclusion had little effect, and losing these data series would hinder reconstructions of earlier climate, WA06 rejected this criticism.

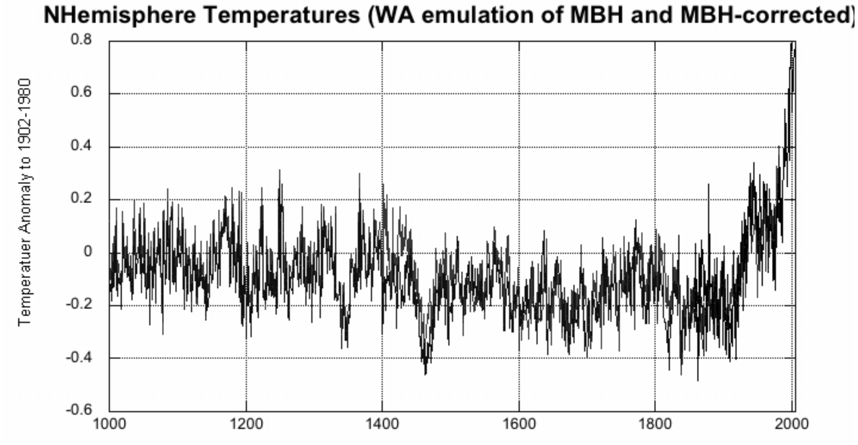


Fig. 3. Wahl-Ammann corrections of the MBH99 reconstruction (Ammann & Wahl, submitted). The original MBH99 reconstruction is shown in blue and the corrected WA version is shown in red. Corrections were made for the decentering issue and the Gaspe tree-ring series. Instrumental data are in black.

The additional 15th-century warmth revealed by making the valid MM corrections still does not approach the warmth of the late 20th century, so MM's critique cannot yet be said to have merit. However, the corrected result creates the impression of an upward temperature trend backward in time before 1400, begging the question of what would happen to the Middle Ages in the 1000-year MBH99 reconstruction if it were also corrected? Answering that question is requisite for determining the merit of MM's critique of MBH. The original 1000-year MBH99 reconstruction is shown in blue and the corrected version is shown in red (Fig. 3; Ammann & Wahl, submitted). Carrying the correction back to the full millennium reveals that the largest effects remain in the early 15th century, and both earlier and later periods were less affected. Therefore, there is very little difference between the corrected MBH98 and MBH99 reconstructions and the originals, and the original observation that the late 20th century is uniquely warm in the context of the past 1000 years is not affected. Hence, the valid methodological caveats that MM pointed out do not undermine the main conclusions of the original MBH papers or the conclusion of the 2001 IPCC assessment.

The scientific debate over the Medieval Warming Period (MWP) has been on the same trajectory for at least 20 years, with early indications that the MWP was not a globally coherent event becoming more solid over time. The MBH99 reconstruction represented an evolutionary step—not a revolutionary change—in this established trajectory. The 1990 IPCC figure that Mr. McIntyre, the *Wall Street Journal* editorial page, and Dr. Wegman have used in their own assessment of past climate is a cartoon, as stated by Dr. Wegman in his testimony last week. I have confirmed this with a number of individuals who were involved with the 1990 IPCC report or with versions of the schematic that pre-dated the 1990 IPCC report. The schematic is not a plot of data and is inappropriate as a comparison to MBH. The text of the 1990 IPCC report clearly states that the figure is a "schematic diagram" and that "it is still not clear whether all the fluctuations indicated were truly global" (p. 202). Furthermore, only three sources of information were cited and those sources conflicted on whether the Northern Hemisphere was warm or cold: **"The late tenth to early thirteenth centuries… appear to have been exceptionally warm in parts of western Europe, Iceland and Greenland… China was, however, cold at this time, but South Japan was warm…"** Clearly, this report certainly did not paint a picture of any consensus regarding a Medieval Warm

Period as a hemisphere-wide phenomenon and characterizing it as such reveals a fundamental misunderstanding of climate science.

The 1992 and 1995 IPCC reports continued this same trajectory of thought. Four years before MBH99, citing 6 papers—still a very limited number by twice as many as were cited in 1990—the 1995 report stated:

> There are, for this last millennium, two periods which have received special attention, the Medieval Warm Period and the Little Ice Age. These have been interpreted, *at times*, as period of global warmth and coolness, respectively. Recent studies have re-evaluated the interval commonly known as the Medieval Warm Period to assess the magnitude and geographical extent of any prolonged warm interval between the 9th and 14th centuries… The available evidence is limited (geographically) and is equivocal. …a clearer picture may emerge as more and better calibrated proxy records are produced. However, at this point, it is not yet possible to say whether, at a hemispheric scale, temperatures declined from the 11-12th to the 16-17th century. Nor, therefore, is it possible to conclude that the global temperatures in the Medieval Warm Period were comparable to the warm decades of the late 20th century" (p. 174).

Remember that this was written by a team of climatologists as a consensus statement. The consensus at this time, as in 1990, was that there was no strong evidence of a hemisphere-wide MWP.

Continuing the same trajectory, the 2001 IPCC Third Assessment Report examined evidence from 10 cited sources for the MWP. The consensus at this point seemed to be turning to the conclusion that the there actually was a generally warm Northern Hemisphere during the Middle Ages, but that it was not a strong, coherent pattern of warming:

> It is likely that temperatures were relatively warm in the Northern Hemisphere as a whole during the earlier centuries of the millennium, but it is much less likely that a globally-synchronous, well defined interval of "Medieval warmth" existed, comparable to the near global warmth of the late 20th century… Marked warmth seems to have been confined to Europe and regions neighboring the North Atlantic.

Since the MBH reconstructions were hemisphere-wide, and the MWP probably was not, it should not surprise us that the reconstructions lack a strong MWP (MBH99 does show slightly warmer temperatures in the 9th to 14th centuries than in the 15th to 19th centuries).

All available evidence indicates that the situation during the Middle Ages was fundamentally different that what is happening with climate today, which is a well-documented, globally coherent warming trend that is happening North, South, East, and West; at low latitudes and high latitudes; over land and over—and into—the sea. There are new data, published earlier this year, indicating that the atmosphere above Antarctica has warmed dramatically in recent decades (Turner et al. 2006). There is no large region on Earth where large-scale 20th century warming has not been detected, which simply cannot be said of the MWP.

Wahl and Ammann (2006) have demonstrated that the results of MBH are robust "down in the weeds":

> Our examination does suggest that a slight modification to the original Mann et al. reconstruction is justifiable for the first half of the 15th century (~ +0.05°), which leaves entirely unaltered the primary conclusion of Mann et al. (as well as many other reconstructions) that both the 20th century upward trend and high late-20th century hemispheric surface temperatures are anomalous over at least the last 600 years.

The NAS has affirmed the MBH results are also robust in the bigger picture, as well:

> The basic conclusion of MBH99 was that the late 20th century warmth in the Northern Hemisphere was unprecedented during at least the last 1,000 years. This conclusion has subsequently been supported by an array of evidence that includes both additional large-scale surface temperature reconstructions and pronounced changes in a variety of local proxy indicators, such as melting on icecaps and the retreat of glaciers around the world, which in many cases appear to be unprecedented during at least the last 2,000 years. Not all individual proxy records indicate that the recent warmth is unprecedented, although a larger fraction of geographically diverse sites experienced exceptional warmth during the late 20th century than during any other extended period from A.D. 900 onward. (p. 3)

Examination of the IPCC reports through time, as well as the primary scientific literature, reveals why the MBH results are so robust—MBH simply assimilated all the available evidence into a quantitative reconstruction—evidence that had already been evaluated qualitatively as lacking a coherent MWP.

This committee is seeking to know the significance of the criticisms leveled at the MBH reconstruction for climate change assessments. The significance is that these criticisms have resulted in the most thoroughly vetted single climate study in the history of climate change research. Dr. Tom Karl summarized the impact most succinctly in his testimony to this committee last week when he said that he would stand by the IPCC's original assessment: "If you ask me to give qualifications about the findings in the 2001 report with the same caveat in terms of defining likelihood, I personally would not change anything." Hence, the impact of the MM critique, after being scrutinized by the NAS, the Wegman panel, and a number of meticulous individual research groups, is essentially nil with regard to the conclusions of MBH and the 2001 IPCC assessment.

Also relevant to this committee's questions about climate change assessments is the revelation that climate scientists do know their business, and that a lack of knowledge of geophysics is a genuine handicap to those who would seek to provide what they deem "independent review." If the assessment of climate science presented in Mr. McIntyre's presentation to the NAS committee, the Wegman Report, and the WSJ is an example of what can be expected from those who have not conducted climate research, then the investigation launched by this committee has demonstrated clearly that "independent review" by non-climate scientists is an exceedingly ineffective way to make climate change assessments.

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MR. WHITFIELD. Dr. Wegman, you are recognized for 5 minutes.

DR. WEGMAN. Thank you. Good afternoon. I would like to begin by summarizing our previous testimony. Let me first begin by circumscribing the substance of our report. As you know, we were asked to provide an independent verification by statisticians of the critiques of the statistical methodology found in the papers of Dr. Michael Mann, Raymond Bradley, and Malcolm Hughes, published respectively in *Nature and Geophysical Review Letters*. These two papers have commonly been referred to MBH98 and 99. The critiques have been made by Stephen McIntyre and Ross McKitrick published in *Energy and Environment* in 2003, and in that same journal and also in *Geophysics Research Letters* in 2005. We refer to these as MM03, 05A, 05B, respectively.

We were also asked about the implications of our assessment. We were not asked to assess the reality of global warming, and indeed this is not an area of our expertise. Our panel was composed of myself from George Mason University, Dr. David W. Scott from Rice University, and Yasmin Said, Dr. Said, from the Johns Hopkins. This ad hoc panel has worked pro bono, has received no compensation, and has no financial interest in the outcome.

The debate over Dr. Mann's principal components methodology has been going on for nearly 3 years. When we got involved, there was no evidence that a single issue was resolved or even nearing resolution. Dr. Mann's RealClimate.org website said that all of Mr. McIntyre and Dr. McKitrick claims had been discredited. UCAR had issued a news release saying that all their claims were unfounded, by the way, based on the Ammann paper just referred to.

The situation was ripe for a third party review of the types that we and Dr. North's NRC panel have done. Because of the very high visibility of the original study, we see no harm and much advantage of having two independent analyses of the situation, from quite different perspectives.

While the two studies overlap on the important topic of Mann's principal components methodology, Dr. North's NRC panel considers topics that were outside the scope of our study, such as other temperature reconstructions. Where we have commonality, I believe our report and the NRC panel essentially agree. The error in the use of principal components methodology, the NRC panel reported under some conditions, the leading principal component can exhibit a spurious trend in the proxy-based reconstruction.

The NRC panel illustrated this with their own spurious hockey stick in Figure 9-2 on page 87 of the report. Our explanation of this phenomenon was similar, the authors make the seemingly innocuous and somewhat obscure calibration assumption. Because the instrumental temperature records are only available for a limited window, they use instrumental temperature data from 1902-1995 to calibrate the proxy data set. This would seem reasonable except for the fact that temperatures were rising during this period. So that centering on this period has the effect of making the mean value for any proxy series exhibiting the same increasing trend to be decentered low.

Because the proxy series exhibiting the rising trend are decentered, the calculated variance will be larger than their normal variance when calculated based on centered data, and hence they will tend to be selected preferentially as the first principal component. The centering of the proxy data is a critical factor in using principal components methodology.

The effect of decentering was illustrated by us in Figure 2, which is Figure 4.3 in our report. The top panel represents the North American Tree Ring PC1 as calculated based on the MBH98 methodology. The bottom panel illustrates the PC1 based on the same set of tree ring proxies with the centered PCA computation. We believe that our discussion, together with the discussion from the NRC report should take the centering issue off the table. The decentering methodology is simply incorrect mathematics as was illustrated in our Appendix A as well as with ample simulation evidence in both our report and that of the NRC report.

I am baffled by the claim that incorrect method doesn't matter because the answer is correct anyway. The method wrong plus answer correct is just bad science. But with the centering issue off the table, the question then shifts from principal component analysis to which proxies exhibit the hockey stick shape and whether these proxies contain valid temperature signals. We agree with Dr. Mann that the hockey stick shape is in some proxies.

Figure 4 is an image that I showed in our previous testimony showing just six bristlecone pine proxies used in the construction of the North American PC1 series. The hockey stick shapes are clearly visible in the last two proxies. Given our discussion, it is clear how the decentering methodology will select these and give them prominence in PC1. So the question is are these valid temperature proxies. I quote from our report, "Graybill and Idso, 1993, specifically sought to show that bristlecone pines were CO_2 fertilized. Bondi et al., 1999, suggest bristlecones are not reliable temperature proxy for the last 150 years as it shows an increasing trend in about 1850 that has been attributed to atmospheric CO_2 fertilization." We also know that IPCC 1996 report stated that the possible confounding effects of carbon dioxide fertilization need to be taken into account when calibrating tree ring data against climate variations. At the very least, the effect of these proxies on temperature reconstruction should be examined.

Figure 5 shows Dr. Mann's own illustration, MBH, Internet, 2003, of the direct effect of North American tree ring data on reconstruction results in the 15th century. Indeed, it is our understanding as outsiders that all parties agree as to the significance of this tree ring network to final results, and that has made the use of the tree ring network a disputed issue as Mr. McIntyre has just pointed out.

Figure 6 is also a repeat graphic from my previous testimony. Please note that the Bristlecone/Foxtail PC1 proxy is used not only in MBH, but in virtually every subsequent reconstruction. We do not claim to be experts in dendrology either but it seems to us as outsiders that there are sufficient confounding factors that proxies based on bristlecones should be avoided. We should add that we were specifically asked to resolve the differences between MPH98/99 and the McIntyre and McKitrick papers. There is a bewildering array of subsequent work that we were not asked to consider, but which probably deserves much more intense scrutiny. We would include such refereed papers as Rutherford et al., 2005, and Wahl and Ammann, 2006, which are purported to be written by independent teams, but which are co-authored by Dr. Mann himself in Rutherford et al. and by Dr. Mann's student Dr. Ammann in Wahl and Ammann.

MR. WHITFIELD. Dr. Wegman, excuse me for interrupting. You are about 3 minutes over on the testimony, and we did hear your testimony last week and we have it in the record. And we genuinely appreciate your being back here today, and I am sure we will have some questions for you.

DR. WEGMAN. Thank you, sir.

MR. WHITFIELD. And you adequately covered last week also the social network and which we appreciate very much.

[The prepared statement of Dr. Edward J. Wegman follows:]

PREPARED STATEMENT OF DR. EDWARD J. WEGMAN, DIRECTOR, CENTER FOR COMPUTATIONAL STATISTICS

Good morning. I would like to begin by summarizing our previous testimony. The debate over Dr. Mann's principal components methodology has been going on for nearly three years. When we got involved, there was no evidence that a single issue was resolved or even nearing resolution. Dr. Mann's RealClimate.org website said that all of the Mr. McIntyre and Dr. McKitrick claims had been "discredited". UCAR[1] had issued a news release saying that all their claims were "unfounded". Mr. McIntyre replied on the ClimateAudit.org website. The climate science community seemed unable to either refute McIntyre's claims or accept them. The situation was ripe for a third-party review of the types that we and Dr. North's NRC panel have done. Because of the very high visibility of the original study, we see no harm and much advantage of having two independent analyses of the situation, from quite different perspectives.

While the two studies overlap on the important topic of Mann's principal components methodology, the Dr. North's NRC panel considers topics that were outside the scope of our study, such as other temperature reconstructions. Where we have commonality, I believe our report and the NRC panel essentially agree. On the error in the use of principal components methodology, the NRC panel reported, "…under some conditions, the leading principal component can exhibit a spurious trend in the proxy-based reconstruction. To see how this can happen, suppose that instead of proxy climate data, one simply used a random sample of autocorrelated time series that did not contain a coherent signal. If these simulated proxies are standardized as anomalies with respect to a calibration period and used to form principal components, the first component tends to exhibit a trend, even though the proxies themselves have no common trend. Essentially, the first component tends to capture those proxies that, by chance, show different values between the calibration period and the remainder of the data."

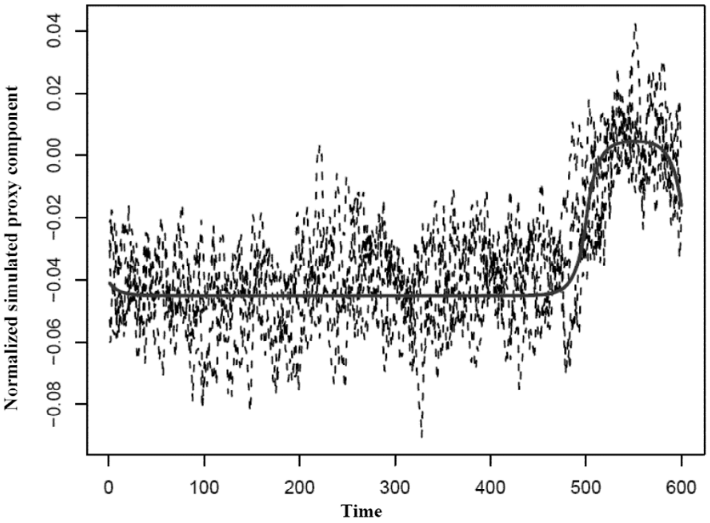

Figure 1 – Spurious Hockey Stick as reported in the NAS Panel report (Figure 9-2 in the NAS report).

The NRC panel illustrated this with their own spurious hockey stick in Figure 9-2 on page 87. Our explanation of this phenomenon is similar. "… the authors make a seemingly innocuous and somewhat obscure calibration assumption. Because the instrumental temperature records are only available for a limited window, they use instrumental temperature data from 1902-1995 to calibrate the proxy data set. This would seem reasonable except for the fact that temperatures were rising during this period. So that centering on this period has the effect of making the mean value for any proxy series exhibiting the same increasing trend to be decentered low. Because the proxy series exhibiting the rising trend are decentered, the calculated variance will be larger than their normal variance when calculated based on centered data, and hence they will tend to be selected preferentially as the first principal component. … The centering of the proxy series is a critical factor in using principal components methodology."

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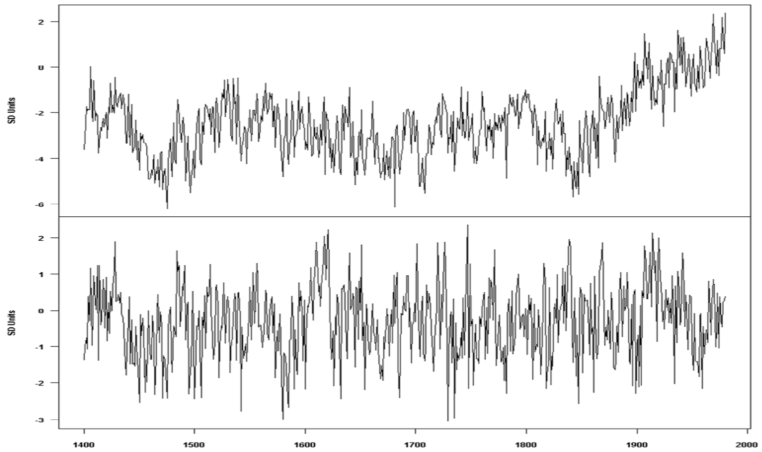
[1] UNIVERSITY CORPORATION FOR ATMOSPHERIC RESEARCH, FINANCIAL ARM OF THE NATIONAL CENTER FOR ATMOSPHERIC RESEARCH (NCAR).



The North American Tree Network PC1 proxy was featured in MBH98. The top panel is the PC1 using the MBH98 methodology. The bottom panel is the centered PCA reconstruction.

Figure 2

The effect of decentering was illustrated by us in Figure 2, which is Figure 4.3 in our report. The top panel represents the North American Tree Ring PC1 as calculated based on the MBH98 methodology. The bottom panel illustrates the PC1 based on the same set of tree ring proxies with the centered PCA computation.

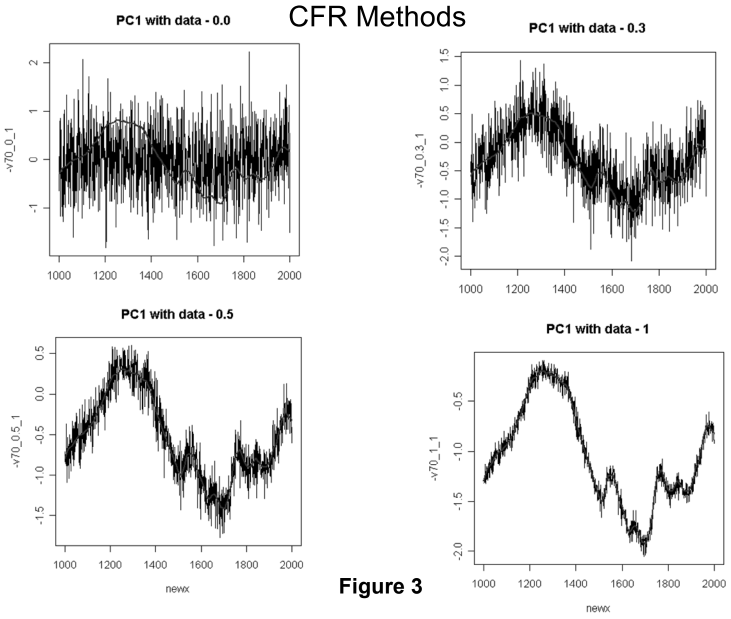


Figure 3

To illustrate that this spurious decentering effect is not limited to just hockey sticks we created an additional illustration based on the IPCC 1990 temperature curve. With 69 uncorrelated white noise proxies and one IPCC 1990 curve, it is clear that decentering can overwhelm the remaining proxies and preferentially select the one anomalous one.

We believe that our discussion together with the discussion from the NRC report should take the "centering" issue off the table. The decentered methodology is simply incorrect mathematics as was illustrated in our Appendix A as well as with ample simulation evidence in both our report and that of the NRC report. I am baffled by the claim that the incorrect method doesn't matter because the answer is correct anyway. Method Wrong + Answer Correct = Bad Science. But with the centering issue off the table, the question then shifts from principal component analysis to which proxies exhibit the hockey stick shape and whether these proxies contain valid temperature signals. We agree with Dr. Mann that the hockey stick shape is in some proxies.

Sample Proxy Series

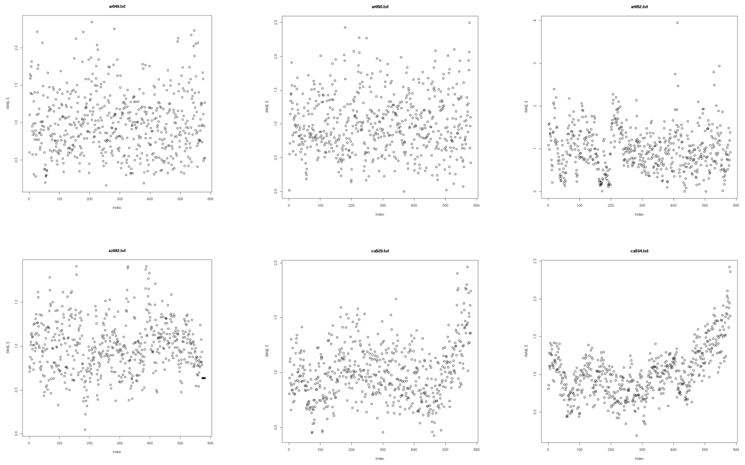


FIGURE 4

Figure 4 is an image that I showed in our previous testimony showing just six sample Bristlecone pine proxies used in the construction of the North American PC1 series. The hockey stick shapes are clearly visible in the last two proxies. Given our discussion, it is clear how the decentering methodology will select these and give them prominence in PC1. Are these valid temperature proxies? I quote from our report, “Graybill and Idso (1993) specifically sought to show that Bristlecone Pines were CO_2 fertilized. Bondi et al. (1999) suggest [Bristlecones] ‘are not a reliable temperature proxy for the last 150 years as it shows an increasing trend in about 1850 that has been attributed to atmospheric CO_2 fertilization.’ … We also note that IPCC 1996 report stated that ‘the possible confounding effects of carbon dioxide fertilization need to be taken into account when calibrating tree ring data against climate variations.’” At the very least, the effect of these proxies on temperature reconstruction should be examined.

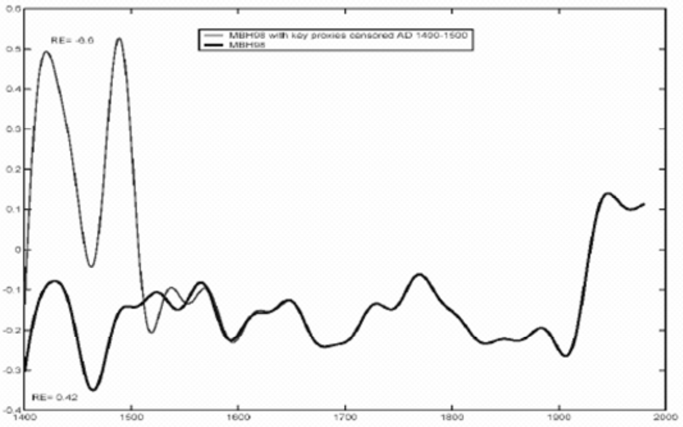
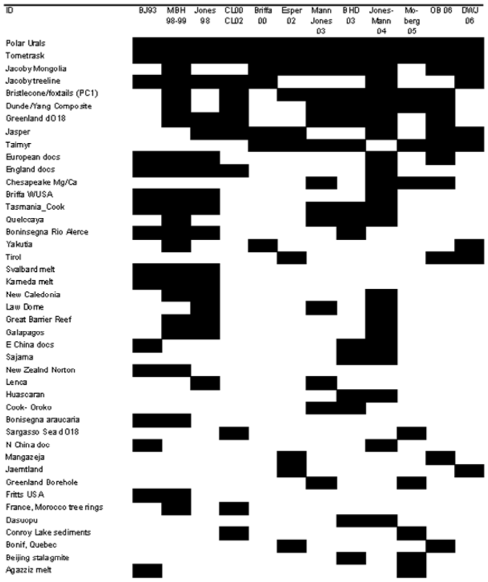


FIGURE 1. COMPARISON OF MBH98 RECONSTRUCTION (BLUE) WITH RECONSTRUCTION RESULTING FROM THE ELIMINATION OF KEY PROXY DATA SETS (1)-(3) OVER THE AD 1400-1500 INTERVAL. THIS YIELDS ESSENTIALLY THE SAME RESULT OBTAINED BY MM BY THE APPARENT ELIMINATION OF THESE DATASETS OVER THE 1400-1600 PERIOD. (BOTH SERIES HAVE BEEN SMOOTHED WITH A 40 YEAR LOWPASS FILTER).

Figure 5

Figure 5 shows Dr. Mann’s own illustration (MBH, Internet, 2003) of the direct effect of North American tree ring data on reconstruction results in the 15th century. Indeed, it is our understanding as outsiders that all parties agree as to the significance of this tree ring network to final results. And that has made the use of the tree ring network a disputed issue.



Bradley & Jones (1993)

Mann, Bradley, Hughes (1998,1999)

Jones et al. (1998)

Crowley & Lowery (2000)

Briffa (2000)

Esper (2002)

Mann & Jones (2003)

Bradley, Hughes, Diaz (2003)

Jones & Mann (2004)

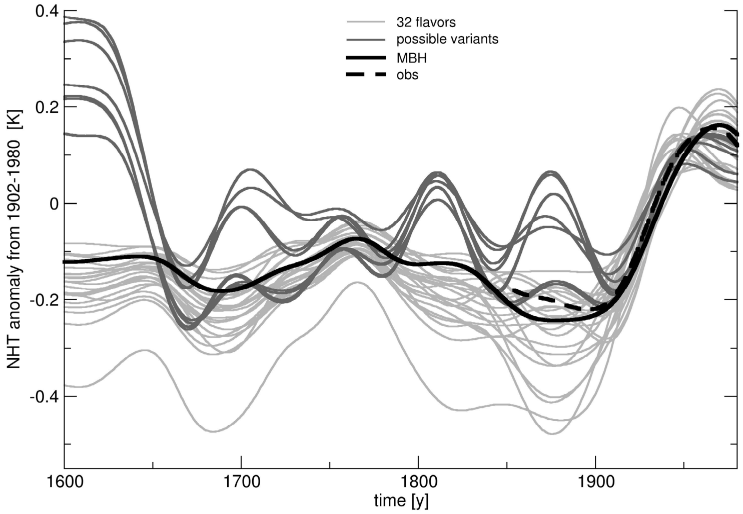
Moberg et al. (2005)

Osborn & Briffa (2006)

D'Arrigo, Wilson, Jacoby (2006)

FIGURE 6

Figure 6 is also a repeat graphic from my previous testimony. Please note that the Bristlecone/Foxtail PC1 proxy is used not only in MBH, but also in virtually every subsequent reconstruction. We do not claim to be experts in dendrology, but it seems to us as outsiders that there are sufficient confounding factors that proxies based on Bristlecones should be avoided. We should add that we were specifically asked to resolve the differences between MBH98/99 and MM03/05a/05b. There is a bewildering array of subsequent work that we were not asked to consider, but which probably deserves much more intense scrutiny. We would include such refereed papers as Rutherford et al. (2005) and Wahl and Ammann (2006), which are purported to be written by independent teams, but which are co-authored by Dr. Mann himself in Rutherford et al. and by Dr. Mann's student Dr. Ammann in Wahl and Ammann.



Indeed, far from there being uniform agreement on the hockey stick shape, Bürger and Cubasch (2005) have reported that a discomforting array of different results can be obtained from MBH proxies under minor methodological differences. Figure 7 illustrates that while there may be reasonable consensus on warming since 1900, i.e. the calibration period, as the NRC report suggests, paleoclimate temperature reconstruction past 1600 is much more problematic. Indeed, on the matter of consensus, the NOAA website titled *A Paleo Perspective ... on Global Warming* has the following contradictory statements: "The latest peer-reviewed paleoclimatic studies appear to confirm that the global warmth of the 20th century **may not necessarily** be the warmest time in Earth's history, what is unique is that the warmth is global and cannot be explained by natural forcing mechanisms."

From http://www.ncdc.noaa.gov/paleo/globalwarming/paleobefore.html

Also from the same website: "In summary, it appears that the 20th century, and in particular the late 20th century, is likely the warmest the Earth has seen in at least 1200 years."

From http://www.ncdc.noaa.gov/paleo/globalwarming/medieval.html

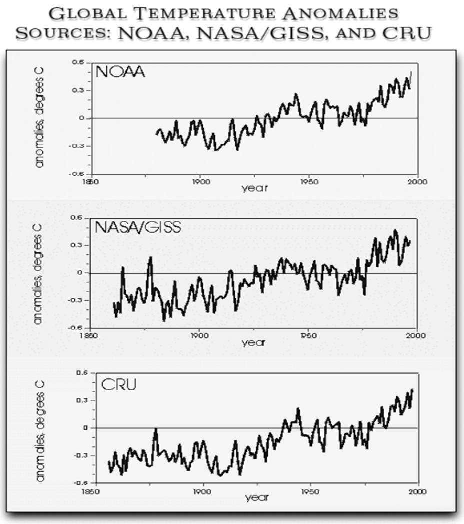


Figure 8

We do agree with Dr. Mann on one key point: that MBH98/99 were not the only evidence of global warming. As we said in our report, “In a real sense the paleoclimate results of MBH98/99 are essentially irrelevant to the consensus on climate change. The instrumented temperature record since 1850 clearly indicates an increase in temperature.” We certainly agree that modern global warming is real. We have never disputed this point. We think it is time to put the ‘hockey stick’ controversy behind us and move on.

I would like to make it clear that our role as statisticians in the hockey stick game is not as players in the hockey game, but as referees. What we have seen and continue to see is that, not withstanding the efforts by Dr. Nychka and others at NCAR, there is relatively little interaction between the statistical community and the climate science/meteorology communities although the latter frequently use statistical techniques. Statisticians in general have to pay their mortgages just like everyone else and in general cannot afford to do pro bono work such as we have been doing. We advocated in our report that if statistical methods are being used, then statisticians ought to be funded partners engaged in the research to insure as best we possibly can that the best quality science is being done. Drs. Nychka and Bloomfield, the statisticians involved with the NRC report, raise other issues on calibration, validation, and full quantification of uncertainty in these studies. Indeed there are a host of fundamental statistical questions that beg answers in understanding climate dynamics.

Sampling

How were the 70 trees in NOAMER 1400 selected?

4 Arkansas
4 Arizona
13 California
12 Colorado
3 Georgia
1 Louisiana
1 Montana

1 North Carolina
5 New Mexico
14 Nevada
3 Oregon
1 South Dakota
3 Utah
1 Virginia
4 Wyoming

How representative are these trees of the population of trees that grew from 1400-2000? In terms of geography, altitude, and type. If these trees seemed "interesting" to various individuals who took the core samples, do you believe those trees can/should be treated as a "random sample"? Are there biases in the selection of these trees? Presumably many trees could not be sampled because they had died or been harvested. What is the effect of this "censoring" on your data (and your analysis)? Similar questions exist about ice cores and how representative such data might be. What are the effects of gas diffusion in the ice core layers?

Analysis

What is the correlation between temperature and tree ring growth? What calibration studies have been performed? The rescaling steps taken seem to suggest that the correlation must be near 100%. Is that the case? The temperature proxy search is a regression problem. Why did you choose to use principal components (not appropriate for finding a nonstationary mean)? What weights do you use to combine different proxy types? Why? If the data are not a random sample, then what confidence can be given to any modeling and to any "error bars"?

Forecasting and Modeling

CO_2 modeling shows a rapid increase in the near term. What do the models show in the longer term? Given the apparent high correlation between CO_2 and temperature in the model outputs, how direct is the link in the model itself? What is the difference between a true forecast and a "model run"? Do you believe your model runs have any statistical validity? The output looks like a Taylor series with no higher order terms?

Planning Experiments

What data should be collected that would be most cost-effective in increasing our understanding of the climatic models and the underlying physics (and statistics)? Is all data valuable? How does one avoid the desire to collect data at sites that appear "interesting" beforehand? What are the parallels between modern experimental science and experimental medical research of the 1960's? How many surgeons were "certain" their treatments were superior or that drugs were safe and found out otherwise with carefully designed and controlled studies? Is the risk of global warming so acute that such studies are deemed unwise?

Our report is not aimed at criticizing Dr. Mann or his colleagues, but in outlining a path for doing the science better. We note that the American Meteorological Society has a Committee on Probability and Statistics. I believe it is amazing for a committee whose focus is on statistics and probability that of the nine members only two are also members of the American Statistical Association, the premier statistical association in the United States, and one of those is a recent Ph.D. with an assistant professor appointment in a medical school. The American Meteorological Association recently held the 18th Conference on Probability and Statistics in the Atmospheric Sciences (January, 2006). Of the 62 presenters at a conference with a focus on statistics and probability, only 8 (12.9%) are members of the American Statistical Association. I believe these two communities should be more engaged and if nothing else our report should highlight to both communities a need for additional cross-disciplinary ties.

MR. WHITFIELD. So at this time I will start off the questions, and I would direct my first question to Dr. Mann and Dr. Christy and Dr. Cicerone. If you look at the 1990 U.N. report from the Intergovernmental Panel on Climate Change, it is quite pronounced the so-called Medieval Warming Period. And so the first question I would ask was there a Medieval Warming Period, Dr. Mann?

DR. MANN. Let me tackle that first. Actually the graphic you are referring to in the 1990 report was not an actual numerical estimate. It was a schematic based on very limited evidence in some parts of the globe, and that was actually emphasized in the report that they based that schematic on very limited information. Another interesting thing about that plot is that it actually ends in 1975. Now there has been roughly .5 degrees C of additional warming in the climate in the Northern Hemisphere since 1975. And if you superimpose--

MR. WHITFIELD. How much since then?

DR. MANN. Point 5 degrees C additional warming since 1975.

MR. WHITFIELD. Point 5 degrees. Okay.

DR. MANN. Yes. So if you superimpose that on the end of that 1990 curve where it stops in 1975 actually the modern warmth is above the medieval peak. So it actually reinforces the later conclusions shown in the 1996 report and the 2001 report.

But we have learned a lot since then. For example, we know that the so-called Medieval Warm Period was actually fairly cold in the tropical Pacific. There is coral data that tell us that it was a La Nina like period. Now that means that there were large parts of the global surface that were cold at that time. As we learn more about the regional detail, we realize that it is incorrect to simply label that period as the Medieval Warm Period, and that is why most scientists now call it the Medieval Climate Anomaly.

MR. WHITFIELD. Okay. Dr. Christy, would you make comment about it?

DR. CHRISTY. Yes. Regarding the 1990 picture--

MR. WHITFIELD. The Medieval Warming Period.

DR. CHRISTY. Some places were obviously warm, other places weren't, and it is one that doesn't look like it has a warm period at that time but there were other places that were warmer than today, I think.

MR. WHITFIELD. Okay. And Dr. Cicerone.

DR. CICERONE. I have nothing to add. I went back and looked at the cartoon after last week's hearing and read all the surrounding pages and I have nothing to add.

MR. WHITFIELD. Okay. Now Mr.--

CHAIRMAN BARTON. Mr. Whitfield, could I--

MR. WHITFIELD. Yes, sir. Yes, sir.

CHAIRMAN BARTON. Why do we call that a cartoon and these others something different? I know the methodology is different but I would assume that the 1990 graphic was based on some mathematical evidence. It may not have been as complicated with as many variables as Dr. Mann's later work, but I don't think they just pulled that out of the air, did they?

DR. MANN. Let me comment. Actually it is a schematic. It is a cartoon. It was not a numerical estimate.

CHAIRMAN BARTON. They threw spaghetti up on the wall and wherever it stuck is what they put in the chart.

DR. MANN. Guided by some qualitative interpretations of historical climate records in a few locations in the Northern Hemisphere. It was not a quantitative estimate of climate.

CHAIRMAN BARTON. There is no averaging, there is no data to back it up?

DR. MANN. There is no numerical estimate that I am familiar with that went into that calculation that went into that graphic. There was no calculation.

DR. GULLEDGE. Mr. Barton, I have some--if you please. I actually spoke to some scientists who a couple have actually retired now who were involved in a 1975 NAS report on climate change that actually used a figure like this. And I spoke to Dr. Tom Webb who remembers the development of this figure and it actually originated from somebody's lecture notes at one time from the early '70s.

CHAIRMAN BARTON. There is no data set?

DR. GULLEDGE. That is correct. There is no data set that is used in the production of this plot. There were studies where they said it looks like the north Atlantic was warm. There are studies that say China was cold. You know, we are proposing that there may have been a warm period in the Middle Ages, and to quote from the 1990 IPCC report in reference to this figure it says specifically, "It is still not clear whether all the fluctuations indicated in the diagram were truly global." And that is directly from the report referring to this diagram.

CHAIRMAN BARTON. Thank you, Mr. Whitfield.

MR. WHITFIELD. Yes, sir. Just referring to Mr. Inslee's chart about CO_2 concentration levels and temperatures going back 400,000 years, it is constantly up and down, constantly up and down. Now is that something that we normally expect that CO_2 emissions constantly go up and down for 400,000 years? Would someone reply to that?

DR. CICERONE. May I respond?

MR. WHITFIELD. Yes, sir.

DR. CICERONE. The CO_2 data comes from extracting gas dissolved in ice as was explained last week.

MR. WHITFIELD. And where is the Vostock ice core, where is that?

DR. CICERONE. It is at a particular region in Antarctica where the ice is so thick that you can actually go back that many years and do reasonable dating. It doesn't mean that every year is exactly one year but it is pretty good resolution so they crush the ice or melt it. The problem with melting is some of the gas can dissolve in liquid so probably the safest technique is to crush the ice and extract the air. The CO_2 record is absolutely quantitative. It shows that through the last four ice ages if you go back to 650 or 700,000 years when the Earth was cold the CO_2 amounts were low.

When the Earth was warm in between the ice ages the CO_2 got higher, and the range was about 180 to 280 parts per million. Those are the natural cycles of the Earth. People have tried very hard to say did the CO_2 increase cause the warming or the cooling or did the warming and cooling cause the CO_2. The only evidence that seems clear is that there were times when the warming preceded the CO_2 and the cooling preceded the loss in CO_2 but they are nearly linked in time. So people are scratching their heads, what are the feedbacks that cause this? How did these ice ages start? What triggered them? How do we get out of them?

Methane amounts also track perfectly. When Earth was warm methane was two-thirds of a part per million. When it was cold it was one third of a part per million. Now we are at five-thirds of a part per million so we are out of that range. That is about all I can say. So the biological process that release CO_2 and methane were probably responsible.

MR. WHITFIELD. So it is continually going up and down. Would you anticipate that it would go down at some point in the future or do you feel like it is going to continue to go up?

DR. CICERONE. Well, the CO_2 that is in the air now is 385 parts per million, which is 200 parts per million larger than the 180 minimum at cold times and 100 larger than the CO_2 maximum at hot times. It is going to take 200 years for that CO_2--if we quit putting CO_2 in the atmosphere today and all the plants decomposed, it would take a couple hundred years for the CO_2 to fall back to that region. It is not going to happen.

MR. WHITFIELD. And what percent of all the CO_2 being emitted today would you say is man-made and what percent is natural?

DR. CICERONE. Well, the decay in biota and respiration and geological processes put 100 gigatons of CO_2 carbon in the air each year. Combustion of fossil fuels puts in 6 or 7. So the natural inputs are larger by far but the equilibrium of the system as established as Professor North mentioned last week is the processes that suck it up are about 100, so the imbalance is the 6 or 7 and about half of that shows up in the air and the other half seems to go in the oceans every year.

MR. WHITFIELD. But the natural emissions are overwhelmingly larger than man made but the man-made part is what messes up the equilibrium.

DR. CICERONE. Well, numerically they are overwhelmingly larger but the atmosphere seems to think otherwise because the atmosphere is responding to the increase.

MR. WHITFIELD. Right. Now, Dr. Christy, you have done some work on satellites, observations of the Earth's surface, and I read a book a number of years ago entitled "A Moment on the Earth" by a guy named Greg Easterbrook, and there was some part of that where he talked a lot about the satellites were not--the models being used to project global warming and the satellite observations were not in sync. I am sure I am not expressing it in the proper scientific way but hopefully you may know what I am referring to.

And I know that some of the work that you did, you received a lot of criticism or not criticism, but people were taking shots at you also because you had an error in your work relating to satellites and you were off like .035 percent of one degree or something. But would you elaborate a little bit about the satellite observations today and how that matches up with the global warming that we hear about from a scientific standpoint?

DR. CHRISTY. Yeah, it is curious. I have a couple papers coming out this year, in fact, in which we show that the evidence indicates the atmosphere is not warming as fast as it is typically thought from enhanced greenhouse gases particularly in the tropics, so that is the short answer.

MR. WHITFIELD. Okay. And these papers will be coming out when?

DR. CHRISTY. I turned the page proofs back for one yesterday so it is probably a couple months. The other one will probably be about 3 months.

MR. WHITFIELD. Okay. My time has expired. Mr. Stupak.

MR. STUPAK. Mr. Chairman, as a courtesy to Mr. Waxman I am going to yield my time to Mr. Waxman, and I will assume his time when his time comes.

MR. WAXMAN. Thank you very much, Mr. Stupak. Mr. Chairman, it is interesting that you are citing Gregg Easterbrook as someone who in the past had been a skeptic, and he recently wrote where he said "as an environmental commentator, I have a long record of opposing alarmism.

But based on the data I am now switching sides regarding global warming, from skeptic to convert."

MR. WHITFIELD. Well, I mentioned his name so you could bring that up, Mr. Waxman.

MR. WAXMAN. All right. Dr. Mann, your work was extensively criticized by Dr. Wegman last week. He criticized certain statistical aspects of your work and provided testimony on global warming more generally. However, Dr. Wegman isn't a climatologist, and I would like to give you the opportunity to respond to some of his statements from last week's hearing. He stated, "Carbon dioxide is heavier than air." And "if the carbon dioxide is close to the surface of the Earth it is not reflecting a lot of infrared back." Would you care to respond to that statement?

DR. MANN. Yes. It is a somewhat problematic statement on a couple levels. First of all, of course the greenhouse effect is not based on the reflection of radiation, it is based on the absorption of outgoing radiation. Rather than escaping to space it is radiated back towards the surface and the surface has to warm up in response to that. So reflections isn't involved at all. It is the process of absorption, selective absorption.

The other problem with that statement is that the well mixed atmospheric constituents, oxygen, nitrogen, carbon dioxide, their distribution, their vertical distribution in the atmosphere doesn't have to do with their weight or their relative masses. It just has to do with the basic force balances that act in the atmosphere. There is gravity and then there are gradients due to the pressure of the atmosphere and these two things have to balance out. And it turns out that all of the well-mixed gases decay with the same vertical profile falling to about one-third of their surface concentration at roughly eight kilometers up in the atmosphere. And that is true for CO_2 as well as oxygen.

MR. WAXMAN. I thought that at the time, and I am glad to hear your response because I knew there was something wrong with that statement. When Dr. Wegman was asked about your research since 1999 he stated that you had circled your wagons "and tried to defend this incorrect methodology." I would like to know if this is true. Did you continue to use the same methodology or have you worked to improve your approach since 1998?

DR. MANN. Thanks for the question. It is another troubling statement that you quote there because of course my collaborators and I have far from circling our wagons, we have been spearheading efforts to develop more sophisticated statistical methodologies for reconstructing climate and rigorously testing those methods using climate model simulation. We published a number of papers that show that the methods we used performed very well in the context of climate model simulations where we know the answer. We don't have to guess because we have the simulation. There were some other statements--

MR. WAXMAN. Well, let me asks you about some of the other statements because he attempted to impeach your statistical background by complaining that you used non-standard statistical phrases in your research like "statistical skill." Can you help us understand? Is this an unusual phrase as Dr. Wegman suggests?

DR. MANN. That was another very odd statement on his part, and I found his lack of familiarity with that term somewhat astonishing. The American Meteorological Society considers it such an important term in the context of statistical weather forecasting verification that they specifically define that term on their website and in their official literature. And in fact it is defined by the American Meteorological Society in the following manner: "A statistical evaluation of the accuracy of forecasts or the effectiveness of detection techniques." Several simple formulations are commonly used in meteorology. The skill score is useful for evaluating predictions of temperatures, pressures, et cetera, et cetera, so I was very surprised by that statement.

MR. WAXMAN. Dr. Wegman testified he thought global warming "is probably less urgent than some would have it be." He also discounted the impact of increasing the planet's temperature by 2 degrees testifying that he would "challenge anybody to go out and tell the difference between 72 and 74 degrees Fahrenheit." Dr. Mann, the impacts of climate changes are a well studied area. Does Dr. Wegman have any basis for being so cavalier about global warming?

DR. MANN. Well, just to provide some context. The difference between the height of the last glacial period when there was more than a kilometer of ice sitting above New York City global temperatures were probably only about 4 degrees colder than they are today so that gives you some idea of the dramatic nature of climate associated with fairly moderate changes in global mean temperature. Those changes in global mean temperature are often associated with much larger changes in certain very important regions like the Arctic where the warming over the last century is much greater than the global mean, and we have seen melting of perma frost and other impacts of that.

MR. WAXMAN. He also said that global warming "must be understood in the context which is that we have relatively speaking a Little Ice Age, which everybody seems to acknowledge, and so it is not so surprising that it is warming if we are coming out of a Little Ice Age." Does Dr. Wegman's statement accurately reflect the scientific consensus?

DR. MANN. No. In fact, the implications are just about the opposite of what he had stated. In fact, we know with the climate models that we have today that embody the basic physics of the atmosphere and the ocean and the interactions between them, actually we can describe, we can predict and explain the factors that underlied the Little Ice Age and the fact that certain regions like Europe cooled somewhat more dramatically than the rest of the globe some time between the 17th and 19th Century.

It turns out that that is the response of the climate to the changes in natural factors like explosive volcanic eruptions and small changes in solar radiants that were relevant to the past. Those same models that so successfully describe the Little Ice Age tell us that there is no way to explain the warming of the last century without the influence of human beings on concentration of greenhouse gases in the atmosphere.

MR. WAXMAN. So I shouldn't be nostalgic for that Little Ice Age.

DR. MANN. Perhaps not.

MR. WAXMAN. Did Dr. Wegman ever contact you to talk about your work or ask for any further explanation from you about it?

DR. MANN. No.

MR. WAXMAN. Some have criticized you for lack of willingness to disclose your data and computer code. Could you briefly tell us how you have handled the availability of your research?

DR. MANN. Well, first of all I would like to draw a distinction between data and code. The statement was made earlier here that I didn't make my data available until 2004, and that is simply incorrect. Our entire data set was available on the worldwide web several years before that. Now a code, well, that is a different sort of thing. It is a matter of intellectual property because it takes a lot of work to implement the algorithm that one might be using to perform a certain sort of operation, but as long as the algorithm is available then other people can independently reproduce your work without having the actual physical code.

And, in fact, that is what Dr. Wahl and Dr. Ammann have shown. They have independently implemented our algorithm in a different programming language that is available to anybody who wants to go to their website to access it. As a matter of fact, over the past few years we have been making all of our codes available for all of the calculations that we do, and that is actually a standard that many others in our community, the climate research community, haven't really followed, so we are sort of leading the way there.

MR. WAXMAN. Thank you. I want to ask Dr. Christy about this because you stated that you provided your computer code to other researchers when it has been requested, and you specifically mentioned providing your code to Remote Sensing Systems or RSS. Is that accurate?

DR. CHRISTY. We provide the part of the code that was in question.

MR. WAXMAN. Well, I contacted RSS about your testimony and Mr. Frank Wentz sent me a letter last night, and he wrote to say, "Dr. Christy has never been willing to share his computer code in a substantial way," and he provides the text of a 2002 e-mail exchange between RSS and yourself. And according to this letter when asked for your code, you replied "I don't see how sharing code would be helpful because there are at least seven programs that are executed (several thousands lines of code) and we would be forced to spend a considerable amount of time trying to explain coding issues of the spaghetti we wrote." In light of this letter, Dr. Christy, I would be interested if you care to clarify your testimony because Mr. Wentz wrote further, "I think the complexity issue was a red herring. My interpretation of Dr. Christy's response is he simply didn't want us looking over his shoulder, possibly discovering errors in his work. So we had to take a more tedious trial-and-error approach to uncovering the errors in his methods."

And then he went on to explain "RSS manages data software from a large array of climate satellites." What do you say about that? That sounds inconsistent with what you have told us.

DR. CHRISTY. We shared with them the parts of the code that they were most concerned about. What is called the drift effect was one of them. Because ours were machine dependent and so on like that but we did share not only that but we also shared the intermediate data to say, okay, if you implement this code this is the intermediate data you should get, and that is what they published.

MR. WAXMAN. I must say I am a politician as all the people here at our dais are and all of us engage in politics as we know it, but here is a session with scientists, and you went ahead and attacked Dr. Mann, who is an accomplished and respected climate researcher. I think you and Dr. Wegman attempted to smear his good name. Now I just got a letter from another person--

MR. WHITFIELD. The gentleman's time has expired.

MR. WAXMAN. --in your field who says that you haven't been forthcoming, so I just want to point out to all of you, we don't do the back biting as frequently as it seems to me that some of you scientists seem to do to each other.

MR. WHITFIELD. But Dr. Christy did say that he shared part of the code that he asked for.

DR. CHRISTY. Yes. They got what they wanted.

MR. WAXMAN. May I ask unanimous consent to put the letter from Dr. Wentz in the record?

MR. WHITFIELD. Without objection.

MR. STEARNS. I would object, Mr. Chairman.

MR. WHITFIELD. Okay. Objection.

MR. STEARNS. I object just because I think staff should have an opportunity to see the letter first.

MR. WHITFIELD. Okay.

MR. WAXMAN. I certainly would share it with staff. Assuming staff sees no objection from the letter that I received last night, I would like to--

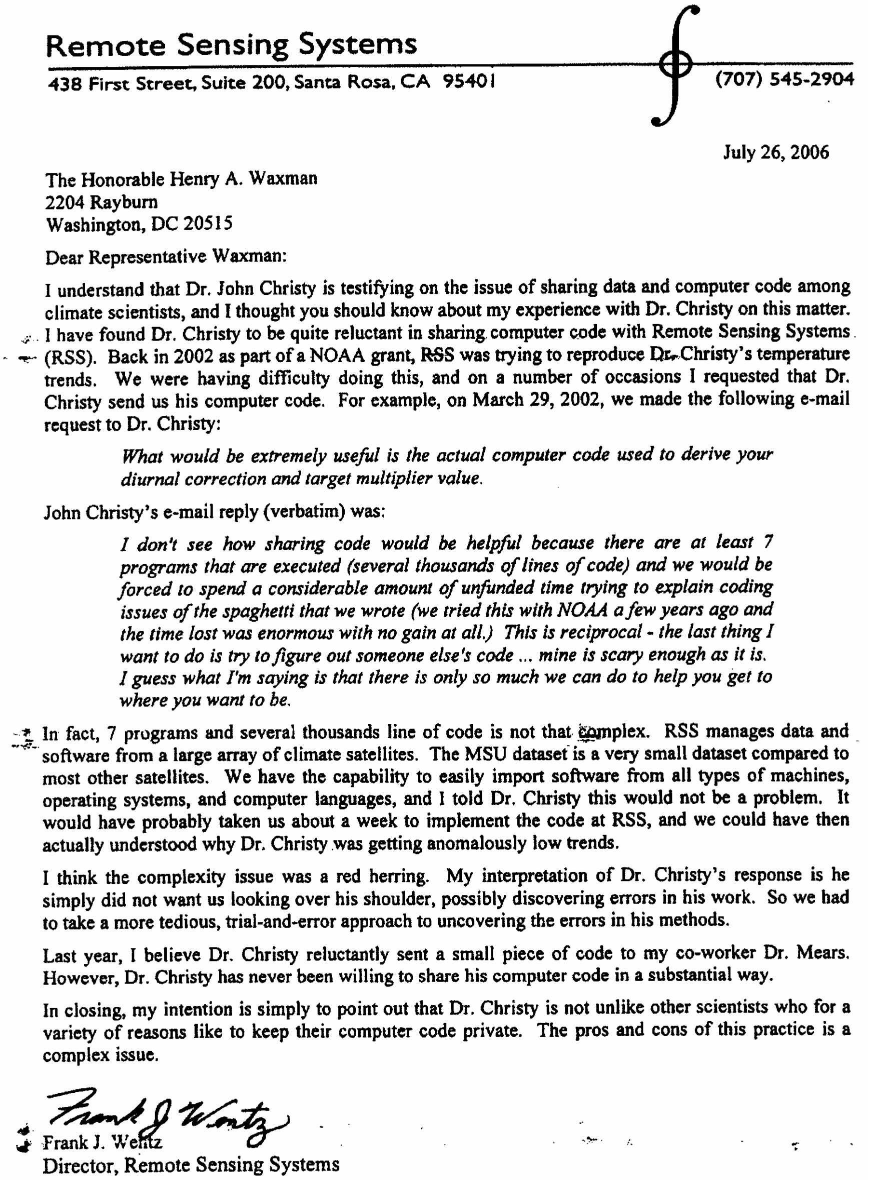
MR. STEARNS. Mr. Chairman, you remember last time that I asked a letter to be submitted to the record and they objected until they saw it--

MR. STUPAK. But we put the letter in.

MR. STEARNS. I know, but I produced a letter for the gentleman.

MR. WHITFIELD. If I could have order a minute. We will look at the letter. We will have staff look at the letter. In the meantime I recognize the Chairman of the full committee for 10 minutes.

[The information follows:]



Remote Sensing Systems
438 First Street, Suite 200, Santa Rosa, CA 95401 (707) 545-2904

July 26, 2006

The Honorable Henry A. Waxman
2204 Rayburn
Washington, DC 20515

Dear Representative Waxman:

I understand that Dr. John Christy is testifying on the issue of sharing data and computer code among climate scientists, and I thought you should know about my experience with Dr. Christy on this matter. I have found Dr. Christy to be quite reluctant in sharing computer code with Remote Sensing Systems (RSS). Back in 2002 as part of a NOAA grant, RSS was trying to reproduce Dr. Christy's temperature trends. We were having difficulty doing this, and on a number of occasions I requested that Dr. Christy send us his computer code. For example, on March 29, 2002, we made the following e-mail request to Dr. Christy:

> *What would be extremely useful is the actual computer code used to derive your diurnal correction and target multiplier value.*

John Christy's e-mail reply (verbatim) was:

> *I don't see how sharing code would be helpful because there are at least 7 programs that are executed (several thousands of lines of code) and we would be forced to spend a considerable amount of unfunded time trying to explain coding issues of the spaghetti that we wrote (we tried this with NOAA a few years ago and the time lost was enormous with no gain at all.) This is reciprocal - the last thing I want to do is try to figure out someone else's code ... mine is scary enough as it is. I guess what I'm saying is that there is only so much we can do to help you get to where you want to be.*

In fact, 7 programs and several thousands line of code is not that complex. RSS manages data and software from a large array of climate satellites. The MSU dataset is a very small dataset compared to most other satellites. We have the capability to easily import software from all types of machines, operating systems, and computer languages, and I told Dr. Christy this would not be a problem. It would have probably taken us about a week to implement the code at RSS, and we could have then actually understood why Dr. Christy was getting anomalously low trends.

I think the complexity issue was a red herring. My interpretation of Dr. Christy's response is he simply did not want us looking over his shoulder, possibly discovering errors in his work. So we had to take a more tedious, trial-and-error approach to uncovering the errors in his methods.

Last year, I believe Dr. Christy reluctantly sent a small piece of code to my co-worker Dr. Mears. However, Dr. Christy has never been willing to share his computer code in a substantial way.

In closing, my intention is simply to point out that Dr. Christy is not unlike other scientists who for a variety of reasons like to keep their computer code private. The pros and cons of this practice is a complex issue.

Frank J Wentz
Frank J. Wentz
Director, Remote Sensing Systems

CHAIRMAN BARTON. We are about truth, and my guess is Mr. Waxman's letter helps the truth so we will almost certainly put that in the record. Dr. Mann, I read your prepared testimony and I have listened to your synopsis, and you said something that I didn't see in the prepared testimony. Maybe it was there. You talked about scientists trying to make certain they don't make categorical statements. I don't know exactly but it sounded, to coin a phrase, plausible what you said. Now in our opening statements my friends on the other side, and they really are my friends. We get along a lot better off camera than we do on camera.

Their opening statements seem pretty categorical to me. Their minds seem to me to be pretty made up, that this is a major problem and it is time to stop foot dragging and let's get on and fix it. I don't quite have that religion yet. I haven't been born again quite yet. And that is what this is all about. If in fact all these things that my friends, Mr. Inslee and Ms. Schakowsky and Mr. Waxman, believe so fervently are literally factually true without question then we need to move to problem solution.

But I look at these data sets, I look at these data points, I look at these theories and things, and I see a sign curve phenomenon where the Earth gets warmer, the Earth gets cooler, the Earth gets warmer the Earth gets cooler. It certainly appears that it is getting warmer faster in this century. It is certainly plausible that it has got to be partially caused by man-made emissions. But I think it is a little early to categorically make some of the statements that my friends on the Minority side are making.

And the reason that we asked you to try to provide your data sets and your codes and stuff is because yours was the very first one and it is referred to. Now there may be a hundred since then and maybe we ought to look at all hundred of them, but yours is the one even in the National--the science review--Research Council review. It talks about that in the executive summary. So do you feel--from everything I can find out about you is that you are a very fine person and an excellent scientist and totally dedicated to your work, but do you think it is fair to ask you to try to let other people verify that first study since it seemed to have such an impact on the community?

DR. MANN. Well, no, I don't think it is unfair at all to expect the scientific community to validate previous results and to refine them, and that process has been occurring over the past 10 years since our work was begun. I think the National Academy members at their press conference said something to the effect that they felt that the scientific process had worked quite well in this area in that methods have been refined, new proxy data have become available. Multiple estimates are now available where there were three at the time of the IPCC 2001 report. There are now more than a dozen different estimates. There are also independent model simulations--

CHAIRMAN BARTON. You don't think it is unfair to have a little scrutiny to the--if it is a conclusion, anybody has a right to a conclusion and an opinion but when it gets into the mainstream that it is just a given that is what I take a little exception to even today. Now I want to ask a follow-up question. Dr. Wegman said when he tried to get enough information to try to verify the model, verify the algorithm, he says he had some trouble getting that. Now you talked about codes and algorithms. What is the difference, and I am not a statistician and I am not a climatologist or a paleoclimatologist. What is the difference between a code and an algorithm?

DR. MANN. Okay. Let me try to use an analogy.

CHAIRMAN BARTON. Use a simple one.

DR. MANN. I will do my best.

CHAIRMAN BARTON. The simpler, the better.

DR. MANN. Well, let's think of an algorithm is--suppose you were trying to build a house. And you wanted to build a house and the data would be the materials you need to build the house, the nails, the wood, et cetera. The algorithm would be the architectural plan. Now what would the code be? Well, imagine that instead of builders you had a computer to make your house for you. Well, the code would be implementing the architectural plan by telling the computer to pick up the hammer, pick up the nail, hammer it in. And so the code is simply implementing the algorithm but the real scientific process is embodied within the algorithm, and the algorithm is what has been independently reproduced.

CHAIRMAN BARTON. What is proprietary about a formula or mathematical model that tries to compute something as gargantuan as world climate over 2,000 years? I don't see anybody making any money on that. I mean if you put it out there and said this is what is happening and try to predict the future, why should that not all be made available in some public way that independent reviewers can try to replicate it?

DR. MANN. Well, let me preface this by putting out that we now as a matter of course do make available our codes that we have written to implement these different methods and so the Rutherford et al. paper that was shown earlier reproduces essentially the original reconstruction, that entire code can be downloaded from our website.

CHAIRMAN BARTON. If we asked, which we are going to, asked Dr. Cicerone--we are going to ask him to review some of these recommendations that Mr. McIntyre and Dr. Wegman and others have made, but one of them is going to be that because the stakes are so big and the consequences are so big that these models and data sets and things be made available in some way that they can be verified. Do you have a problem with that?

DR. MANN. No. I think this is a bigger question than one that should be asked of me. There are bigger questions about intellectual property rights, and people--the scientific community and the policy makers need to work that out, what is the balance between making sure that scientists are allowed to write a code, spend a whole lot of time doing it and be able to implement it and use it without immediately having to turn it over to somebody else who suddenly then gets all of their intellectual contributions over a several year period. So I think there is a balance there. I don't disagree with the premise of what you are saying.

And I think there is the issue that Dr. Christy brought up earlier, if you take, for example, our 1998 work, well, that was a program, I think you alluded to this last week, it was written in Fortran and a fairly--

CHAIRMAN BARTON. I was stunned to know that that program was still in existence.

DR. MANN. It is still more widespread than you might think actually.

CHAIRMAN BARTON. What generation is it now because I was up to Fortran 4.

DR. MANN. It was 90 and then--and we were back in F77, Fortran 77 is what we wrote this program in. So there is the issue of platform dependence. And now we are getting away from that. For example, we write all of our codes now in MAT Lab, which is a portable programming language and anybody who has MAT Lab can implement it. And that is the direction things are moving but to apply the standard to work that was done 10 years ago may be unfair because the standards have changed.

CHAIRMAN BARTON. Dr. Christy, I read your testimony, and I want to compliment you on its preparation and your forthrightness. On page 11 you talk about, in the second paragraph, that the issue of climate model evaluation has been performed mostly by the modelers themselves. It is my view, this is you speaking here, and recommendation that policy makers would learn much from independent hard-nosed assessments of these model simulations by those who are not directly vested in the outcome. Some of this is going on but the level of support is minimal. Do you still stand by that?

DR. CHRISTY. Yes, I do. I think probably any scientific endeavor could stand with independent eyes looking over it.

CHAIRMAN BARTON. Dr. Cicerone, do you agree with that statement?

DR. CICERONE. The more the merrier. I have done a lot of mathematical modeling maybe 15 or 20 years ago, and I remember efforts to try to compare models where unfortunately what happened was everybody said, well, let's put the same assumptions in the models and see how they do. And I think it worked against the science because it created less independence. So to do this kind of exercise I think we have to take everything into account but generally it is a good idea in my opinion.

CHAIRMAN BARTON. I know I am over time. I want to read one more paragraph in Dr. Christy's testimony because it kind of encapsulates the policy dilemma that we are faced with and ask the panel to comment on it. And I am quoting, "To understand the scale of what we are dealing with this serves as a rough example. We know that we on Earth benefit from 10 terawatts of energy production today. To achieve a reduction of the CO_2 representing 10 percent, 1 terawatt, of that production we need 1,000 nuclear power plants at 1,000 megawatts each. Massive implementation of wind and solar does not achieve this result and would not provide the baseload power needed by the economies today in any case. Thus, to have a 10 percent impact on emissions from energy, that is growing at the same time, will require a tremendous and difficult and expensive restructuring of energy supplies."

So even if we accept the problem and move to solution to get a 10 percent reduction in CO_2 takes 1,000 megawatt nuclear power plants and it probably doesn't have any impact for 50 to 100 years. Do you all want to comment on that, anybody, other than Dr. Christy, which you can. It is your statement.

DR. CHRISTY. I would just say the energy committee is where a lot of this is going to be done and that is just to give you an idea of the scale of what you are going to be tackling, I think, in the next few years.

CHAIRMAN BARTON. That is why I am still a skeptic. I don't want to jump in there especially if this is a naturally reoccurring phenomenon that is exacerbated by human emissions but it is going to happen regardless of what we do. Dr. Cicerone.

DR. CICERONE. The numbers that you just summarized from Dr. Christy are really intimidating. I agree with you. I would like to see us all get together with the elements of a win-win strategy. There are some actions we can take as first steps, I think, which are truly win-win, and they have to do with energy efficiency. Just look at it from the United States point of view. If we could decrease our dependence on foreign energy we would improve national security, we would decrease the trade deficit, we would, I think, stabilize geo politics a little, we would increase national competitiveness by making our manufactured products cheaper.

When energy prices are high you know better than I our manufactured products have to bear that increase. We could develop new products which would create new world markets and we could be leaders. We would decrease the energy costs for households and incidentally slow down the emissions of CO_2. So I think we need a win-win strategy and we can take a bite out of that 1,000 gigawatts with energy efficiency.

CHAIRMAN BARTON. My time has more than expired so I apologize. Thank you all for being here.

MR. WHITFIELD. I recognize Mr. Stupak of Michigan.

MR. STUPAK. Thank you. We were talking about that Fortran 4 program, and I was just wondering was that during the Medieval Warming Period we have been talking about? If I may, Mr. Chairman, when I gave my opening statement I had a couple of exhibits. I should have asked at that time that they be made part of the record with my opening statement. It is the Interfaith Stewardship Alliance that I mentioned and how they were funded by ExxonMobil, so if I may without objection put that as part of my opening statement.

MR. WHITFIELD. And we have a copy of it.

MR. STUPAK. Yes.

MR. WHITFIELD. Thank you.

MR. STUPAK. Dr. Cicerone, just speaking of the Medieval Warming Period as it was described in the *Wall Street Journal*. We talked a little bit about it earlier. In fact, are we even sure that even happened in the Northern Hemisphere, that Medieval Warming Period that the *Wall Street Journal* talked about, that was that chart there, the 1990 chart that we had some discussion about earlier.

DR. CICERONE. I am sorry. Were you addressing that to me, sir?

MR. STUPAK. Yes, sir.

DR. CICERONE. Okay. There were certainly records of warm places in that period of time.

MR. STUPAK. Warm places and cold places.

DR. CICERONE. The question continues to be how extensive was it, how long did it last, and how solid is the evidence. But, yes, there is evidence of a Medieval Warm Period, but no one can sit here and tell you how geographically extensive it was with strong evidence and how long it lasted. But, yes, there was a Medieval Warm Period.

MR. STUPAK. Dr. Crowley says that even though it was difficult to unequivocally assert that the current warming period is significantly greater than the peak warmth of the Medieval Warm Period there is even less justification for saying that the medieval period was warmer than it is today, is that correct?

DR. CICERONE. The committee that Professor North reported on, Professor North from Texas A&M, last week representing the National Research Council, I am pretty sure what they concluded was there was no evidence that that period was warmer than say the year--the decade of the 1990s through 2006.

MR. STUPAK. But were considerably warmer?

DR. CICERONE. They could not say with strong evidence that each year in the 1990s was warmer than then but there was no evidence that the Medieval Warm Period over an extensive geographical region was as warm as the Northern Hemisphere is now.

MR. STUPAK. Is it fair to say then that neither the pro-hockey stick researchers or the anti-hockey stick researchers can talk with scientific certainty about this medieval period, would that be correct?

DR. CICERONE. In certain locations they can where there were records kept, but the question again is how does one location compare with all the others. For example, some proxy indicators from China inferred what the temperatures were from agricultural crops and stream flows and so forth, which are pretty extensive, but it is hard to compare the timing of those with other strong proxies from elsewhere.

MR. STUPAK. Let me ask you this question then. This is the second hearing we have had on this hockey stick theory, but you were on the National Academy of Science panel that looked at these studies. Are you telling us basically forget the hockey stick and the Medieval Warm Period, it is a diversion? Is it your position that global warming is occurring now in the 20th and 21st Century? Human beings are at least partially responsible. Our climate will continue to change during the next century and we ought to pay attention to it today. Is that fair to say?

DR. CICERONE. Well, I wouldn't say forget the hockey stick and efforts to reconstruct because what we can learn, and if we work harder we might be able to learn some more about the context, it is still important, but, yes, all the other evidence shows us that the climate is changing and that the human hand is there causing at least part of the warming and that everything we know from physics and chemistry and mathematics is that it is going to continue as long as we continue to load up the atmosphere with greenhouse gases.

MR. STUPAK. Do you think it is useful then, Doctor, for us policy makers to hold hearings on just one 8-year old study, Dr. Mann's study, that your committee found was not even the principal evidence for the conclusion about current warming period?

DR. CICERONE. I hope that it has been useful. I have never seen this kind of interest before. I think a couple--I have forgotten who said it earlier on about that this could be--perhaps it was Mr. Bass, who said this could be the beginning of even more serious interest. So I guess I will wait and see what happens.

MR. STUPAK. If there is so much interest in this one and if in the Vice President's book he talks about 928 more peer reviewed articles, so that means if we have two hearings for every one of these we would have about 1,800 hearings just on global warming. I guess that would be a sufficient amount to get everyone's attention. Let me ask this question if I can. There has been a lot of discussion about social networking, and I think it is a practice that is not utilized, should not be utilized. Peer review and whether it is an accepted practice, isn't it, in paleoclimatology field, social networking, Dr. Cicerone?

DR. CICERONE. No. No, that was I guess kind of an original piece work. It is not common.

MR. STUPAK. Have you looked at or have you reviewed Dr. Wegman's social network analysis of the paleoclimatology field?

DR. CICERONE. Last week at the time of the hearing I got a copy and I read it.

MR. STUPAK. I know that the National Academy of Science has done research of social networking analysis. Do you have any views you would care to share with us about the field of research?

DR. CICERONE. Not today. I think there is probably some developments that have taken place in the classified arena that I am not totally up on that I would like to find out earlier before I would comment.

MR. STUPAK. Is it a relatively new field?

DR. CICERONE. Graph theory, the kind of statistical patterns, I think so. I haven't seen it applied to this kind of a field of study before.

MR. STUPAK. What do you think of Dr. Mann's social network analysis of the paleoclimatology field? Dr. Wegman's. I am sorry. I said Dr. Mann's. Dr. Wegman's.

DR. CICERONE. I have no further comment.

MR. STUPAK. Okay. Dr. Wegman, in looking at your report here today and your testimony, I am on page 6, if you would, sir, and I am looking at the paleo perspective on global warming. And you say these are contradictory statements, and I guess I am a little confused on it and maybe you could help us out. It says the latest--and I am quoting the first here, the first paragraph on page six. You got it there?

DR. WEGMAN. Yes.

MR. STUPAK. "The latest peer-reviewed paleoclimatic studies appear to confirm that the global warmth of the 20th Century may not necessarily be the warmest time in Earth's history, what is unique is that the warmth is global and cannot be explained by natural forcing mechanisms." And it says also from the same website, and this is a NOAA website, "In summary, it appears that the 20th Century, and in particular the late 20th Century, is likely the warmest the Earth has seen in at least 1,200 years." How is that inconsistent? You said contradictory statements. How is that contradicting?

DR. WEGMAN. Well, at one stage people are suggesting that it is the warmest and another stage it is saying it not necessarily the warmest. Being likely is a phrase that has been bandied about quite a bit.

MR. STUPAK. But aren't those really different time frames? One is talking about 1,200 years, the other one is talking about the 20th Century and Earth's history, it seems like, because one says the 20th Century and particularly the late 20th Century is likely the warmest, and the other one is talking about the earth's history. So that is why I didn't see it as inconsistent. One is talking about 20th Century, late 20th Century, and the other one is talking about all of Earth's history, so that is why I didn't see the inconsistency. Do you see what I am saying, those two statements?

DR. WEGMAN. Yes, sir, I see what you are saying.

MR. STUPAK. Dr. Mann, if I may ask you a question. I want to go back to this social network. Dr. Wegman has hypothesized that you have a social network of 42 other scientists and that they cannot independently evaluate your work because they have at various times co-authored work with you. This may be based on his belief that people who interact regularly will foster a common attitude or identity. What is your response to that?

DR. MANN. Well, frankly, I was a bit baffled by that finding. My profession is highly competitive. We often disagree publicly. Scientists disagree publicly and in our articles, with each other on certain matters, and yet we can co-author on other areas where we agree so there is no contradiction in--

MR. STUPAK. Well, do you have peer review of your articles by people who don't agree with you?

DR. MANN. I have probably had articles rejected because of reviews by people who were co-authors with me on other articles. In fact, I am quite certain that is the case. Of course, Dr. Christy and I are co-authors and yet there are a lot of issues in the science that we don't agree on. So I was very surprised by that. I was flattered by that. The implication that as a post doc when I started this work back in the late 1990s that I was sort of the center of the entire field of climate research but it is as incorrect as it is flattering.

MR. STUPAK. You don't dominate the thinking of the entire paleoclimatology community, do you?

DR. MANN. Well, I don't know if I do now but I am sure I didn't back in the late 1990s.

MR. STUPAK. My time is up. Thank you, Mr. Chairman.

MR. WHITFIELD. Thank you. At this time I recognize Mr. Stearns of Florida.

MR. STEARNS. Thank you, Mr. Chairman. Mr. McIntyre, you are the only one who doesn't have a Ph.D. here on the table so I thought I would ask you this question. As I understand your background, your undergraduate degree is mathematics. Is that from Oxford?

MR. MCINTYRE. My degree in mathematics was from the University of Toronto but I attended Oxford subsequently. I think my stay there probably overlapped with that of President Clinton's.

MR. STEARNS. Oh, good.

MR. MCINTYRE. I think we might have played rugby against one another.

CHAIRMAN BARTON. We hope you did a little more studying than he did.

MR. STEARNS. Well, you know, I just want to give you your due here. We have heard in testimony that Drs. Wahl and Ammann have reproduced Dr. Mann's work and shown your criticism to be invalid, and I guess--is this true and were your criticism erroneous? I will give you an opportunity to respond to that.

MR. MCINTYRE. Well, a couple of points. First of all, the code that we used to emulate Dr. Mann's work reconciles almost exactly with that of Wahl and Ammann. And so any conclusions that differ are not because of differences in how we have emulated the reconstruction. They think that certain steps are fine, we don't. They have in my opinion not carefully considered the implication of bristlecones. Our codes reconcile so right now I am confident in our conclusions that if you remove the bristlecones you have a major impact on the final results.

Last December, I met with Ammann in San Francisco and suggested to him that since our codes reconciled so closely that it would make sense if we co-authored a paper in which we set down the points that we agreed on, set down the points we disagreed on in an objective way so that we didn't seem to be launching missiles at one another and creating more controversy. I said that we could declare an armistice for 6 weeks until we accomplished this, and if we didn't get to conclusion everybody would go back to square one and that each of us could write separate appendices, say where we disagreed.

I formally sent e-mails to him suggesting that. He told me in San Francisco that if he did that that that would interfere with his career advancement.

MR. STEARNS. Dr. Wegman, I am going to give you an opportunity to respond to some of the testimony today. The testimony of both Dr. Gulledge and Dr. Mann draw upon the findings of Dr. Wahl and Ammann to suggest your work doesn't matter. Let me give you an opportunity to respond to that.

DR. WEGMAN. Well, I think although the social network analysis has been sort of dismissed the amazing thing to me is that these supposed independent replications of the original Mann work are done by Rutherford et al., which includes the top seven people in the social network that we identified last week. Every one of them is in there, and they are frequent co-authors with Dr. Mann. So I can hardly see how that is an independent replication of his original work.

Secondly, on Dr. Mann's résumé he lists Dr. Ammann as one of his students as a co-advisor to him although Dr. Ammann does not list him as an advisor. But it is clear to me that Wahl and Ammann are not independent agents as well.

MR. STEARNS. And that goes to this idea of the social network you are talking about?

DR. WEGMAN. Yes. We never claimed, by the way, that Dr. Mann was, in 1998 as a post doc was the center of the social network. What we are saying is that subsequently he has 42 co-authors many of whom, particularly the top seven in the block we identified, who are frequent co-authors with him and co-authors with each other, and there is some element of thinking that if they are frequent co-authors they are thinking the same way.

MR. STEARNS. Is there anything else that you have heard Dr. Mann say earlier that you would like to comment on? You are welcome to go across the spectrum.

DR. WEGMAN. Well, first of all, in the question that Mr. Waxman mentioned about the carbon dioxide distribution, that was prefaced by a comment by me that I didn't know anything about this but I suppose, for example, that carbon dioxide, so that was purely a hypothetical conjecture which I did not mean to be taken as testimony. It was also clear in the discussion that even Dr. North talked about a barrier of carbon dioxide at high levels of the atmosphere so he gave in his diagram an illustration that carbon dioxide was not mixed so that certainly is something that should be clarified. I did not mean to testify that carbon dioxide sat at the ground level. That certainly was not what I was saying.

MR. STEARNS. Any other thing that has come up that you wish to comment on either that Dr. Mann or others have spoken on or perhaps we as members have spoken on you would like to--

DR. WEGMAN. Well, I stand by the statements that I have made and particularly in the written testimony that I didn't get a chance to comment on. My own sense is that if you look at, for example, this matter of statistical skill, it doesn't matter that the American Meteorological Society says what statistical skill is. Statisticians do not recognize that term. I went around to a whole dozen or so of my statisticians network and asked them if they knew what they were talking about. It is my contention that there is a gulf between the meteorological community and the statistical community.

We examined, for example, this committee that is on probability and statistics of the American Meteorological Society. We found only two of the nine people in that committee are actually members of the American Statistical Association, and in fact one of those people is an assistant professor in the medical school whose specialty is bio-statistics. The assertion I have been making is that although this community, the meteorological community in general and the paleoclimate community in particular, used statistical methods. They are substantially isolated. They are using our methods but not talking to us. In contrast, we are not doing meteorology and--

MR. STEARNS. You are talking to them.

DR. WEGMAN. We are talking to them.

MR. STEARNS. I understand.

CHAIRMAN BARTON. Would the gentleman yield just for clarification, please? Dr. Mann in his testimony referred to this Dr. Ammann and Wahl study who said they have recentered the data and the conclusion is the same if I understood him correctly. Could you comment on that because one of your points was when you center it correctly the conclusions don't follow.

DR. WEGMAN. The studies are done in different ways. There is the so-called CFR methodology, the CPS methodology, and in I believe it was Dr. Mann's 2005 report he illustrates several different studies that do this. One of the things that is critical is the set of proxy data that you use when you are trying to replicate these studies. And in fact if you use a nice set of proxies that all have the same signal in them then it really doesn't matter a whole lot what methodology you use. If you use a very mixed set of proxies that have some noise and different kinds of structure in it then it does matter what kind of--

CHAIRMAN BARTON. It goes to Mr. McIntyre's point that depending on the data set you use it is the result you are going to get.

DR. WEGMAN. That is right.

CHAIRMAN BARTON. If I understood him correctly.

MR. STEARNS. Reclaiming my time. Mr. Christy, Dr. Christy, have you read Dr. Wegman's report, and, if so, what is your opinion of his working conclusion? I understand you are one of the individuals that was in the group that developed the National Research Council on surface temperature reconstruction of the last 2,000 years, so I would appreciate, Dr. Christy, your comment.

DR. CHRISTY. This is the short answer. I have not read the report.

MR. STEARNS. You have not read the report?

DR. CHRISTY. No, I am sorry.

MR. STEARNS. Okay. Dr. Cicerone, you are the President of the National Academy of Science. Dr. Wegman is an appointed member of the National Academy of Science Board of Mathematical Sciences and Their Application. He is chair of the NAS Committee on Applied and Theoretical Statistics, highly credentialed in math and statistics, wouldn't you say? Shouldn't we take his judgments on statistical matters very seriously, and don't they carry significant weight? Would you say his judgment about statistical matters is important and that he has credibility based upon those credentials?

DR. CICERONE. Yes.

MR. STEARNS. So there is some attempt by some folks to make some of his findings not correct but based upon what you just said this man is highly credible in math and statistics and we should take his judgment particularly on statistical matters with a high credibility?

DR. CICERONE. Yes, but not on the mixing of gases.

MR. STEARNS. Not on the mixing of gases. All right. Thank you, Mr. Chairman.

MR. WHITFIELD. The chair recognizes Mr. Inslee. Oh, no, Ms. Schakowsky. I am sorry. Ms. Schakowsky.

MS. SCHAKOWSKY. Thank you, Mr. Chairman. I want to begin by referring to the end of Dr. Christy's testimony where you drew on a certain kind of expertise where you were a missionary in Africa, and you end with a plea. And I just want to quote from the testimony. It says, speaking of the people in Africa you say, "They are far more vulnerable to the impacts of poverty, water, and air pollution, and political strife than whatever the climate does." I actually found that to be a pretty strange comment from someone who is the chair of the Earth System Science Center and deal with climate.

And I wanted to actually ask Dr. Cicerone don't those issues of certainly of water and air pollution, et cetera, are they unrelated entirely to issues of climate?

DR. CICERONE. No. Of course they are related. I don't know what Dr. Christy would answer to the question of what he meant, but, yes, it is clearly related.

MS. SCHAKOWSKY. So I was really confused by that because, first of all, I have to tell you I resented that a little bit. I close with a plea to remember the needs and aspirations of the poorest amongst us when energy policy is made as if to say that those of us who would ask for some changes in business as usual and energy as usual somehow are not taking into consideration the poor people of Africa. So I found that a condescending remark, I have to tell you.

But are not those things--because I have to tell you, Dr. Christy, that precisely for the reasons of the kind of impact it will have on human life including drought and exacerbating poverty and even you mention political strife, war water actually do worry me a bit. So how do you segregate that from climate issues?

DR. CHRISTY. Was that water wars?

MS. SCHAKOWSKY. You said political strife. I would say that if we end up with a situation where people are fighting over water or limited food supplies because of drought that that could be related to the climate, could it not?

DR. CHRISTY. We don't know what is going to happen, for example, with the water cycle as the climate evolves so--

MS. SCHAKOWSKY. The overall statement about water and air pollution, are they unrelated to climate?

DR. CHRISTY. We know today that people die because of water pollution, air pollution and those other things. Those are issues that we know today and can assess and determine how answers and solutions can be found. So those are critical things to do today. And I am sorry if that last line came across condescendingly. When you live with the people as I did you know that they don't have much of an advocacy in places.

MS. SCHAKOWSKY. Actually in the Congress they do have a number of people who care and advocate on their behalf. I wanted to get to that and it is a perfect lead into the Chairman's question, and what do we know, this was his question, and so I wanted to look at Dr. Gulledge's materials that he provided. And again I would like to ask him or anyone, it says in your presentation, Dr. Gulledge, human activities are increasing atmospheric greenhouse gases, that that is unequivocally agreed to in the scientific community. The Earth is warming unequivocal at an unprecedented rate, confident, so somewhat less.

Warming over past five decades caused primarily by man-made greenhouse gases, confident. So let me add one more preface to this question that I would like to put to the panel, first of all, the question of agreement. We have a panel here where it is three and three, so if there is a reporter looking at this they would say, well, there is three people who agree with this, three that don't, so there is a split here. So part of my question is does the disagreement over your unequivocal, unequivocal, confident on this panel reflect the scientific community in any kind of accuracy. And I would like to just question these unequivocal and confident ratings.

DR. GULLEDGE. Well, I am not sure if you are describing the panel as being three against three on whether they agree with these statements or not, but I suspect that it might not fall out exactly that way.

MS. SCHAKOWSKY. Okay.

DR. GULLEDGE. It might vary among some of the lower statements and then--

MS. SCHAKOWSKY. Then let me ask this, let me ask the panel. Is there anyone who disagrees with the unequivocal--that it is unequivocal that human activities are increasing atmospheric greenhouse gases? Is there anybody? Okay, good. That the Earth is warming? Okay. And at an unprecedented rate?

DR. CHRISTY. What is the confidence level on that?

MS. SCHAKOWSKY. Confident.

MR. MCINTYRE. I don't know that it is unprecedented.

MS. SCHAKOWSKY. Actually I wanted to ask you--I hope you don't think this sounds rude but when I looked at the witness list I see, you know, everyone has got kind of a credential and then it just says your name, so I wanted to ask you about your credentials, Mr. McIntyre, and perhaps it gets into social networks because when I asked for your resume what I found was: for the last 16 years I have been an officer and director of several small public mineral exploration companies, previous to that I worked for a large international mining company, and that mainly it is your experience in mineral exploration industry that you tout in your resume and your background. I don't know if that gets to social networks or not.

MR. MCINTYRE. Well, in this case this has nothing to do with any work that I have ever done. I just became interested in it as a citizen when I read the studies, and I thought that politicians were facing difficult policy decisions so I thought that it would be interesting to examine one particular paper which was being cited by the Canadian government. It wasn't clear to me how people knew that 1998 was the warmest year in the millennium, and I was just interested in how--

MS. SCHAKOWSKY. So are you qualified to make a judgment on whether or not the Earth is warming at an unprecedented rate?

MR. MCINTYRE. For the things that I have published on, my statistical and mathematical skills are adequate for what I have published on. The findings that we have had about principal components have been--

MS. SCHAKOWSKY. But are you qualified to comment on whether or not the Earth is warming at an unprecedented rate?

MR. MCINTYRE. Well, you asked whether the people knew or didn't know. I am just saying I didn't know.

CHAIRMAN BARTON. Will the gentlelady yield?

MS. SCHAKOWSKY. Yes.

CHAIRMAN BARTON. That group is much more qualified than I am to comment on these things, and yet I have the responsibility as Chairman of the committee to put the bill together to change the way Americans work every day if we decide to do something about it.

MS. SCHAKOWSKY. Absolutely. Absolutely.

CHAIRMAN BARTON. The least qualified--I will stipulate--

MS. SCHAKOWSKY. And me too. I am with you.

CHAIRMAN BARTON. The quality of the commenters is more on that side of the dais than at least it is in the Chairman's chair. I am not going to comment on anybody else's qualifications but in a democracy anybody with an opinion is entitled to express that opinion and some are more qualified than others obviously because of their credentials, but I don't think we have a standard of witnesses that says unless you have a Ph.D. you cannot testify before--

MS. SCHAKOWSKY. Well, actually we are having a--reclaiming my time. Actually we are having a conversation today about the science here so it is not just about opinion, and it is relevant, I think, to talk about. And Dr. Wegman has been pretty up front about what he is qualified to testify to and what he is not, and I think that that is fair and it is fair to ask for individual's backgrounds and what their connections or interests might be. That is the kind of conversation that we are having.

But what I really wanted to get to was your question about what is it that we know, and if there is pretty wide agreement or no comment because you don't know that human activity is increasing atmospheric greenhouse gases and that the Earth is warming and that it has certain consequences. Mr. Chairman, when you said that you are a skeptic the difficulty of the task at hand to me is not a reason to be a skeptic about the science.

Admittedly, this is a daunting task, and we heard about the 1,000 nuclear power plants or whatever it could take, but we also heard practical suggestions from Dr. Cicerone about energy efficiency that we could make a start on this. And so if there is widespread agreement that human activity is contributing to this that this climate--that the warming of the climate is happening, that it can have very detrimental effects. I am anxious to understand why we don't just move toward solution at this point, and that is what I really was getting to so I have over stepped my time, and I thank you, Mr. Chairman.

MR. WHITFIELD. Thank you. And at this time I will recognize Mrs. Blackburn of Tennessee.

MRS. BLACKBURN. Thank you, Mr. Chairman, and you all are very patient with us. As I said in my opening remarks, I think a lot of this is born out of curiosity of knowing what the truth is and being able to have some answers. And I will tell you one of the reasons I have such an interest in this. I have a mom who is 81 years old who has been very involved in conservation efforts all of her life. She won the Keep America Beautiful Lifetime Achievement Award here about 15 years ago, and she is very careful in her instruction to her children and her grandchildren that one of the things we have to be very careful about is environmental extremism which many times hurts our argument for actually being good conservationists and leaving this Earth in better shape than we have found it.

And so when we have studies that seem to go around the horn and then they can't be substantiated and they are coming out as government proof as something it does cause us questions. And as I mentioned, we have been through this thing and we have talked about it and I have talked about how when I was growing up in the 60s that, the thing was that it was going--we were going to be in an ice age or have a return of the ice age. And then I guess that there were some schooled scientists, if you will, some of your colleagues maybe who found that that was not going to be so.

So I think it is important that we have the opportunity to visit with you and find out what is an item of agreement and what is not an item of agreement. And, Dr. Mann, if I could have your attention for just a few moments if you don't mind, I would like to direct my question, my opening question, to you. You have said that other studies have confirmed your results, but it does not appear that their statistical analysis has been thoroughly examined, and I wanted to know if you would be open to a review by an independent team of top statisticians of climate change papers before those papers get published.

You know, I think Dr. Christy had mentioned that in some of his work there were some flaws that were found. He mentioned that in his testimony and then they submitted to that. So if we are going to put government money into papers should they be reviewed by others other than your social network before they are published with government funds and considered to be the truth?

DR. MANN. Well, I think there is a misunderstanding about the nature of peer review as it currently exists with scientific journals, and there have been some misstatements along these lines in the previous comments by some of the others on this panel. For example, two of the studies that have shown that the centering convention in PCA doesn't make a difference in the reconstruction as shown also by Dr. Gulledge were done by groups that are entirely independent of me and my collaborators, von Storch and Zorita. In fact, von Storch and Zorita and I and my collaborators have had vigorous disagreements in the peer reviewed literature.

So one of the studies that actually validated our approach in showing that the centering convention doesn't make a difference was by that group. Another scientist at Woods Hole, Peter Huybers, if I could finish that, also came up with the same result so there are four different studies, only one of which I was connected with that came to that conclusion so the peer review process is actually working quite well.

MRS. BLACKBURN. My question to you is do you think that they should be submitted for independent review before they get published?

DR. MANN. Well, that goes on so again it requires an understanding of what the peer review process at the major scientific journals actually is. For example, with *Nature and Science* when they receive a paper that involves both statistics and climatology you can be certain that they will seek out leaders in the world's scientific community in all of the relevant areas before they make a decision about the publication of that paper, and that is standard in most of the leading journals.

MRS. BLACKBURN. Now let me ask you this then. If your work were submitted to an independent group and they had questions or found items that needed to be changed would you be willing to make those changes prior to that work being published?

DR. MANN. Again, as I have tried to convey to this committee in my earlier testimony and some of my earlier responses to questions, in fact, that has been going on for more than 10 years now. My collaborators and I have been re-examining the data. Other groups have been re-examining the data, testing different methods, testing the methods with climate models simulations, figuring out which methods perform well, which methods don't perform so that process is ongoing. It has been going on for more than a decade now and that is how scientific progress works.

MRS. BLACKBURN. Thank you. Dr. Wegman, your thoughts on those questions?

DR. WEGMAN. Well, I think, first of all, we disagree on, you know--Dr. Mann did not answer your question which was if--

MRS. BLACKBURN. Absolutely he did not answer my question.

DR. WEGMAN. If you would submit to a statistical review panel, would you be willing to do that. He did not answer that question. And one of the troubling aspects of this paleoclimate and the meteorological community in general is that they don't have interaction with statistical people even though they used statistical methods heavily. We have examined this group in general as I mentioned before with Mr. Stearns. We have tried to examine this to see the engagement of the meteorological community, the paleoclimatology community with the statistical community, and it is almost non-existent, so they are not interacting with our group although they are using methods that are based in the statistical literature.

I would like to see, frankly, I would like them to be engaged with us. I think it would be a good idea. What we were trying to do in our testimony was create a path to a better way of doing the science essentially saying that these are two groups that should be interacting and in some sense it behooves the meteorological community to be interacting with us. They are using our methods. We are not using their methods. So I think it would be an important thing to do and I--

MRS. BLACKBURN. Let me ask you very quickly too, I had Michael Crichton's testimony that he had before the Senate. Let's see, I think this was in '05. And he was talking about having a--that government grants should require a replication package which would provide some transparency as part of their funding where posting that package online so that saying that if it is funded with government money there is no reason to exclude anyone from reviewing the data that is found in research. Is that the type thing that you think would be appropriate for transparency?

DR. WEGMAN. As I said last week in one of our conclusions, basically when there is important public policy and human health implications this stuff ought to be subject to exceptionally more intensive review. We drew the NIH model out last time talking about the FDA and how the FDA requires some statistical consultation just to that the drug issue, and it seems to me that in this climate arena this has incredibly important implications for society in general, the world in general, and I think it ought to be carefully reviewed. The fact is Dr. Mann continues to appeal to peer review but the fact is the peer review process failed in the 1998 paper.

MRS. BLACKBURN. And you would say that was primarily because it was not an independent and separate review outside of that social network?

DR. WEGMAN. I believe that is the case.

MRS. BLACKBURN. Okay. Thank you very much. Dr. Christy, let me ask you this. There is an article we have gone to a couple different times in my office, *Energy, Environment and Economics*. It was Dr. Soon wrote an article, Ten Myths of Global Warming. I don't know if you have seen that or not. Are you--

DR. CHRISTY. Sorry, ma'am. I haven't seen it.

MRS. BLACKBURN. Okay. I know there is so much here that has been written. We have killed a lot of trees using all this paper, haven't we? Okay. And he talks about showing the Medieval Warm Period, and I was going to ask you to comment on this but since you have not and my time has basically expired I will just let that pass. And I thank you. Thank you, Mr. Chairman.

MR. WHITFIELD. Thank you. At this time I recognize Mr. Inslee for 10 minutes.

MR. INSLEE. Thank you. I think this really is an amazing hearing. It is amazing because all six people at this table have all agreed on the fundamental thing that this Congress has got to figure out, and that is whether CO_2 is going up, whether humans are partially responsible for that, and whether that is part of the reason the Earth is getting warmer. That is the fundamental issue that Congress faces. And all six people at this table agree with those propositions so I have been asking myself why if we have unanimity on the fundamental question that we got to ask, has Congress not done diddley to do anything about this, and I think the answer is fear, because we fear our inability to deal with it we blind ourselves to the science.

And I think it is a little bit like a person who is shown an X-ray of their lung cancer, refusing to believe it because they don't want to deal with it. And I think that is a pretty good metaphor of what is going on right here. I want to ask Dr. Cicerone, because I think he represents President of the National Academy of Science, how many scientists are involved in that organization, by the way?

DR. CICERONE. About 2,000 members, but our work is done largely by another 6,000 people who are chosen from expertise from different fields who are non-members.

MR. INSLEE. So I figure there is somewhere between 6,000 and 8,000 scientists that you represent here today, and I am impressed by that. The consensus as I understand it in the scientific community is that smoking causes lung cancer on a more probable than not basis in certain instances. Is it the scientific consensus now on a more probable than not basis that increasing CO_2 is associated with global climate change and that humans are responsible for increasing CO_2?

DR. CICERONE. Yes.

MR. INSLEE. So we can say that we have the same level of probability in our belief as to what humans are doing to raise temperatures or at least that both are above 50 percent as we do about lung cancer, is that a fair statement?

DR. CICERONE. I think we understand the mechanics of CO_2 and climate better than we do of what causes lung cancer.

MR. INSLEE. So here we as a country have decided to try to limit and reduce the tremendous damage that is done by lung cancer, but we have got as good or better science on a global cancer and this Congress hasn't done a single thing to deal with that, and I think that is very, very disturbing. Now could I put a slide up here, please, gentlemen, if we can? I want to ask Dr. Cicerone to explain something to us.

If we look at this slide it is going to show the cyclical nature--that is not actually the one I want. Yes. If we look at this slide here it shows the cyclical nature. It is from Dr. Gulledge's slides. It will show the cyclical nature going back 450,000 years ago moving forward to today. We also see CO_2 going down, back up, down, back up, down, back up, and we show a natural variability that has occurred before the industrial age of from about 190 parts per million to about 290 parts per million, and I think that is what Dr. Cicerone referred to as the natural variability that has occurred before we started burning coal, oil, gas, and wood.

Now what I see since the industrial period I have seen this vertical curve go up, and it is vertical since the beginning of the industrial period, so that now we are at a level, this says about 372. I actually think it is about 382 today. And as I understand it, it is bound again on about a vertical curve on this scale to levels of about 550 PPM double, double the highest level of CO_2 in pre-industrial ages back 450,000 years.

So is my understanding of that, Dr. Cicerone, basically accurate that we have an accelerated rate of CO_2 that will end up about twice as high carbon dioxide, which is a known heat trapping gas in our atmosphere that is occasioned since the dawn of the pre-industrial age?

DR. CICERONE. Yes, although I don't think it is necessary that we will end up at double CO_2. And then also we don't know for sure what happened before this time. This is the longest instrumental record we have of real data.

MR. INSLEE. So this is going back as far as we can with real data. We are at higher levels by about 130--excuse me, more than that, about 170 parts per million, is that a fair statement?

DR. CICERONE. Above the minimum, yes.

MR. INSLEE. Now the projections I have seen would suggest that if we continue to spew carbon dioxide and methane into the air or carbon dioxide into the air the best assessments I have seen we will end about double pre-industrial levels by the end of the century. Could you give us your best estimate of that or comment on that at all?

DR. CICERONE. Oh, by the end of the century. It depends on human population. It depends on our energy usage and what technologies we are using to produce the energy so you have to make assumptions about human population, how much energy we will use, and what the technologies will be. Double CO_2 is certainly plausible. It really depends on what humans do.

MR. INSLEE. And it depends on what this Congress does, and what Congress should do is what British Petroleum has done. British Petroleum 7 or 8 years ago decided they were going to meet Kyoto targets. Maybe it was 5 years ago. And in 3 years they met their Kyoto targets in their internal operations. They reduced their CO_2 as much in their internal operations as the Kyoto targets would require. You know what they did? They saved $350 million in wasted energy when they decided to adopt efficiencies of the type that Dr. Cicerone talked about.

The other thing we will do is try to get these plants started. Right down the hallway here yesterday I met with these guys, Iogen Corporation. They are going to open up the first cellulosic ethanol plant in southeastern Idaho. When they do that, we will power our cars on E-85 ethanol. We will reduce our CO_2 emissions per mile by 80 percent or more. They actually think it may actually be negative because of some of the stuff you grow actually takes carbon out of the air and puts it into the soil. It might actually be negative.

This is the kind of thing we need to do, and we are not going to do that until we come to grasp what this science really is. I want to ask--I think there is just such an overwhelming consensus of--I will just read the Academy of Science report. “The IPCC’s conclusion that most of the observed warming of the last 50 years is likely to have been due to the increase in greenhouse gas concentrations accurately reflects the current thinking of the scientific community on this issue.” That is a direct quote from the National Academy of Science. Now there has been some issues brought about Dr. Mann’s studies. There has been some questions about Dr. Christy’s studies. I frankly think there are some legitimate questions about the statistical assessments, and the first one Dr. Mann did, I think they have been changed a little bit since then, I think the same could be said for Dr. Christy, but I guess the question I have, Dr. Cicerone, if Mr. and Mrs. Mann had never met and we never had the services of Dr. Mann, would that have varied the conclusion of the National Academy of Sciences on these fundamental questions?

DR. CICERONE. You must be referring to his parents and not his wife.

MR. INSLEE. I am indeed.

DR. CICERONE. I don’t think so.

MR. INSLEE. And why do you say that? In other words, if Dr. Mann’s work had just never appeared, and, by the way, I respect it and I think it has added to the debate but if his work had never occurred why do you think the Academy of Science would still reach the same fundamental conclusions?

DR. CICERONE. Because of the blending of the physical evidence, the mathematical rigor and the comparisons that can be made now with the predictions and the actual records of the last 30 years especially.

MR. INSLEE. And I have a chart here, gentlemen, if you can put it up here of ice core data. I think it might be the last slide on the series that I had introduced. If you have the groupings of the one that I had brought today. This is just another representation of the CO_2. There should be one more slide. You are not finding it right now. Let’s keep going. Just go through these quickly. Right there. Okay. This is a slide basically showing ice core data and we show CO_2, and if I can read this basically this is methane at the top, carbon dioxide here, from ice core data showing these levels, only it goes backwards. These are today’s dates. This goes back 400,000 years. These are today’s dates showing CO_2 levels higher in ice core data than at any time in the last I believe it is 400,000 years. It should be 600,000 years.

If you can, Dr. Cicerone, can you describe how that ice core data work through the deuterium isotopes, if you can just give us a quick description.

DR. CICERONE. I mentioned earlier the way the gases are pulled out of these dated ice cores. With CO_2 you can do it two ways. With methane you can do it two ways. With nitros oxide you get similar results, low when it was cold, high when it was warm. The deduction of temperatures at the same time depends on the different isotopes, the different forms of the same chemical like carbon, the same element in carbon, in this case oxygen and hydrogen where because the way they evaporate a gas like water evaporates differently if it has heavy hydrogen in it, deuterium, for example, or oxygen 18 instead of O-16.

We can go back and infer what the temperature was in the vicinity of the ice when it formed or the snow in this case which later becomes ice. These records are pretty widely used now, and under certain circumstances they are absolutely the best we can do. They are very quantitative. The statistics are clear. There is some concern over whether the temperature at which the snow formed that made the ice was really a global or a hemispherically average temperature or did it just reflect what was happening regionally, but there you can go into how much O-18 was in the oceans and the changes are big enough that you can infer a pretty good geographical validity of these temperature deductions as well as the carbon dioxide.

MR. INSLEE. Thank you, and thank you all for your testimony today.

MR. WHITFIELD. We may just have a short second round here. I am going to recognize the Chairman of the full committee for 5 minutes.

CHAIRMAN BARTON. Yeah, and I have to go so I apologize for going out of order. This is today's USA Today newspaper, the temperature map on the back. It shows the high temperature was 126 degrees Fahrenheit and the low temperature was 43 degrees Fahrenheit. That is yesterday. Is there a model in existence that can replicate this with any degree of accuracy? This is yesterday's temperature. Dr. Mann, do you have a model that can do that? This is just one country.

DR. MANN. I personally do not.

CHAIRMAN BARTON. We have got an 83 degree difference on one day out of 365 days in one country.

DR. MANN. If I can just talk a little bit to that.

CHAIRMAN BARTON. I only have 4 minutes and--

DR. MANN. I will make it quick.

CHAIRMAN BARTON. Dr. Christy, you are the meteorologist, I think, for Alabama. Do you have a model that could even do this in Alabama?

DR. CHRISTY. No, sir, we wish we did.

CHAIRMAN BARTON. Okay. Now did you want to comment, Dr. Cicerone?

DR. CICERONE. I would. Chairman Barton, you said any degree of accuracy. That gives us some room. The British meteorological office is probably the world's best. They are in the Ministry of Defense in England. Their models have pretty good predictive capability. If you average over a few days and you say let's not argue about the difference between San Francisco and Marin County or San Antonio and El Paso. If you average over enough space in time they can hit that. The models at Penn State University are excellent. The National Weather Service can give you some degree of accuracy and predictability, and they can reproduce a lot of those patterns.

CHAIRMAN BARTON. Well, my point is, and I am not trying to be cute about this, in preparing for last week's hearing I read the summary and I read most of the report of the National Academy of Sciences here, the National Research Council. I read Dr. Wegman's report. And somewhere in those two reports it said the data sets they use to base all these models on in the whole world there are like 60 or less data sets. There are just not a lot of data, and we are trying to make predictions over thousands of years. Even where we have really good records for the last hundred years, and some of the most advanced satellites and smart people that put these computer models together with hundreds of variables, we can't really predict after the fact yesterday's weather with too much accuracy, and yet to go to Dr. Christy's point if we accept Congressman Inslee's point that we need to be in solution mode a thousand nuclear power plants by themselves is a trillion and a half dollars, and that will get you a CO_2 reduction of 10 percent.

There are somewhere between 5,000 and 10,000 power plants in this country. Now I don't know exactly but I know there are only 100--I think 112 operating nuclear plants of those between 5,000 and 10,000. And that is just one part of the economy. We have got 300 million cars and trucks. We have a lot. I mean, it is not scientifically accurate but we have got a boon' doggle worth of economic consequences if we really go where Mr. Inslee says we ought to be going. And I am not dogmatic about it. I am concerned when I hear Dr. Cicerone say that the parts per billion of CO_2 in the atmosphere is 100 parts per million higher than it ever has been.

Now that has got to give anybody pause to think, but I look at all these charts and all these data sets and I can't back it up, but it would certainly appear to me to be plausible, to use that term again, that the Earth is always changing temperature. It is either in a warming period or a cooling period. It appears that it is a curve function. It appears that it is over the same general period of time and it certainly appears that in the last 100 years that the upward curve has accelerated at a more rapid rate than say a thousand years ago. But it is not clear what, if anything, we can do to change that basic system. And so before we go off the deep end I really do want to make sure that these models are independently reviewed and really are scientifically accurate and really can be replicated. And I really do want to know what the confidence levels are. We are going to get to problem solution, and we are going to have a huge debate about that. But since we can't even predict with much accuracy what yesterday's temperature was, it is a little bit much to ask us to make multi-trillion dollar decisions on models that 10 years ago when Dr. Mann put out his report, he was the first one, and even today most of the people that are doing the modeling are some part of his network, which is not a bad thing. It shows you operate with a lot of smart people that care a lot about the environment.

But it doesn't mean that the United States government makes trillion dollar changes in public policy until we get a little bit more information about that, and that is why we are doing these hearings. And so I apologize for going another minute over but I thank you, Mr. Chairman, for holding this, and I thank the members, Mr. Inslee and Mr. Stupak and Mrs. Blackburn for being here. I wish every member of the Oversight Subcommittee was here. I wish we had more intensity on this so that we could get more involved. Thank you, Mr. Chairman.

MR. WHITFIELD. Thank you. Dr. Mann, did you want to make a comment?

DR. MANN. I just wanted to clarify a distinction here in this discussion. On the one hand we are talking about weather, and that is the day-to-day fluctuations and the character of the atmosphere, and in the other cases we are talking about climate and there is a very important distinction between the two. Climate is the statistics, the long-term statistics of the weather, and there are certain things that we can say very well about climate.

CHAIRMAN BARTON. But your model is predicting temperature change.

DR. MANN. It is not a model.

CHAIRMAN BARTON. That may be the variable but that is the variable, and we are talking about catastrophic consequences with 3 to 4 degree Fahrenheit changes.

DR. MANN. That is the point. It is not a model. A model is a set of numerical equations that we try to solve the equations that describe the atmosphere and the ocean. Our reconstructions aren't that. They are not a model. The models are a completely different thing, and there are weather forecasting models as well as climate models, and in certain things the climate models are quite good. We are doing very well now in predicting El Ninos.

CHAIRMAN BARTON. What term should I be using? Not model. Program, algorithm?

DR. MANN. They are statistical reconstructions and data and then there are the models, and I just wanted to make that distinction.

CHAIRMAN BARTON. I stand corrected.

MR. WHITFIELD. I just have one other question I would like to ask because we have heard a lot today about core samples, and I have been sort of interested in this chart that Mr. Inslee brought in showing CO_2 concentrations and from that extrapolating temperatures. And I would just ask Mann and Christy and Cicerone once again, I didn't really ask this question before, but I would like for you to tell us the facts about the reliability of the ice core samples. And we have heard a lot of comments about using that to determine CO_2 and then the question is using ice core samples as historical thermometers. Can they really be considered accurate thermometers. Can you take those CO_2 levels from ice cores and extrapolate in an accurate way?

DR. MANN. I will take the first stab at that. There are certain physical processes and there are basic physical processes that control the ratio of different isotopes, of oxygen in the ice, the water that is in solid form, it is ice trapped in those ice cores, and so it is on a somewhat different footing from some of the other sorts of proxies like tree rings that we use where we are relying on some biological relationship.

In the case of ice core isotopes it is really physics. It is physics that is controlling the ratio of the different isotopes of oxygen and that is telling us something about the sea surface temperatures when the water evaporated from the ocean because the ice that is deposited at some point had to evaporate from the ocean surface. It also tells us something about the local conditions when the ice was deposited. Both the evaporation and the deposition depend--they influence the ratio of those isotopes.

DR. CHRISTY. Just in terms of the temperatures, reproducing temperatures from them?

MR. WHITFIELD. Yes.

DR. CHRISTY. The closer you get to the poles, the better the temperature relationship is. I think in the NAS report we show six tropical and Tibetan ice cores and they are all different. All six of them are different. But the closer you get to the poles the relationship looks a lot better there.

MR. WHITFIELD. Okay. Do you have anything to add, Dr. Cicerone? Okay. Yes. Mr. Waxman had asked we enter into the record the remote sensing system letter which we will do and you asked about the interface stewardship alliance which we will do. And then we are going to keep the record open for 30 days. And does anyone else have any comments?

MR. STUPAK. If I may, Mr. Chairman. We were talking earlier, I was going to start off my questioning and we were talking about the

Fortran, and I was joking with the Chairman so I forgot to ask these questions. Dr. Wegman, in your report you state that, and I am quoting now, "We judge that the sharing of research materials data and results by Dr. Mann was haphazardly and grudgingly done." You also go on to state that Dr. Mann--you had trouble reading Dr. Mann's code in part because it was in Fortran and that you had trouble understanding some of the data that Dr. Mann used.

Did you or your co-authors contact Mr. McIntyre and get his help in replicating his work?

DR. WEGMAN. Actually, no. What I did do was I called Mr. McIntyre and said that when we downloaded his code we could not get it to work either, and it was unfortunate that he was criticizing Dr. Mann when in fact he was in exactly the same situation. Subsequently, he reposted his code to make it more user friendly and we did download it subsequently and verified that it would work.

MR. STUPAK. And then after you re-downloaded and verified it worked, did you have any further contact with Mr. McIntyre then?

DR. WEGMAN. Well, as I testified last week, Dr. Said and myself had gone to one of the meetings where he was talking, and we spoke with him but did not identify who we were at the time. This was early in the phase. Subsequently, I had had no contact with him until basically last week.

MR. STUPAK. Okay. Any of your co-authors that you know of, Dr. Said or any others, have contact with Mr. McIntyre other than that one time at this convention or wherever he was speaking?

DR. WEGMAN. One of my graduate students, John Rigsby, who did the code for us, worked the code for us, did have some interaction with him in order to verify some of the details of the code.

MR. STUPAK. So you, Dr. Said and this Mr. Rigsby would be the people who had contact with Mr. McIntyre then?

DR. WEGMAN. That is correct, yes.

MR. STUPAK. Thank you. Nothing further.

MR. WHITFIELD. Mr. Inslee, do you have any--

MR. INSLEE. I just want to comment in response to Chairman Barton's comment about the 1,000 or 10,000 nuclear plants he posited might be necessary. I really--and I don't want to get in debate about nuclear but I am really much more optimistic about that, and the reason I say that is that we have been so successful in improving the efficiency of our economy because of the intellectual capital of men and women like you who have helped us develop technologies to be much more efficient. Let me give you an example.

We actually per unit of gross domestic product use almost half as much energy as we did in 1973. You think about that. Since 1973 our economy produces twice as much domestic product with the same amount of energy that it did in 1973, and there is just no reason on this green Earth that all of a sudden we got stupid, that we are not going to be able to continue as the most brilliant society on Earth and innovation to continue those efficiency innovations.

And they are not rocket science. Three of my neighbors drive cars that have already reduced their transportation related CO_2 by 50 percent. The Chairman talked about the need to reduce our emissions by 40 percent to meet Kyoto. Three of my neighbors and myself, I may add, have already reduced ours by 50 percent in our transportation sector. Simple. They are on the lots today. This is no new technology. So I just want to say in partial closing that I am a person, as my comments have indicated, who believe this is a major challenge for us and that we have to act, and it is well past the date where we need to move to solutions rather than debating the problem.

But I also believe that I am an optimist because I totally believe it is in the human--it is capable because of our intellectual ability to invent our way out of this pickle. And those who are people of great faith, because the faith community is now becoming engaged in this debate, because we are stewards of God's creation, and they are starting to urge Congress to act as well. We also ought to be optimists and believe we can do it.

And I got to tell you, in the last 3 weeks I have met five people, one in cellulosic ethanol, one in wave power, one in efficiency in cars, one in efficiency in airplanes the Boeing 787 we are building in Seattle is going to get 20 percent better fuel mileage than their last model. These are the kind of things that America is going to do when we tackle this. So I just hope that this is a first step toward moving just one quick question, Dr. Cicerone. I have heard there has been some new evidence about finding large amounts of energy in the ocean that has suggested that this is sort of new research to indicate in the last 12 months. Is this something I am dreaming or is there new research in that regard?

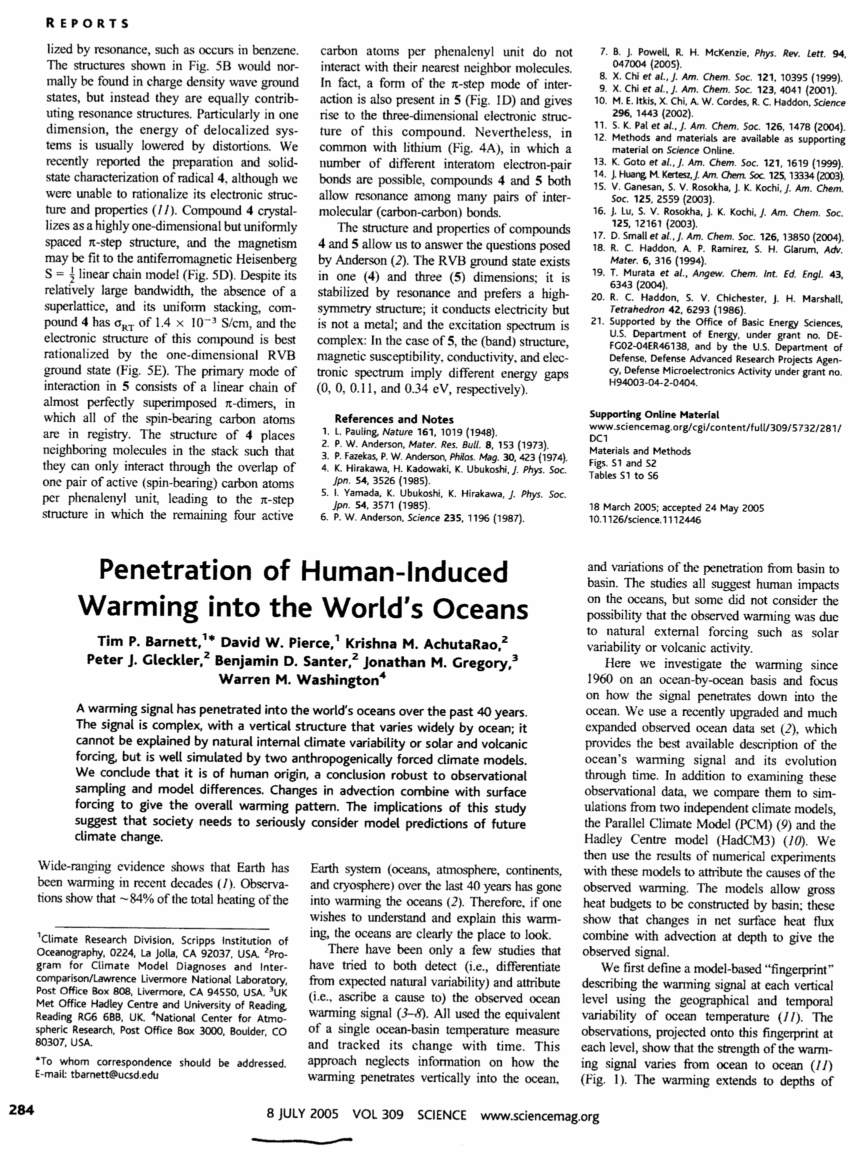
DR. CICERONE. Maybe methane clath rates would be the only thing I am--

MR. INSLEE. I am sorry. What I mean is as far as we found temperature increases in the oceans that have--

DR. CICERONE. Oh, yeah. The result was reported about a year and a half ago about over the last 40 or 45 years the oceans, the upper 700 meters or so have warmed up, and I summarize it very briefly in my testimony, yes.

MR. INSLEE. And I will put in the record a study called Penetration of Human-Induced Warming into the World's Oceans. It is published in *Science* in July, 2005. Many people thought this was sort of the nail in the coffin of skeptics about global warming. And again thank you for your testimony.

[The information follows:]



lized by resonance, such as occurs in benzene. The structures shown in Fig. 5B would normally be found in charge density wave ground states, but instead they are equally contributing resonance structures. Particularly in one dimension, the energy of delocalized systems is usually lowered by distortions. We recently reported the preparation and solid-state characterization of radical **4**, although we were unable to rationalize its electronic structure and properties (*11*). Compound **4** crystallizes as a highly one-dimensional but uniformly spaced π-step structure, and the magnetism may be fit to the antiferromagnetic Heisenberg $S = \frac{1}{2}$ linear chain model (Fig. 5D). Despite its relatively large bandwidth, the absence of a superlattice, and its uniform stacking, compound **4** has σ_{RT} of 1.4×10^{-3} S/cm, and the electronic structure of this compound is best rationalized by the one-dimensional RVB ground state (Fig. 5E). The primary mode of interaction in **5** consists of a linear chain of almost perfectly superimposed π-dimers, in which all of the spin-bearing carbon atoms are in registry. The structure of **4** places neighboring molecules in the stack such that they can only interact through the overlap of one pair of active (spin-bearing) carbon atoms per phenalenyl unit, leading to the π-step structure in which the remaining four active carbon atoms per phenalenyl unit do not interact with their nearest neighbor molecules. In fact, a form of the π-step mode of interaction is also present in **5** (Fig. 1D) and gives rise to the three-dimensional electronic structure of this compound. Nevertheless, in common with lithium (Fig. 4A), in which a number of different interatom electron-pair bonds are possible, compounds **4** and **5** both allow resonance among many pairs of intermolecular (carbon-carbon) bonds.

The structure and properties of compounds **4** and **5** allow us to answer the questions posed by Anderson (*2*). The RVB ground state exists in one (**4**) and three (**5**) dimensions; it is stabilized by resonance and prefers a high-symmetry structure; it conducts electricity but is not a metal; and the excitation spectrum is complex: In the case of **5**, the (band) structure, magnetic susceptibility, conductivity, and electronic spectrum imply different energy gaps (0, 0, 0.11, and 0.34 eV, respectively).

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21. Supported by the Office of Basic Energy Sciences, U.S. Department of Energy, under grant no. DE-FG02-04ER46138, and by the U.S. Department of Defense, Defense Advanced Research Projects Agency, Defense Microelectronics Activity under grant no. H94003-04-2-0404.

Supporting Online Material

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Materials and Methods
Figs. S1 and S2
Tables S1 to S6

18 March 2005; accepted 24 May 2005
10.1126/science.1112446

Penetration of Human-Induced Warming into the World's Oceans

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Peter J. Gleckler,[2] Benjamin D. Santer,[2] Jonathan M. Gregory,[3]
Warren M. Washington[4]

A warming signal has penetrated into the world's oceans over the past 40 years. The signal is complex, with a vertical structure that varies widely by ocean; it cannot be explained by natural internal climate variability or solar and volcanic forcing, but is well simulated by two anthropogenically forced climate models. We conclude that it is of human origin, a conclusion robust to observational sampling and model differences. Changes in advection combine with surface forcing to give the overall warming pattern. The implications of this study suggest that society needs to seriously consider model predictions of future climate change.

Wide-ranging evidence shows that Earth has been warming in recent decades (*1*). Observations show that ~84% of the total heating of the Earth system (oceans, atmosphere, continents, and cryosphere) over the last 40 years has gone into warming the oceans (*2*). Therefore, if one wishes to understand and explain this warming, the oceans are clearly the place to look.

There have been only a few studies that have tried to both detect (i.e., differentiate from expected natural variability) and attribute (i.e., ascribe a cause to) the observed ocean warming signal (*3*–*8*). All used the equivalent of a single ocean-basin temperature measure and tracked its change with time. This approach neglects information on how the warming penetrates vertically into the ocean, and variations of the penetration from basin to basin. The studies all suggest human impacts on the oceans, but some did not consider the possibility that the observed warming was due to natural external forcing such as solar variability or volcanic activity.

Here we investigate the warming since 1960 on an ocean-by-ocean basis and focus on how the signal penetrates down into the ocean. We use a recently upgraded and much expanded observed ocean data set (*2*), which provides the best available description of the ocean's warming signal and its evolution through time. In addition to examining these observational data, we compare them to simulations from two independent climate models, the Parallel Climate Model (PCM) (*9*) and the Hadley Centre model (HadCM3) (*10*). We then use the results of numerical experiments with these models to attribute the causes of the observed warming. The models allow gross heat budgets to be constructed by basin; these show that changes in net surface heat flux combine with advection at depth to give the observed signal.

We first define a model-based "fingerprint" describing the warming signal at each vertical level using the geographical and temporal variability of ocean temperature (*11*). The observations, projected onto this fingerprint at each level, show that the strength of the warming signal varies from ocean to ocean (*11*) (Fig. 1). The warming extends to depths of

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700 m or more in both the North and South Atlantic oceans, but is largely confined to the upper 100 m of the northern Pacific and northern Indian oceans. The northern Indian Ocean is particularly unusual in that it has a subsurface maximum. Both the northern and southern Pacific Ocean show a sign reversal in the warming signals, indicating a cooling at ~150-m depth. These differences between oceans constitute the spatial structure of the warming fingerprint. The final dimension of the signal is the temporal evolution of the differences. Because we are interested in low-frequency variations, we use decadal time averages to describe this time evolution.

Our purpose is to understand the origin of this complex time- and space-dependent signal. We explore three possible causes: natural variability internal to the coupled ocean-atmosphere system; external natural variability, such as solar or volcanic forcing; and forcing arising from human activity [emission of greenhouse gases (GHGs) and sulfate aerosols].

The likelihood that natural internal climate variability is the cause of the observed warming signal can be examined by analyzing a long control run of the PCM; i.e., how well did natural internal variability in the control run project onto the warming fingerprint (*11*)? This approach was used in earlier work (*3*) and is a variant of standard detection and attribution analysis (*12–15*). It has the advantage of having a simple geometric explanation while being rigorous in a statistical sense (*11*).

The strength of the warming signal in the control run (sampled in the same places as the observations) is shown in Fig. 2 for each ocean. This gives some indication of the fluctuations in signal strength that one might expect due to natural internal variability alone. Because we had multiple realizations of the 40-year time period in the control runs, we can show the 90% confidence limits of the natural variability by the hatched region. Also shown is the signal strength in the observations (red dots) from Fig. 1. The illustration demonstrates that the warming signal is far stronger than would be expected from natural internal variability, as estimated by the model. To assess whether the model's estimate of natural variability is reasonable, we compared the levels of variance in the control run at decadal time scales to those observed and found that they matched reasonably well (*16*) (supporting online text). Therefore, the control run variations are a reasonable representation of natural internal variability, at least on the decadal time scales of interest here.

Another possible candidate for the warming signal is natural variability external to the ocean-atmosphere system, such as solar variability or volcanic eruptions ("SV" forcing). We explored this possibility by analyzing PCM runs forced by estimates of observed solar variability and volcanic aerosol loadings (*17*). The results of four such runs were combined and the warming signal strength estimated in the SV data set. The results (Fig. 2) show that in none of the oceans can the SV forcing (green triangles) replicate the observed warming. Indeed, at these space and time scales, the SV forcing produces signal strengths indistinguishable from those expected from natural internal variability (hatched region).

The final candidate for explaining the signal is anthropogenic factors, such as well-mixed GHGs and sulfate aerosol particles. We examined this possibility in an ensemble of

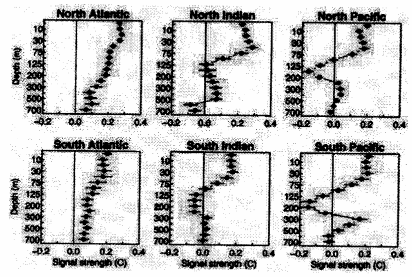


Fig. 1. Warming signal strength by ocean and depth. The dots represent the projection of the observed temperature changes onto the model-based pattern of warming. They show substantial basin-to-basin differences in how the oceans have warmed over the past 40 years, although all oceans have experienced net warming over that interval. The horizontal bars represent the ±2SD limits associated with sampling uncertainty.

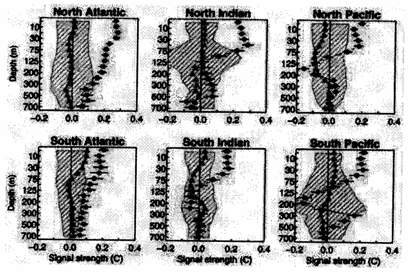


Fig. 2. Multiple realizations from the PCM control run allowed estimation of the probability distribution of signal strength associated with natural internal variability. The hatched region represents the 90% confidence limits of the natural internal variability signal strength. The observed signal strength (red dots) bears little resemblance to that expected from natural internal variability. The ensemble-averaged strength of the warming signal in four runs forced by observed solar and volcanic variability (green triangles) is also shown. There is no agreement between the two. The solar plus volcanic signals are generally indistinguishable from those expected from natural internal variability alone on the time and space scales used in this study.

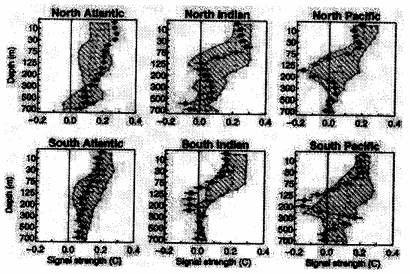


Fig. 3. Anthropogenic forcing signal strength (green hatched region) compared to that obtained from the observations (red dots). There is excellent agreement at most depths in all oceans. The hatched region shows the range of the signal strength estimates from five different realizations of identically forced simulation with the PCM, whereas the smaller green dots within the region are the individual realizations.

five PCM runs with such forcing (*17*, *18*). The results (Fig. 3) show the range of the signal strength in five scenario runs by ocean and depth (hatched area) in comparison with the observations (red dots). An ocean-by-ocean and depth-by-depth comparison shows that the agreement is compelling. The immediate conclusion is that human influences are largely responsible for the warming signal. This level of agreement could not have been tuned into the models, because the fingerprint is too complex in space and time. Further, about half of the observations used in this study were not available when the computer simulations were run.

The different response of individual oceans to GHG forcing is an interesting finding. The physical reasons for this are fairly well known, with one major surprise. For instance, it is well known that deep convection is characteristic of both the North and South Atlantic oceans (*19*). That explains why the warming signal penetrates relatively deeply in these oceans. In contrast, the northern Pacific Ocean is characterized by a rather shallow meridional overturning circulation (*20*) that tends to isolate the surface layers from the deeper ocean. It is also true that no deep water is formed in the northern Pacific. Both physical properties act to confine the signal to the upper ocean. The same situation is thought to hold over much of the southern Indian Ocean.

One notable feature from the observed and modeled signal strength (Figs. 1 and 3) is the negative lobe at 150- to 200-m depth in the Pacific. The simulation that captured this signal showed that it is associated with a thinning of the western Pacific warm pool associated with shoaling of the deeper isotherms, which has also been observed in the Pacific since the 1970s (*20*).

The major surprise is the northern Indian Ocean, which has rather shallow signal penetration and a subsurface maximum in signal strength. The heat budget for this region (Fig. 4) shows that it is the only basin where the ensemble variability includes zero for the net surface heat flux, and where advective warming is the dominant cause of the basin temperature change over the last 60 years in PCM. This result is likely due to the cancellation of GHG warming by sulfate aerosol cooling, a result recently found from direct observations (*21*). Further simulations have shown that carbon aerosols also play a role in this effect (*22*), but are not included in the simulations of this paper. Hence the warming of this ocean over the past 60 years in the simulation was largely due to changes in advection. Recent observations show a slowing of the shallow meridional circulation cell in the Indian Ocean such that advection from northern to southern Indian Ocean is reduced, leading to a net warming of the northern Indian Ocean (*23*), a result in accord with the model predictions. Indeed, inspection of the partition of net surface air/sea heat exchange and advection for the various oceans (Fig. 4) shows that in several basins, changes in advection of heat by ocean currents redistribute the heat gained from the anthropogenic forcing, and so are important to determining the structure and evolution of the warming signal in the oceans [compare (*7*)].

The normalization by surface area used in Fig. 4 removes the geometric impact of ocean size on our results. It also makes clear that the southern oceans are absorbing more heat per unit area than are the northern oceans. We suggest that this is again due to the smaller aerosol concentrations over the southern oceans. They do not have the same near-canceling effects observed in the more polluted Northern Hemisphere (*21*, *22*).

Is PCM unique in being able to capture the complex observed signal? To address this question, we repeated the analysis with HadCM3, which was developed at the Hadley Centre independently of PCM. The four realizations from this model were the "All" forcings runs, which combined a variety of forcings [GHG, solar, volcanic, aerosols, etc. (*24*)]. HadCM3's warming fingerprint (not shown) is little different from PCM's, but we use exclusively PCM's fingerprint here for consistency. The results from HadCM3 are compared to observations in Fig. 5. Using the HadCM3 fingerprint gives even better agreement (not shown), as one would expect. In any event, Figs. 3 and 5 show that PCM and HadCM3 both reflect reasonably well the evolution and spatial characteristics of the warming signal.

An interesting feature of the above result is that the PCM and HadCM3 are very different models. In the simulations used here, their forcings are also rather different. How could they give very similar evolutions of ocean temperature? Whatever the combinations of forcing used by the two modeling groups, the net forcing at the surface of the ocean had to be essentially the same. Inspection of each model's surface heat flux fields, in so far as possible, shows this to be the case. The details of the atmospheric forcing and climate feedbacks that go into producing that net value do not affect the overall oceanic response. The story may be different in the atmosphere.

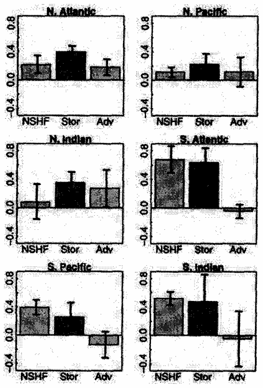


Fig. 4. Gross heat budget by ocean showing the important role that heat advection by ocean currents plays in the anthropogenic warming of the world's oceans. The PCM's net ocean surface heat flux averaged over 1940 to 1999 is shown by the "NSHF" bars, the modeled changes in ocean basin heat storage by the "Stor" bars, and the advection of heat by ocean currents needed to close the heat budget by the "Adv" bars. The latter was obtained as a residual from the first two estimates. The uncertainty bars indicate ±1SD based on the ensemble spread. The actual energy change (joules) over the time period has been normalized by surface area of respective oceans to give the average heating rate in W/m^2. This normalization makes it appear that the net advection over the globe is nonzero, a condition that vanishes using the area-weighted fluxes.

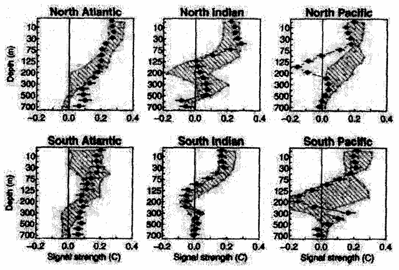
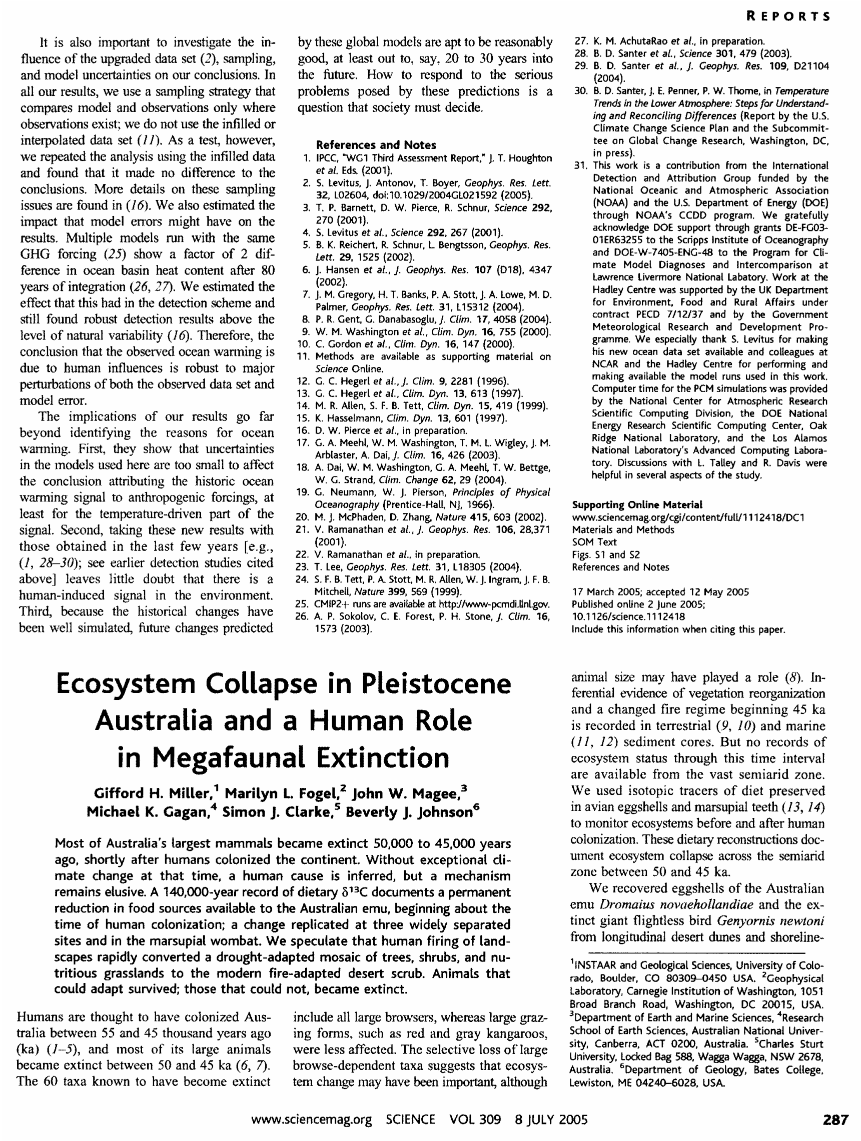


Fig. 5. Comparison of the model-predicted anthropogenic signals between HadCM3 and observations for the oceans. The format is described in Fig. 3. Comparison of Figs. 3 and 5 shows that both models capture the main structure of the signal in the observations. These figures were derived by using the warming signal defined by PCM as the basis set to allow a consistent comparison.



It is also important to investigate the influence of the upgraded data set (*2*), sampling, and model uncertainties on our conclusions. In all our results, we use a sampling strategy that compares model and observations only where observations exist; we do not use the infilled or interpolated data set (*11*). As a test, however, we repeated the analysis using the infilled data and found that it made no difference to the conclusions. More details on these sampling issues are found in (*16*). We also estimated the impact that model errors might have on the results. Multiple models run with the same GHG forcing (*25*) show a factor of 2 difference in ocean basin heat content after 80 years of integration (*26*, *27*). We estimated the effect that this had in the detection scheme and still found robust detection results above the level of natural variability (*16*). Therefore, the conclusion that the observed ocean warming is due to human influences is robust to major perturbations of both the observed data set and model error.

The implications of our results go far beyond identifying the reasons for ocean warming. First, they show that uncertainties in the models used here are too small to affect the conclusion attributing the historic ocean warming signal to anthropogenic forcings, at least for the temperature-driven part of the signal. Second, taking these new results with those obtained in the last few years [e.g., (*1*, *28*–*30*); see earlier detection studies cited above] leaves little doubt that there is a human-induced signal in the environment. Third, because the historical changes have been well simulated, future changes predicted by these global models are apt to be reasonably good, at least out to, say, 20 to 30 years into the future. How to respond to the serious problems posed by these predictions is a question that society must decide.

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31. This work is a contribution from the International Detection and Attribution Group funded by the National Oceanic and Atmospheric Association (NOAA) and the U.S. Department of Energy (DOE) through NOAA's CCDD program. We gratefully acknowledge DOE support through grants DE-FG03-01ER63255 to the Scripps Institute of Oceanography and DOE-W-7405-ENG-48 to the Program for Climate Model Diagnoses and Intercomparison at Lawrence Livermore National Labatory. Work at the Hadley Centre was supported by the UK Department for Environment, Food and Rural Affairs under contract PECD 7/12/37 and by the Government Meteorological Research and Development Programme. We especially thank S. Levitus for making his new ocean data set available and colleagues at NCAR and the Hadley Centre for performing and making available the model runs used in this work. Computer time for the PCM simulations was provided by the National Center for Atmospheric Research Scientific Computing Division, the DOE National Energy Research Scientific Computing Center, Oak Ridge National Laboratory, and the Los Alamos National Laboratory's Advanced Computing Laboratory. Discussions with L. Talley and R. Davis were helpful in several aspects of the study.

Supporting Online Material
www.sciencemag.org/cgi/content/full/1112418/DC1
Materials and Methods
SOM Text
Figs. S1 and S2
References and Notes

17 March 2005; accepted 12 May 2005
Published online 2 June 2005;
10.1126/science.1112418
Include this information when citing this paper.

Ecosystem Collapse in Pleistocene Australia and a Human Role in Megafaunal Extinction

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Michael K. Gagan,[4] Simon J. Clarke,[5] Beverly J. Johnson[6]**

Most of Australia's largest mammals became extinct 50,000 to 45,000 years ago, shortly after humans colonized the continent. Without exceptional climate change at that time, a human cause is inferred, but a mechanism remains elusive. A 140,000-year record of dietary $\delta^{13}C$ documents a permanent reduction in food sources available to the Australian emu, beginning about the time of human colonization; a change replicated at three widely separated sites and in the marsupial wombat. We speculate that human firing of landscapes rapidly converted a drought-adapted mosaic of trees, shrubs, and nutritious grasslands to the modern fire-adapted desert scrub. Animals that could adapt survived; those that could not, became extinct.

Humans are thought to have colonized Australia between 55 and 45 thousand years ago (ka) (*1*–*5*), and most of its large animals became extinct between 50 and 45 ka (*6*, *7*). The 60 taxa known to have become extinct include all large browsers, whereas large grazing forms, such as red and gray kangaroos, were less affected. The selective loss of large browse-dependent taxa suggests that ecosystem change may have been important, although animal size may have played a role (*8*). Inferential evidence of vegetation reorganization and a changed fire regime beginning 45 ka is recorded in terrestrial (*9*, *10*) and marine (*11*, *12*) sediment cores. But no records of ecosystem status through this time interval are available from the vast semiarid zone. We used isotopic tracers of diet preserved in avian eggshells and marsupial teeth (*13*, *14*) to monitor ecosystems before and after human colonization. These dietary reconstructions document ecosystem collapse across the semiarid zone between 50 and 45 ka.

We recovered eggshells of the Australian emu *Dromaius novaehollandiae* and the extinct giant flightless bird *Genyornis newtoni* from longitudinal desert dunes and shoreline-

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DR. GULLEDGE. Mr. Inslee, if I may just may make a comment. Also regarding Mr. Barton's comments, I realize he is gone. I am also from Texas and I use scientific terms from down there. There are whole passels of money to be made on alternative energy, and it is not just about being expensive. Also, there are real serious costs to inaction that have not been figured into this equation here.

MR. INSLEE. And I just want to compliment the Chairman's humor about this. As I was walking off the field at the baseball game this year and he was at third base, and I just pulled my hamstring. As I was walking by he says, well, Inslee, I suppose that was because of global warming too. So he has a great finely tuned sense of humor and we will look forward to using it as the debate goes on. Thank you.

MR. WHITFIELD. Thank you, Mr. Inslee. I would like to stipulate that in my district we just opened up two new ethanol plants as well. So I want to thank you all very much for your patience. We got documents to enter here.

MR. STUPAK. Mr. Chairman, that is a request to put in an abstract of an article. I would suggest we just get the whole article, put it in there, and then we have the complete article for everyone to see.

MR. WHITFIELD. Okay. Without objection.

MR. STUPAK. That can serve as a place holder until I get the whole article.

MR. WHITFIELD. So ordered. And then we will keep the record open. Mr. Inslee.

[The information follows:]

Top 10 Myths About Global Warming

MYTH 1: *Scientists have conclusively proven that human activity is causing the earth to warm.*

FACTS: The theory of global warming rests on the idea that human emissions of carbon dioxide and other greenhouse gases have caused earth's temperature to rise unnaturally over the last century.

The largely undisputed scientific facts of climate change are the following: 1) human use of fossil fuels over the last century has increased the amount of carbon dioxide and other greenhouse gases in our atmosphere; 2) greenhouse gases naturally absorb infrared radiation and retain some of that heat close to earth's surface; and 3) the 20th century was 0.5° C warmer than the previous century.[1] What is hotly disputed among scientists is how sensitive global temperature is to human greenhouse gas emissions and how much warming—if any—is attributable these emissions.

We are just beginning to explore the function and magnitude of natural feedback systems that affect concentrations of greenhouse gases and global temperature changes. In a 2002 study attempting to compare carbon dioxide concentrations and earth's temperature over the last 500 million years, the author found that the "comparison with the geologic record of climatic variations reveals no obvious correspondence."[2]

Some scientists have argued persuasively that the earth's temperature changes are markedly more sensitive to changes in solar radiation than greenhouse gases. Solar radiation indirectly affects cloud formation that can act as a feedback mechanism to regulate earth's temperature.[3] However, the complex behavior of clouds and the primary greenhouse gas—water vapor—are poorly understood.[4] The temperature effects of clouds and aerosols are significant enough to reduce estimated warming from greenhouse gases and in some scenarios may even produce a slight global cooling.[5,6]

Many global warming proponents point to computer model simulations that show earth's temperature dramatically increasing in the 21st Century with a concurrent rise in carbon dioxide emissions. However, due to the extraordinary complexity of the natural world, computer models simplify, ignore, and/or misinterpret many important climate feedback mechanisms. Models are also prone to enormous errors from the simple fact that they often track over five million parameters. Upon testing two different computer models used in the discredited U.S. National Assessment on Climate Change, Dr. Patrick Michaels, a climatologist with the University of Virginia, found that a random number table did a better job of simulating the past century's temperatures than both models.[7] In Figure 1, a comparison of model temperature predictions are much higher than the observed trend.

Finally, climate models produce simulations on a global level, and any attempt to predict what may happen on a smaller, regional scale flies against the consensus of international climate scientists. The United Nations' Intergovernmental Panel on Climate Change (IPCC), wrote this about computer climate models: "Despite recent improvements and developments ... a coherent picture of regional climate change ... cannot yet be drawn."[8] Climate models are improving all the time, but policymakers should understand they are simulations of how we believe the world works—not reality.

MYTH 2: *Earth's temperature has risen to an unnatural level over the last century.*

FACTS: NASA temperature records indicate the 20th century was slightly warmer than the previous century with surface temperatures rising about 0.5° C. Climatologist Patrick Michaels, Ph. D, identifies three different temperature trends during the last century. From 1900 to 1940 there was a period of strong warming followed by another period of cooling until about the late 1970s. Then, from the late 1970s to the present, the earth's surface has warmed again.[9]

Over 80 percent of greenhouse gas emissions occurred after 1940, so the first period of warming cannot be attributed to human activities. And the mid-20th century cooling can't be a warming response due to increased greenhouse gases. Therefore, it appears that global temperature changes for most of the last century has been natural.[10]

Reconstructions of long-term temperature records show that the 20th Century was not unusual. One analysis of 250 climate studies covering the previous millennium confirms the presence of the Medieval Warm Period from 800 to 1300 AD that was warmer than our present day temperature. Furthermore, global temperatures dipped during a period known as the Little Ice Age, which lasted approximately from 1300 to 1900 AD.[11] As we emerge from this cooling period, it is natural that our temperature is somewhat higher than in the immediate past.

On an even longer scale, researchers have reconstructed a temperature record covering the last 219,000 years by coring through accumulated ice in Antarctica.[12] In Figure 2, the line across the top represents deviation

Figure 1: Comparison of Model Temperature Projections

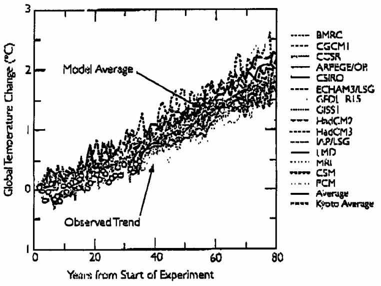
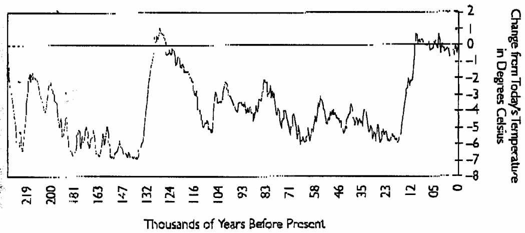


Figure 2: Vostok, Antarctica Ice Core Temperature Record



from today's temperature. By this measurement, you can see our current temperature is within the range of temperatures experienced over the past 9,000 years.

MYTH 3: ***The ice caps are melting and sea levels are rising.***

FACTS: According to satellite data, ice coverage over the Arctic (northern hemisphere) has indeed decreased over the last 30 years. [13] However, the Arctic climate reconstructed over the last 125 years shows that ice coverage varies on multi-decade scale. The current period of warming was preceded by a prolonged period of cooling in which ice coverage was much more extensive.[14] Furthermore, the thickness of Arctic Ocean sea ice had remained constant through the 1990s. [15] On the opposite side of the globe, Antarctic (southern hemisphere) ice coverage has *increased* since 1977.[16]

As a whole, glaciers have indeed been retreating since the end of the Little Ice Age in the 1890s. However, for glaciers on which data exist, the rate of retreat has been constant or decreasing for the last 70 years—the time period during which greenhouse gases have supposedly caused unprecedented warming.[17] One comprehensive glacial study concluded that "there is no obvious common or global trend of increasing glacier melt in recent years." This same study reported that in Europe, "Alpine glaciers are generally shrinking, Scandinavian glaciers are growing, and glaciers in the Caucasus are close to equilibrium for 1980-95."[18]

Regarding the issue of rising oceans, the science is still very unsettled. Oceans have risen and fallen extensively over the geologic past, but their current rate of change is still unknown. Australian policy analyst John Daly wrote, "an impression has been conveyed to the public, media, and policymakers that the sea level rise of 18 cm in the past century is an observed quantity and thus open to little dispute. What is not widely appreciated is that this quantity is largely the product of [computer] modeling and not a product of observation. It is therefore very much open to dispute." [19]

Global mean sea level is modeled because measurements are complicated by changes in moon and sun orbital effects, coastline and seabed topography, ocean currents, and meteorological effects of wind and pressure. Furthermore, most tide gauges are next to urban centers, which tend to subside over time due to the weight of the structures and withdrawal of water from underground aquifers. Additionally, most tidal data from the Northern Hemisphere are affected by a natural geologic phenomenon called "postglacial rebound." After the last ice age 10,500 years ago, the earth uplifted or rebounded as ice as thick as 1.3 miles melted away. Areas on the periphery of the ice sheets have in turn sunk as the crust adjusts and rebalances itself—a process that continues today.[20] In tidal gauges from the Southern Hemisphere including Australia, Uruguay, South Africa, and India—regions that were not glaciated—sea levels have remained relatively stable. [21]

MYTH 4: ***Extreme weather phenomena are increasing due to global warming.***

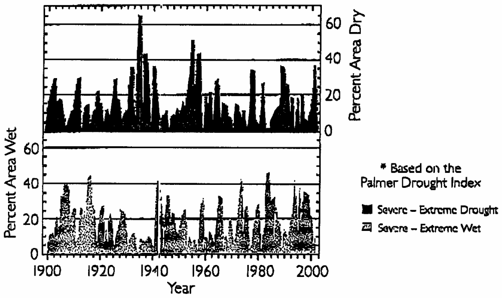
FACTS: In congressional testimony, David Legates, an expert hydrology researcher, said, "Ascertaining anthropogenic changes to these extreme weather events is nearly impossible. Climate models cannot even begin to simulate storm-scale systems, let alone model the full range of year-to-year variability ... Clearly, claims that anthropogenic global warming will lead to more occurrences of droughts, floods, and storms are wildly exaggerated." [22]

The American Association of State Climatologists concurs. According to the Association, "climate predictions have not demonstrated skill in projecting future variability and changes in such important climate conditions as growing season, drought, flood-producing rainfall, heat waves, tropical cyclones and winter storms." [23]

There has been no evidence for an increase in catastrophic hurricanes. Indeed, maximum wind speed in Atlantic hurricanes decreased

Figure 3: U.S. Percentage Area Wet or Dry

January 1900 – April 2001



Source: National Climatic Data Center / NESDIS / NOAA from 1944 to 2000. After normalizing costs for inflation and population change, researchers have found that the biggest losses in the U.S. due to hurricanes occurred before 1950. "[B]y far the biggest decade during the last active era was the 1940s, where five major hurricanes made landfall in Florida. This contrasts dramatically with the very low activity of the 1970s, 1980s and 1990s."[24]

The occurrence of floods and droughts has also remained stable over the past century as can be seen in Figure 3 from the National Oceanic and Atmospheric Administration.[25] The graph shows the percentage of the U.S. that is either in severe drought or flood every year. These events are cyclical and show no increase in either severity or frequency during the latter half of the 20th Century.

MYTH 5: ***Global warming threatens fragile ecosystems and may cause threatened and endangered species to become extinct.***

FACTS: Since the end of the last ice age approximately 10,500 years ago, life on earth has been adapting to the warming environment. Ecosystems are necessarily flexible and shift in response to environmental stimulus including climate change.[26] The advance and retreat of various ice ages can be catalogued by the changes in the range of various plant and animal species. Scientists have documented these changes through fossil records, lakebed silt deposits, soil deposits, and other mechanisms.[27] As our climate continues to change, different ecosystems will expand and contract in response.

Plants increase their productivity in response to increased levels of atmospheric carbon dioxide.[28] Furthermore, plants growing in an atmosphere enriched with carbon dioxide show an increase of up to 5°C in their optimal growth temperature.[29] Over the past two decades, NASA satellites have found that the earth has been getting progressively greener. Their study published in *Science* "proposes climatic changes as the leading cause for the increases in plant growth over the last two decades, with lesser contribution from carbon dioxide fertilization and forest species re-growth."[30] This is important for ecosystems because richness is more positively correlated with plant productivity than any other variable.[31]

MYTH 6: ***The U.S. Government believes the theory of global warming has been proven and supports capping greenhouse gas emissions.***

FACTS: The U.S. government has invested $45 billion in global warming research over the last decade.[32] Despite the enormous outlay of money, the administration concedes the science is still inconclusive. In a report to the president, the National Academy of Sciences wrote, "Because there is considerable uncertainty in current understanding of how the climate system varies naturally and reacts to emissions of greenhouse gases and aerosols, current estimates of the magnitude of future warming should be regarded as tentative and subject to future adjustments upward or downward."[33]

Due to scientific uncertainty, no U.S. president has endorsed the Kyoto Protocol on global climate change. The Senate has not ratified the Kyoto treaty, citing the economic certainty that our economy will be seriously harmed from regulating greenhouse gases. Meanwhile, the U.S. government has continued funding for climate change research and supporting programs for development and dissemination of clean energy technology.

MYTH 7: ***Adhering to the Kyoto Protocol and other carbon dioxide reduction schemes will decrease earth's temperatures.***

FACTS: If global warming is indeed occurring, "According to the world's most advanced climate model, full implementation of the Kyoto Protocol [reducing CO_2 emissions an average of 5 percent below 1990 levels] would avoid only 14/100ths of a degree C of global warming by 2100—probably too small an amount for scientists to detect. Since the United States produces 25 percent of the world's greenhouse gases, U.S. compliance with Kyoto would offset global warming by a hypothetical 35/1000ths of a degree C."[34]

According to some researchers, it would take 40 Kyoto Protocols to avert any meaningful temperature rise, as projected by the models.[35] Furthermore, the major growth in greenhouse gas emissions will come from developing economies, especially China and India. These countries are exempted from international treaties requiring greenhouse gas reduction, so any action taken by the developed world will be quickly swamped by inaction in third world countries.

MYTH 8: ***Adhering to the Kyoto Protocol and other carbon dioxide reduction schemes will be relatively inexpensive.***

FACTS: Carbon dioxide is the inevitable byproduct of burning fossil fuels and therefore any attempt to cap it will result in energy rationing. According to the Energy Information Administration (EIA), fossil fuels supply 70 percent of the U.S.'s electricity and 84 percent of our total energy usage.[36]

The EIA estimated the cost of reducing carbon emissions 7 percent below 1990 levels, as required by the Kyoto Protocol, would be expensive. For the average American, electricity costs would increase as high as 86 percent; gasoline prices could rise 66-cents a gallon, and fuel oil prices would rise as much as 76 percent higher while natural gas prices would rise by as much as 147 percent over baseline.[37] Because energy is such a fundamental part of our economy, the price of manufactured goods and food would also increase. This works out to a total GDP loss of anywhere from $100 billion to $400 billion per year.[38] The people hurt most by rising prices are the poor and elderly living on fixed incomes.

MYTH 9: ***Multi-pollutant regulation, which includes both EPA criteria pollutants and greenhouse gases, is an inexpensive, "no-regrets" method for improving air quality.***

FACTS: "During the 30-year period from 1970 through 2000, total emissions of the six principal ("criteria") pollutants EPA regulates under the Clean Air Act decreased 29 percent, while vehicle miles traveled increased 143 percent, total energy consumption increased 45 percent, and coal consumption increased 106 percent. Automobile and equipment turnover will continue to produce substantial air quality improvement under current regulatory law in the foreseeable future."[39]

"CO_2 is neither an 'ambient' air pollutant like NO_x and SO_2, nor a 'hazardous' air pollutant like mercury. It does not foul the air, impair visibility, contribute to respiratory disease, or bio-accumulate as a toxin in fish."[40]

The Energy Information Administration estimated the cost of reducing NO_x and SO_2 emissions 75 percent below 1997 levels by 2005 would cost $6 billion. Reducing CO_2 emissions 7 percent below 1990 levels by 2005 would cost $77 billion. If all three emission requirements were coordinated, the total cost would be $77 billion—a savings of $5 billion over their aggregate costs if each requirement were imposed one at a time. However, if the goal is cleaner air, in reality you would have spent $77 billion for SO_2 and NO_x reduction that should have only cost $6 billion to begin with.[41]

MYTH 10: ***Renewable energy technology can immediately replace all fossil fuels.***

FACTS: To reach the Kyoto Protocol targets of reducing CO$_2$ emissions 7 percent below 1990 levels, the U.S. would need to cut fossil fuel use by 15 percent and total energy use by 13 percent.[42] However, only 2 percent of our energy is currently derived from renewable sources (excluding hydropower). If fossil fuels, uranium, and hydropower continue to be dismissed by alternative energy advocates, then we must rely on the remaining renewable sources.

To meet the world's energy needs in 50 years, we would need to produce three times the amount of energy currently being made by fossil fuels. One of the main proponents of global warming theory, Dr. Tom Wigley of the National Center for Atmospheric Research, wrote, "Energy sources that can produce 100 percent to 300 percent of present world power consumption without greenhouse emissions do not exist operationally or as pilot plants."[43]

Furthermore, according to Dr. Sallie Baliunas, "[T]he physical laws make the prospect for renewables on a grand scale look dismal: solar and wind energy have enormous environmental footprints. Because they are dilute and intermittent sources, they require great acreage. Their intermittency requires that stable power sources like coal or uranium provide the steady base ... Biomass power requires frequent clear cutting of areas devoted to growing fast-rotation woody crops ... Opportunities for power from geothermal sites and landfills are limited, and decline with use."[44]

"Hydrogen fuel cells, often touted as another futuristic renewable, merely carry energy. Fuel cells are not sources of energy. The process of energizing a fuel cell with hydrogen cracked from methane ... or water requires more energy than returned from use of the fuel cell."[45]

Alternative energy will continue to be developed and play an important, if small role, in diversifying our energy resources. However, "[s]olar, wind and biomass energy cannot be counted on to provide the timely, reliable, inexpensive electricity resources the U.S. needs."[46] ▣

This article was updated from its original publication in Energy, Environment, and Economics: A Guide for State Legislators, The Third Edition *(American Legislative Exchange Council, January 2005).*

Endnotes

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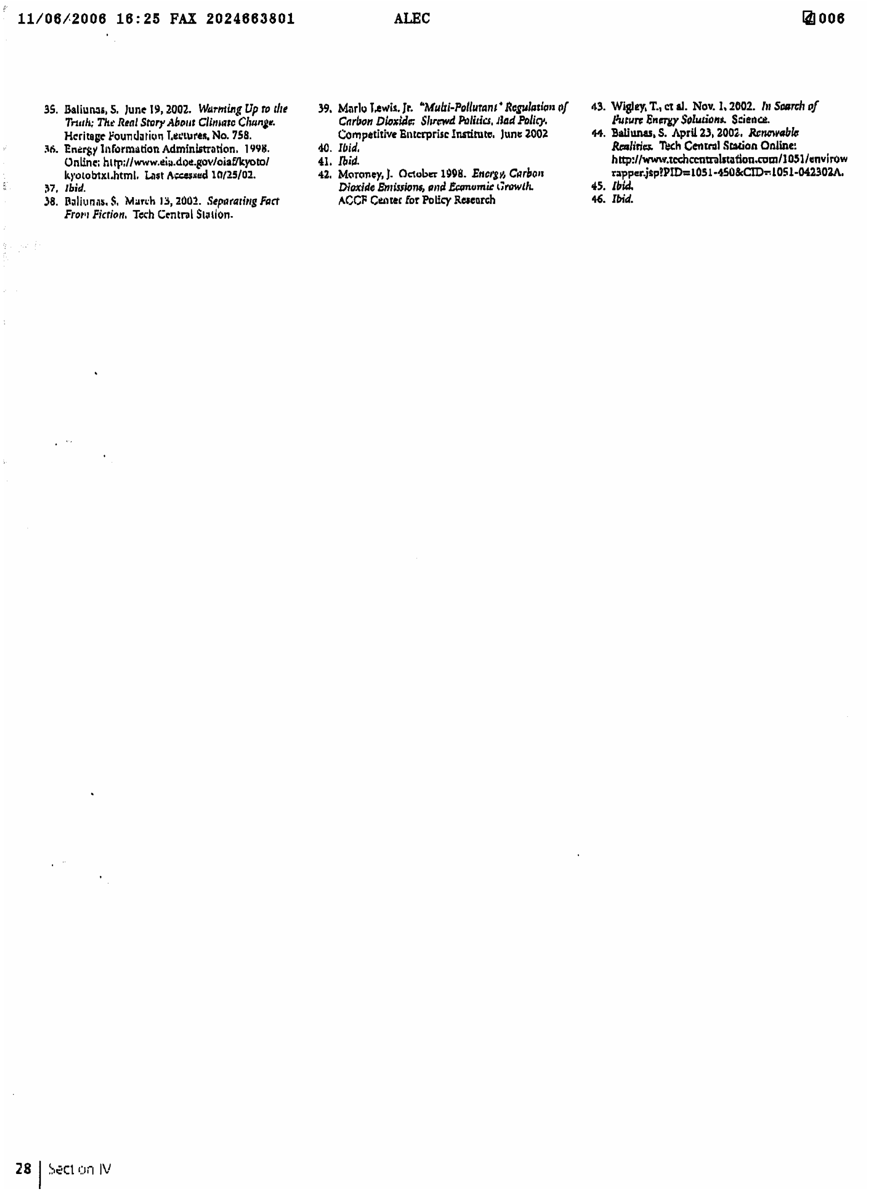

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28 | Section IV

MR. INSLEE. Mr. Chairman, I would like to submit an essay. It is published in *Science* called the Scientific Consensus of Climate Change. It relates to that 928 papers as well as the article I just made reference to. Thank you.

MR. WHITFIELD. Okay. Without objection. And we will keep the record open for 30 days.

[The information follows:]

ESSAY

BEYOND THE IVORY TOWER

This year's essay series highlights the benefits that scientists, science, and technology have brought to society throughout history.

The Scientific Consensus on Climate Change

Naomi Oreskes

Policy-makers and the media, particularly in the United States, frequently assert that climate science is highly uncertain. Some have used this as an argument against adopting strong measures to reduce greenhouse gas emissions. For example, while discussing a major U.S. Environmental Protection Agency report on the risks of climate change, then–EPA administrator Christine Whitman argued, "As [the report] went through review, there was less consensus on the science and conclusions on climate change" (*1*). Some corporations whose revenues might be adversely affected by controls on carbon dioxide emissions have also alleged major uncertainties in the science (*2*). Such statements suggest that there might be substantive disagreement in the scientific community about the reality of anthropogenic climate change. This is not the case.

Without substantial disagreement, scientists find human activities are heating the Earth's surface.

The scientific consensus is clearly expressed in the reports of the Intergovernmental Panel on Climate Change (IPCC). Created in 1988 by the World Meteorological Organization and the United Nations Environmental Programme, IPCC's purpose is to evaluate the state of climate science as a basis for informed policy action, primarily on the basis of peer-reviewed and published scientific literature (*3*). In its most recent assessment, IPCC states unequivocally that the consensus of scientific opinion is that Earth's climate is being affected by human activities: "Human activities … are modifying the concentration of atmospheric constituents … that absorb or scatter radiant energy. … [M]ost of the observed warming over the last 50 years is likely to have been due to the increase in greenhouse gas concentrations" [p. 21 in (*4*)].

IPCC is not alone in its conclusions. In recent years, all major scientific bodies in the United States whose members' expertise bears directly on the matter have issued similar statements. For example, the National Academy of Sciences report, *Climate Change Science: An Analysis of Some Key Questions*, begins: "Greenhouse gases are accumulating in Earth's atmosphere as a result of human activities, causing surface air temperatures and subsurface ocean temperatures to rise" [p. 1 in (*5*)]. The report explicitly asks whether the IPCC assessment is a fair summary of professional scientific thinking, and answers yes: "The IPCC's conclusion that most of the observed warming of the last 50 years is likely to have been due to the increase in greenhouse gas concentrations accurately reflects the current thinking of the scientific community on this issue" [p. 3 in (*5*)].

Others agree. The American Meteorological Society (*6*), the American Geophysical Union (*7*), and the American Association for the Advancement of Science (AAAS) all have issued statements in recent years concluding that the evidence for human modification of climate is compelling (*8*).

The drafting of such reports and statements involves many opportunities for comment, criticism, and revision, and it is not likely that they would diverge greatly from the opinions of the societies' members. Nevertheless, they might downplay legitimate dissenting opinions. That hypothesis was tested by analyzing 928 abstracts, published in refereed scientific journals between 1993 and 2003, and listed in the ISI database with the keywords "climate change" (*9*).

The 928 papers were divided into six categories: explicit endorsement of the consensus position, evaluation of impacts, mitigation proposals, methods, paleoclimate analysis, and rejection of the consensus position. Of all the papers, 75% fell into the first three categories, either explicitly or implicitly accepting the consensus view; 25% dealt with methods or paleoclimate, taking no position on current anthropogenic climate change. Remarkably, none of the papers disagreed with the consensus position.

Admittedly, authors evaluating impacts, developing methods, or studying paleoclimatic change might believe that current climate change is natural. However, none of these papers argued that point.

This analysis shows that scientists publishing in the peer-reviewed literature agree with IPCC, the National Academy of Sciences, and the public statements of their professional societies. Politicians, economists, journalists, and others may have the impression of confusion, disagreement, or discord among climate scientists, but that impression is incorrect.

The scientific consensus might, of course, be wrong. If the history of science teaches anything, it is humility, and no one can be faulted for failing to act on what is not known. But our grandchildren will surely blame us if they find that we understood the reality of anthropogenic climate change and failed to do anything about it.

Many details about climate interactions are not well understood, and there are ample grounds for continued research to provide a better basis for understanding climate dynamics. The question of what to do about climate change is also still open. But there is a scientific consensus on the reality of anthropogenic climate change. Climate scientists have repeatedly tried to make this clear. It is time for the rest of us to listen.

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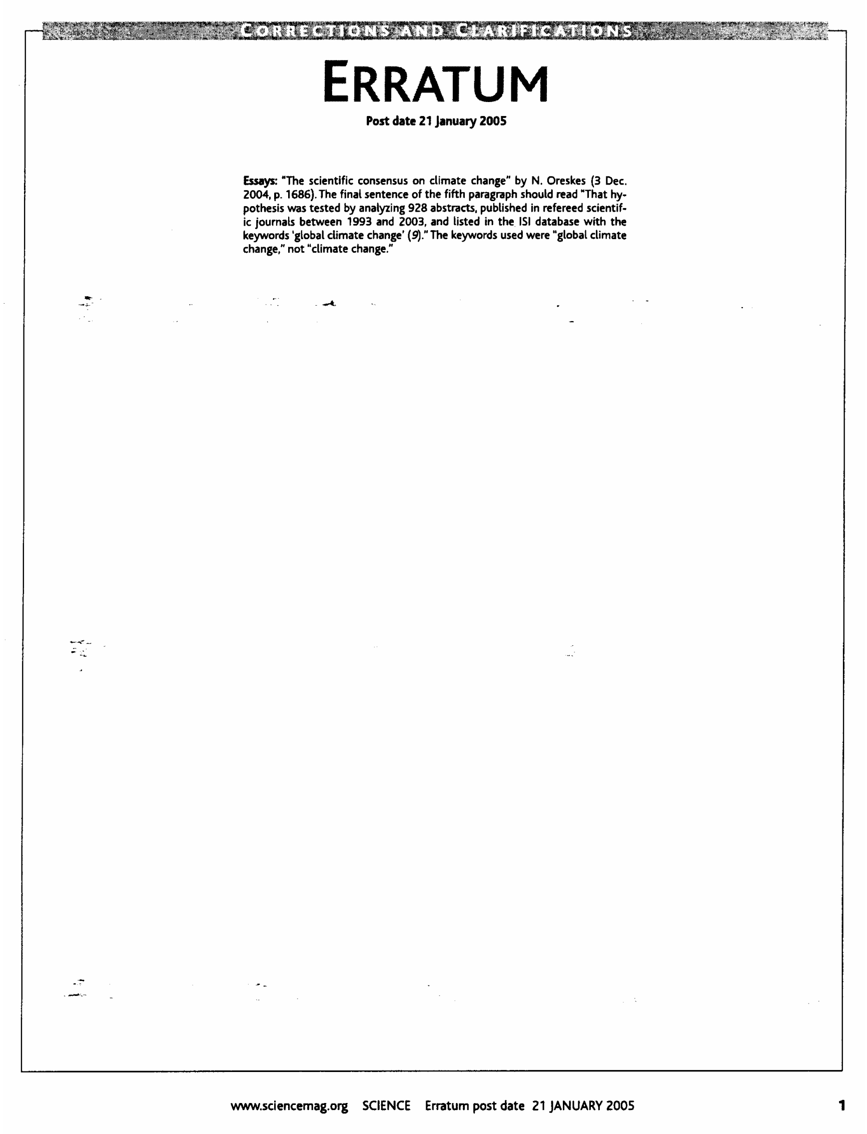
9. The first year for which the database consistently published abstracts was 1993. Some abstracts were deleted from our analysis because, although the authors had put "climate change" in their key words, the paper was not about climate change.
10. This essay is excerpted from the 2004 George Sarton Memorial Lecture, "Consensus in science: How do we know we're not wrong," presented at the AAAS meeting on 13 February 2004. I am grateful to AAAS and the History of Science Society for their support of this lectureship; to my research assistants S. Luis and G. Law; and to D. C. Agnew, K. Belitz, J. R. Fleming, M. T. Greene, H. Leifert, and R. C. J. Somerville for helpful discussions.

10.1126/science.1103618

Published by AAAS



CORRECTIONS AND CLARIFICATIONS

ERRATUM

Post date 21 January 2005

Essays: "The scientific consensus on climate change" by N. Oreskes (3 Dec. 2004, p. 1686). The final sentence of the fifth paragraph should read "That hypothesis was tested by analyzing 928 abstracts, published in refereed scientific journals between 1993 and 2003, and listed in the ISI database with the keywords 'global climate change' (*9*)." The keywords used were "global climate change," not "climate change."

www.sciencemag.org SCIENCE Erratum post date 21 JANUARY 2005 1

MR. WHITFIELD. Thank you all again for your testimony. We look forward to working with you as we move forward. That concludes today's hearing.

[Whereupon, at 6:48 p.m., the subcommittee was adjourned.]

RESPONSE FOR THE RECORD OF DR. MICHAEL E. MANN, ASSOCIATE PROFESSOR AND DIRECTOR, EARTH SYSTEM SCIENCE CENTER

Question No. 1. I understand that although your current practice is to make your computer code available publicly, many researchers in your field do not do so. Although computer code may not have commercial value, why would a researcher not want to release his code?

Answer:

This is a question that my colleagues and I have wrestled with over the years. As the question acknowledges, for the past five years or more, my colleagues and I have made public our computer codes, just as we made public our code for the 1998 study last year. Our decision to make our code public comes at a time when there is increased standardization in codes, and the need to tailor codes to accommodate the various and often idiosyncratic computer systems that were used in the 1990s has diminished. But even today, many, perhaps most, climate scientists do not share their codes. In my view, there are legitimate reasons for reaching that decision, even though it is not the decision my colleagues and I have made.

For one thing, most code is written to enable scientists to perform specific functions, and thus code is generally written in a form of short-hand that is not easily understood by others. To make code usable by other researchers, the code writer has to undertake significant additional work, in the form of documentation, testing for potential platform dependence, tidying, and so forth, that places a significant burden on the code writer. Many scientists do not think that undertaking that additional burden is worth it.

Second, access to computer codes is not necessary to replicate a study. I realize that some of my critics have argued otherwise, but it is just not the case that scientists need access to computer codes to replicate studies. As I tried to make clear in my testimony before the Committee, a study may be replicated if the scientists conducting the initial study make available both the underlying research data and an algorithm that gives a step-by-step account of how that data was analyzed. As my testimony pointed out, the 1998 and 1999 work by my colleagues and me was recently replicated by a team of scientists (Wahl and Ammann) who did not have access to our codes, but who were able to replicate our work without difficulty. So replication does not depend on access to computer codes.

Moreover, scientists, like entrepreneurs, corporations, and others engaged in the production of intellectual capital, are competitive, and rightly so. Competition in the marketplace of ideas is what science is all about. We would all like to make our greatest possible contributions to advancing the forefront of our scientific disciplines. Indeed, we are rewarded (in terms of grants, promotions, academic recognition, and do forth) in proportion to the contributions we make in the advancement of science. Asking scientists to release their codes before they have had an opportunity to apply them to a number of potential interesting problems is asking them to sacrifice their competitive advantage. This would be no different than asking Microsoft to release the code for its latest operating system as soon as it reaches the market. Microsoft is not about to do that, and most people would consider a requirement that Microsoft freely dispense its intellectual property --- its codes --- as antithetical to the principles of a free market. The argument is no different in the case of scientists and their computer codes or other tools of their trade.

Question No. 2. Dr. Wegman states that paleoclimatologists do not interact with statisticians. Do you have any response to that statement? What steps, if any, is the paleoclimatology field taking to ensure that it is using appropriate statistical methodologies?

Answer:

Unfortunately, Dr. Wegman made this claim without engaging in any effort to ascertain the extent to which climate scientists interact with statisticians. To the contrary, Dr. Wegman simply assumed --- without data, indeed, without any basis at all --- that climate scientists, and paleoclimatologists in particular, do not interact with statisticians.

Dr. Wegman's accusation could not be further from the truth. The participation of statisticians in climate science has become so routine that there is an entire field of climate research known as "statistical climatology," which involves the collaboration of large numbers of statisticians and climate scientists. There are even textbooks dedicated to the study of statistical climatology. In his testimony before the Committee, Dr. Hans Von Storch found it necessary to inform Dr. Wegman of this fact. And Dr. Von Storch should know; he and Dr. Francis Zwiers (a Ph.D. statistician specializing in climate applications) have written one widely used textbook on statistics and its applications to climate studies. Another statistician, Professor Dan Wilks of Cornell University, has written an additional textbook on statistics and its applications to the atmospheric sciences.

The extensive collaboration between climate scientists and statisticians is also reflected in the academic literature. Hundreds of papers have been published in the climate and paleoclimate literature involving the collaboration of statisticians and climate scientists. These are all publicly available and could have been identified by Dr. Wegman in a few hours of research. Two members of the NRC committee that reviewed paleoclimate reconstructions in its recent report (Dr. Douglas Nychka and Dr. Peter Bloomfield) are statisticians (both of their doctorates are in statistics) who have published in the climate literature and who have actively collaborated with climate scientists.

Had Dr. Wegman bothered to make even the slightest inquiry, he would have found that there are in fact many statisticians (that is, individuals with doctorates in statistics) who have been and remain active members of the community of researchers in the areas of atmospheric science and climate research. Even a cursory review of the structure of our community reveals this readily. I have been informed that many of my statistical climatologist colleagues are deeply offended by Dr. Wegman's unfounded pronouncements to this Committee, pronouncements which effectively deny their contribution to the advancement of science.

Moreover, the American Meteorological Society --- the leading professional organization of atmospheric scientists --- has a Committee on Probability and Statistics, and members of the committee are drawn from both atmospheric/ocean/climate scientists and statisticians. I was a member of that committee for a 3-year term (2003-2005) that recently ended. The committee's website can be found here: http://www.isse.ucar.edu/ams/ams_ps.html, and the committee members' biographies are available here: http://www.isse.ucar.edu/ams/ams_ps.html#members. The chair of the committee, Dr. Rick Katz is a statistician (with his doctorate in statistics from Penn State University) and senior scientist at NCAR. Other statisticians on the committee include Dr. Tilmann Gneiting (Department of Statistics, University of Washington), and Dr. William Briggs (Adjunct Assistant Professor of Statistical Science, Cornell University). These statisticians are active members of the climate research community.

Equally important, one of the primary centers for climate research in the U.S., NCAR, has maintained a thriving Geophysical Statistics Project ("GSP"), which was founded more than a decade ago. This program has been funded by the National Science Foundation's Division of Mathematical Sciences, which has recognized for some time the importance of encouraging statisticians to collaborate actively with atmospheric scientists/climate scientists. I participated as a graduate student in GSP's inaugural workshop in 1994. Many leading statisticians (*e.g.*, Dr. Grace Wahba, Dr. Arthur Dempster, and Dr. Noal Cressie) were participants. The GSP has since thrived, providing an important opportunity for collaboration between statisticians and climate researchers. More information can be found at the GSP webpage: http://www.image.ucar.edu/GSP/.

It bears noting that the project has now produced more than two dozen Ph.D. statisticians who have become active researchers in the atmospheric, oceanographic, and climate sciences. Its members and visitors have included dozens of statisticians who have worked collaboratively with atmospheric scientists and climate researchers. The leader of the project, Dr. Douglass Nychka, was one of the members of the aforementioned NRC panel. He was also a consultant in the recent paper by Wahl and Ammann that refutes the oft-cited criticisms of the Mann et al. work by McIntyre and McKitrick.

Question No. 3. Dr. Wegman has hypothesized that the peer review process failed and allowed publication of your 1998 and 1999 studies without adequate vetting of the study. This was based in part on his social network analysis that showed you have connections with 42 other authors in paleoclimatology. Of the 42 co-authors identified by Dr. Wegman, how many of them were co-authors with you in or before 1999?

Answer:

Dr. Wegman's accusations are so riddled with flaws that it's hard to know where to begin in response. But let me first address the specious accusation by Wegman that the peer-review process somehow "failed" with respect to our '98 and '99 studies. It is bewildering that Dr. Wegman (who has no expertise in the area of atmospheric science/climate, and indeed was wholly unable to correctly answer some of the most basic questions about climate science during the hearings) would characterize the publication of our work as a "failure." One would assume that an academic would avoid rendering judgments in fields in which he is demonstrably unknowledgeable. Certainly the scientific community has reached the precisely the opposite conclusion. Our 1998 and 1999 studies are widely cited, and the conclusions stated in them have been repeatedly reaffirmed. Just one example of the scientific support for these works should suffice: The National Research Council panel in their recent Report characterized our study as "groundbreaking", and the panel concluded that its key conclusions have held up over nearly a decade of exhaustive and independent follow-up research. That is a pretty good track record by any standard. Thus, judged by experts who understand climate studies, Wegman's efforts to disparage our work as "failed" are nothing short of silly.

Let me next address Wegman's equally specious and unsupported claim that scientists who work in a given field cannot objectively review the work of their colleagues and competitors in that field. By way of illustration, I have attached (as Attachment 1 to these Responses) the famous 1927 photograph of attendees of the Solvay Physics meeting in Brussels. It shows a group of 29 physicists engaged in a collegial, small conference. Virtually every attendee was a driving figure behind our understanding of modern physics. Appearing in the photograph are Einstein, Heisenberg, Bohr, Fermi, Dirac, de Broglie, Born, Pauli, Langmuir, Planck, Curie, Compton, Ehrenfest, Lengevin, and others of equal prominence. The members of this group all knew each other, worked with each other, collaborated on research with one another, visited each other, went mountain-climbing together, and so forth. Familiarity did not compromise their contributions to science. While I do not claim that the group I collaborate with is likely to duplicate the feats of the scientists who gathered in Brussels 80 years ago, the point remains --- scientific collaboration does not turn scientists into timid lapdogs unwilling to criticize the work of their colleagues.

Let me turn now to the specifics of the question. It is baffling how Dr. Wegman arrived at the number (42) he used to describe my co-authors. One would think that a statistician could do simple arithmetic. My curricular vitae (CV) is available on the internet, and it is clear that Wegman consulted it (but not me) in the preparation of his paper. Nonetheless, none of the numbers he uses add up. Part of the problem may stem from Wegman's ill-advised effort to distinguish between authors engaged in "paleoclimatology" and "climatology," since most climate researchers have worked, in some manner, on some aspect of paleoclimate. So the distinction he attempts to draw between "paleoclimatologists" and "climatologists" is illusory at best. This too underscores the hazards of an amateur seeking to draw conclusions in a field in which he has no expertise.

But to answer the question Wegman poses, let us consider the correct numbers (see Attachment 2 to these Reponses) which are based on all of my peer-reviewed journal publications as listed on my CV (and not including "gray literature" such as book chapters, encyclopedia pieces, reports, conference proceedings, letters to editors, opinion pieces). I published with 10 co-authors prior to 1993 based on my undergraduate research in solid state physics. These publications are unrelated to climate research, and are not included.

So let us consider just my climate-related papers (i.e., post 1993), as Wegman purports to do. In climate research, I had 14 co-authors through the year 1999. I had 101 co-authors through the end of 2005. So Wegman's calculations, based on 42 co-authors, are off-base by more than a factor of two. Wegman also appears to have made even more fundamental errors in his review of the science (a point I address below).

But I believe the question goes to how influential I was in the field, in a relative sense, at the time of publication of my '98 and '99 studies. After all, Wegman claims that there is, in essence, an almost sinister conspiracy of like-minded climate scientists who act as a cartel to control the published literature in climate studies. And his "proof" is the fact that I have published with many prominent scientists who, in Wegman's view, would be unwilling to criticize my 1998 and 1999 work even if it were seriously flawed. But this theory does not wash. Apart from the fact that even my closest collaborators are perfectly willing to criticize my work when they think it is flawed, Wegman's math just does not support his theory. As indicated above, the vast majority (86%) of my co-authorships occurred *after* my 1998/1999 studies. So Wegman's effort to suggest that I was influential in the field at the time these studies were published, or in the aftermath of their publication, cannot be squared with the data, and is, in fact, nothing short of absurd.

Question No. 4. Does the scientific community rely exclusively or primarily on the peer review process conducted before an article is published to test the robustness and validity of new scientific discoveries or theories? Or does the development of science depend on an iterative process that involves not only peer review before publication, but also review and competing research and analysis by other scientists after publication?

Answer:

This question raises an important issue that was unfortunately not adequately aired at the hearing. Dr. Wegman and others have expressed the view that the scientific community somehow places exclusive reliance on the peer review process as the determinant of scientific truth. But the peer review process is hardly the only, let alone most important, way that the scientific community tests the accuracy and reliability of scientific papers. Indeed, Wegman's contention reflects a fundamental lack of understanding of the basic principles that govern the scientific discipline. Science progresses through an open, self-correcting process whereby scientists place their ideas in the marketplace, typically by publishing articles in peer review journals. The peer review process ensures only that basic mistakes are not made, that the article acknowledges the existing literature on the subject, and that it contributes in some way to the exploration of important scientific issues. But peer review does not and cannot vouch for the accuracy of the paper. That is the function of the scientific process, by which other scientists test out and question the work of their peers. Some ideas stand the test of time; others do not. Copernicus was proven right over time; Ptolemy's conception that the Earth forms the center of the universe was proven wrong. Much of Einstein's work has stood up to reevaluation, but some of his theories have been proven to be incorrect as well.

It is relevant in this context to again emphasize that the key conclusion that my colleagues and I drew tentatively in our work in the late '90s --- that late 20th century Northern Hemisphere average warmth was *likely* unprecedented in at least the past 1000 years --- has held up for more than a decade, after dozens of independent studies have reexamined that claim. So it has passed this important test of time. The peer-review process is simply a quality control process to make sure that claims, theories, and ideas that are self-evidently flawed from the beginning do not clutter the pages of the legitimate peer-reviewed scientific journals, that is, to ensure that published papers have *potential* merit. Peer review is a simple first step at quality control. It does not, nor should it, be considered evidence that the conclusions of a particular published paper are accurate or not. No single paper should ever be used to establish the validity of a particular hypothesis or conclusion. The accuracy of claims, hypotheses, conclusions, indeed theories, can only be established by examining the collective body of peer-reviewed research to date on any particular topic, and the overall thrust of that body of research. Indeed, the importance of broad-based scientific assessments (such as those provided by the Intergovernmental Panel on Climate Change or "IPCC") is to evaluate the entire body of peer-reviewed literature on a particular topic and to determine the consensus, if there is one, that emerges in that body of literature.

Question No. 5. Should all scientific papers be withheld from publication until the results are independently replicated?

Answer:

This question also raises an important issue that was not adequately aired at the hearing. Once again, Dr. Wegman and others suggested at the hearing that scientific papers be shelved for the time it takes for the results to be verified independently. This view is misguided, and, if followed, would seriously undermine the development of scientific knowledge. It takes considerable time to replicate a study. Meanwhile, important findings that ought to be disseminated widely to the scientific community would be unavailable to other scholars. Such a requirement would dangerously slow the progress of science.

As I explained above, in my view development of scientific knowledge can take place only through an open, self-correcting process whereby scientists put out ideas, other scientists test them, and those ideas which stand up to future tests survive while those that do not are ultimately rejected. It is important in this context that ideas with potential merit be placed in the scientific discourse in a timely manner, so that they can be followed up in a timely manner by the entire scientific community and not just a few researchers engaged in replication, and the scientific process can proceed at an appropriate pace. Were the suggested requirement to be followed where all papers required independent replication before publication, this would bog down the scientific process to a near standstill.

In data-poor areas of science such as paleoclimatology, the added benefit of new data is much more valuable than the pure replication of a past study. "Replication" in a pure sense provides very poor value for money. A good example would be the now- famous GRIP and GISP2 ice cores from Greenland. These are two different Greenland ice cores that were drilled at two nearby but distinct locations by two different (one U.S. and one European) teams. Had the total available funding simply been used for both teams to drill cores at the same site, and thereby replicate each other's work, only the technical accuracy of the coring would have been validated. Instead, the reproduction of a record that was nearby but separate gave both support to the main results, but also allowed the groups to discover a mix-up in dating prior to 100,000 years ago in one of the two cores. So drilling two different ice cores, rather than drilling from the same source twice, proved to be a far more valuable use of the available funding and resources.

The proponents of this idea also ignore the near-impossibility of its implementation. How would scientists be persuaded to replicate the unpublished work of others? What would their incentives be to conduct this work quickly, especially if it meant sacrificing the time researchers would prefer to spend on their own work? Would every study be subject to replication? Or only important studies? And who would decide which studies required replication prior to publication? Who would pay for these replications? Would the government pay for them? Is Congress prepared to double the size of research budgets for all of the major scientific funding agencies (e.g. NSF, NIH, NOAA, etc.)? And these practical problems are only the tip of the iceberg.

My essential plea here is that Congress should not fix that which is not broken. Since Copernicus' time the scientific process has successfully weeded out the wheat from the chaff. It would be dangerous for Congress or any government body to tamper with that process.

There is another element of this question which raises a deeply troubling matter with regard to Dr. Wegman's failure to subject his work to peer review, and Wegman's apparent refusal to let other scientists try to replicate *his* work. Professor David Ritson, Emeritus Professor of Physics, Stanford University, has found error in the way that Dr. Wegman models the "persistence" of climate proxy data. Interestingly, this is the same error Steven McIntyre committed in his work, which was recently refuted in the paper by Wahl and Ammann, which was in turn vetted by Dr. Douglass Nychka, an eminent statistician. Dr. Ritson has determined that that the calculations that underlie the conclusions that Dr. Wegman advanced in his report are likely flawed. Although Dr. Ritson has been unable to reproduce, even qualitatively, the results claimed by Dr. Wegman, he has been able to isolate the likely source of Wegman's errors. What is so troubling is that Dr. Wegman and his co-authors have ignored repeated collegial inquiries by Dr. Ritson and apparently are refusing to provide any basic details about the calculations for the report (see Attachments 3 and 4 to this Response). It would appear that Dr. Wegman has completely failed to live up to the very standards he has publicly demanded of others.

Moreover, the errors that Dr. Ritson has identified in Dr. Wegman's calculations appear so basic that they would almost certainly have been detected in a standard peer review. In other words, had Dr. Wegman's report been properly peer-reviewed in a rigorous process where peer-reviewers were selected anonymously, it likely would not have seen the light of day. Dr. Wegman has thus unwittingly provided us with a prime example of the importance of the peer review process as a basic first step in quality control.

RESPONSE FOR THE RECORD OF DR. JOHN R. CHRISTY, PROFESSOR AND DIRECTOR, EARTH SYSTEM SCIENCE CENTER, NSSTC, UNIVERSITY OF ALABAMA IN HUNTSVILLE

28 August 2006

Hon. Ed Whitfield
Chairman
Subcommittee on Oversight and Investigations
Committee on Energy and Commerce
2125 Rayburn House Office Building
Washington DC 20515-6115

Dear Rep. Whitfield,

Thank you for the opportunity to appear before your Subcommittee to address issues of global climate change. I especially thank you for the opportunity to clarify some of the material that was entered into the official record which appeared to contradict my testimony. I assure you that what I presented was accurate as to my experiences and understanding of climate change in general and dataset construction in particular.

I will be happy and available to answer any further questions regarding my appearance.

Sincerely,

John R. Christy
Director, Earth System Science Center
Alabama State Climatologist
University of Alabama in Huntsville

Questions from Rep. Whitfield for John R. Christy

(1) During the hearing, Mr. Waxman introduced into the hearing record a letter from Frank J. Wentz regarding your sharing of code with Remote Sensing Systems 9RSS). Please explain your interactions with RSS (and Mr. Wentz) and subsequent interactions with Dr. Mann, as mentioned in your testimony.

(1) Answer

In the Hearing before the Subcommittee on Oversight and Investigations of the House Committee on Energy and Commerce on 27 July 2006, I testified about our cooperation with Remote Sensing Systems (RSS) regarding sharing of satellite data and code.

Mr. Waxman introduced into the record a letter from Mr. Frank Wentz of RSS which included an email from me to Mr. Wentz, over 4 years old, implying an apparent lack of cooperation. The problems here are (a) that this March 2002 email to Mr. Wentz from me was simply the *first* in a long series of emails in which we indeed cooperated, and (b) that this exchange related to a different dataset than the one I was speaking of in my testimony. The following discussion describes the way these two datasets were examined by RSS.

Mid-Tropospheric (MT) Temperature Product

Another RSS Scientist, Dr. Carl Mears (not Dr. Mann), began constructing an MT product from the raw microwave digital counts in early 2002, following much of our published methodology. There were some discrepancies between our two results. Mr. Wentz asked for the code with which we constructed our MT data so as to resolve these differences. As stated in my *first* email on the subject, shared by Mr. Waxman in Mr. Wentz's letter, I declined to send the code for the reasons given. However, there were many further exchanges of information (in terms of the Hearing language: there were discussions about the "algorithms") to the point that RSS understood the three main differences between our two datasets. Mr. Wentz's description of "trial and error" in his letter in this process left out the important point that we were in constant communication on the details and subtleties of the dataset construction process.

During this time, we discussed at great length matters concerning (1) the methodology of calculating the strength of the target-temperature effect, (2) the methodology of determining intersatellite biases and to a lesser extent (3) the adjustments for the satellites' east-west drifting (diurnal effect.)

At a conference in Asheville NC, (Oct. 2003) Dr. Mears presented a talk entitled "Understanding the difference between the UAH and RSS retrievals of satellite-based tropospheric temperature estimate" and stated he was satisfied as to having understood the main reasons for the differences between our two datasets. Indeed in this presentation, Dr. Mears used some of the adjustment files we had provided to them to help answer questions of how our adjustment process worked (i.e. diurnal drift files.) He also displayed our target factor calculations, again provided to RSS, along with a detailed description of their computation. It was clear we had provided information to understand the discrepancies.

RSS was also able to publish these findings and results (Mears et al. 2003). I was a reviewer of that paper and recommended publication. In my view, this closed the episode on this dataset.

Lower Tropospheric (LT) Temperature Product

In 2005, Dr. Mears also led in the development of a different temperature product, LT, which UAH had been producing since 1992. He addressed the issues of hot target calibration coefficients and intersatellite biases to his satisfaction but was unable to replicate our diurnal effect. He asked for more information and we supplied the appropriate section of the code and intermediate adjustment files so he could test the code against the output. With these in hand he was able to discover the artifact in the algebra which created the error most visible in the tropics.

That we supplied these items is inarguable as the paper published by Mears and Wentz (2005) in *Science* displays the UAH adjustment files. Additionally, even though we did not know the outcome of their study at the time, I granted permission to publish our files as shown by this following exchange between Dr. Mears and myself on 13 May 2005 in which he responds to me for being open in this way.

> 13 May 2005 8:41 p.m.
>
> Hi Carl:
>
> Anyway, something jogged my memory this morning that you had asked about using the UAH diurnal adjustments in a paper, and I didn't respond with a firm answer. Sorry. I think it would be fine to use and critique ... that's sort of what science is all about.
>
> [John Christy]
>
> 13 May 2005 1:58 p.m.
>
> Hi John
>
> Thanks for permission -- I strongly approve of your view of science expressed [above]. I think that things that aren't nutty or poorly explained should be published in the open literature without too much fuss, so that they can then be commented on..... Of course, different people have different opinions about what constitutes nutty.
>
> You[r] global diurnal effect agree[s] pretty much with mine, but it's the *opposite* sign. The real difference is in the tropics. I suspect the same calculation for 20S to 20N will show a much larger effect. With the model-based diurnal correction, the big disagreement with the surface in the tropics goes away.
>
> [Carl Mears Remote Sensing System]

So, the apparent contradiction between my testimony and the letter from Mr. Wentz sprang from a misunderstanding of how two different datasets were being addressed. One (MT) was solved without sharing the specific code but for which we *did* supply ancillary data files and considerable information. The other (LT) needed parts of the code to resolve the discrepancy. In the Hearing, Mr. Waxman dealt with the former while I dealt with the latter. In both cases, however, UAH *did* cooperate with RSS.

Mears, C.A., M.C. Schabel, and F.J. Wentz, 2003: A reanalysis of the MSU channel 2 tropospheric temperature record. *J. Climate*, 16, 3650-3664.

Mears, C.A. and F.J. Wentz, 2005: The effect of diurnal correction on satellite-derived lower tropospheric temperature. *Science*, 309, 1538-1551.

(2) As you were a member of the National Research Council panel that recently issued the report on millennial temperature reconstructions:

- **(a) Where in the report did the panel describe "plausible" as suggesting roughly a 2/3rds probability of being correct.**
- **(b) In the report, did the panel attach probability estimates to the term "plausible"?**
- **(c) Why did the panel choose to use the term "plausible," as opposed for example to terms such as "likely", to describe confidence in millennial temperature reconstructions?**

(2a) Answer

The report did not intend for "plausible" to be equated with "2/3rds" probability of being correct. My view ,as a panel member, is that "plausible" was chosen to indicate a lack of quantifiability in describing confidence in pre-1600 temperatures.

(2b) Answer

"Plausible" was chosen precisely because it implied that probability estimates could not be assigned to pre-1600 temperature estimates due to (a) the limited amount of proxy information available and (b) the unknown confidence with which these proxy records may determine temperature. The current proxies are mostly consistent with the notion that pre-1600 temperatures were cooler than late 20th century temperatures, but the evidence is still too meager and uncertain.

(2c) Answer

As a member of the IPCC 2001 Lead Author team I outlined in my testimony why the word "likely" was chosen in that document. "Likely" in the IPCC 2001 terminology had an estimated likelihood defined as being at least 2/3rds probable. The NRC panel chose "plausible" for reasons given in (2b) above. My view of the NRC report is that our IPCC statement was inadequate in that the IPCC should have separated the last millennium into two periods with higher than "likely" confidence for post-1600 and lower than "likely" confidence for pre-1600 estimates.

(3) When considering the panel's findings that it is "plausible" that recent decades were the warmest in a millennium, is it correct to interpret that to mean the panel's consensus view was that plausible means roughly 2/3rds probability of being correct, as was suggested in the news reports following the press conference releasing the report?

(3) Answer

I was disturbed when reading the press reports that implied the panel had endorsed with "likely" confidence statements about the pre-1600 temperatures. The panel did not conclude that there was a 2/3rds probability that late 20th century warmth was greater than at anytime prior to 1600. As noted above, there are indications that such is the case, but the data do not allow statements of quantifiable confidence to be made at this point.

(4) In your testimony, you mention your recent study relating to California regional temperature trends and human influences on those trends (Christy et al. 2006a). Please describe the purpose and conclusions of that study.

(4) Answer

As a native of Fresno and an avid weather observer since being a teenager there, I had an abiding interest in determining the extent of temperature changes in the Valley. This eventually led to a study funded by the National Science Foundation. The first part of the study was a data gathering effort in which every available long-term dataset for the Valley and nearby Sierras was acquired, many by manual digitization from paper records. The second part was the development of a means to merge all of these data into a regional time series of temperature for daytime and nighttime temperatures separately, for each season separately and for the Valley and Sierras separately.

We discovered that Valley nighttime temperatures were rising rapidly while daytime temperatures were generally falling slightly. In the Sierras however, there were no real significant trends, with perhaps a suggestion of nighttime cooling in summer and fall. This result suggests that the significant changes in the land surface of the Valley (irrigation and perhaps urbanization) are causing the changes in the Valley. The fact there were no long-term changes in the Sierras for this period suggests that the enhanced greenhouse effect has not been a significant factor in Central California in terms of temperature changes. (For regions this small, one must always consider the natural variations of climate as also being an issue with which to deal, but such variations should have affected both Valley and Sierra in the same way.)

(5) Please explain why the measurement of average global (or average hemispheric) temperature change does or does not represent an adequate metric for understanding or predicting the risks of potential climate change impacts.

(5) Answer

Thermometers near the surface will respond to all of the forcing processes that act upon them. Thus, surface temperature over land will show responses to changes such as urbanization and other land-use changes in addition to that of atmospheric forcing from aerosols or greenhouse gases. As a result, it is difficult to extract out the impacts of one particular forcing on surface temperature with high confidence.

Daily temperature is commonly reported as two values, the maximum and minimum, from which the daily average is calculated. Maximum temperature is more relevant for climate change as it occurs when the surface and upper atmosphere are more closely connected through vertical mixing and thus will give a better idea of what the general climate system as a whole is doing. Minimum temperature is more closely related to a shallow layer near the ground and is thus impacted more by urbanization, aerosol pollution and other land-use changes. Thus, daily average temperature is partially dependent on processes that impact minima.

Theoretically, the temperature of the ocean surface is a better quantity to measure in terms of observing a variable that has a more direct relationship to a forcing such as greenhouse gases. However, there are large areas of the ocean that have never been systematically observed over long-periods, and the manner by which ocean temperatures have been taken and the associated biases contain a certain level of uncertainty, especially in the earliest years.

Surface temperature is one metric for assessing climate variations and change, but is less informative than others. Indeed the ability of model simulations to depict surface temperature distributions is quite primitive at this stage. Focusing on the global *average* surface temperature also circumvents the fact that the spatial distribution of those changes is more important than the overall average in terms of risk and impact. For example, our work in California, the SE USA and preliminary work in East Africa indicate models are not able to replicate what the observations since 1900 have shown, though for the global average they are not in great error. Additionally, the lowest layer (or boundary layer) of the atmosphere in which these surface thermometers are positioned, is an extremely complicated part of the climate system which is not well-represented in climate models. Average surface temperature, while valuable in local terms to humans who live on the surface, is a rather limited and complicated variable, compounding its lack of utility in providing a high level of understanding about greenhouse-gas induced climate change.

A much more fundamental measurement needed to assess how various forcing mechanisms are affecting the planet is the heat content, which is essentially the number of joules of energy in the system. So, by counting the number of joules of energy in the deep atmosphere, ocean (mainly upper ocean), and other components such as ice caps, one has access to a better metric for understanding how much extra energy is (or is not) being trapped in the climate system. Knowing the number of joules, however, is still a step removed from knowing whether particular components of the Earth (and human) system might be at "risk" for a significant impact.

It is a very subjective task to address the idea of "risk" of potential impacts of a changing climate (either natural or human-induced) from surface temperature considerations, and as important, the possible impact of specific policies. The various processes that affect surface temperature render it a less-than-optimal gauge of human-induced climate change impacts, even if concentrating on the better measure - daily maximum temperature. Thus, it is even more difficult to assign an observed change in surface temperature to a particular cause.

Questions from Rep. Supak for John R. Christy

(1) In your written testimony, you stated that the poor of the world are more vulnerable to the impacts of poverty, water and air pollution, and political strife (sic) than to whatever the climate does. You also made a plea that the poor of the world not be denied the use of energy. A recent article in the Washington Post recorded the tremendous cost of subsistence farmers and urban dwellers in Peru because of the melting of the glaciers that has caused a water crisis. The loss of glacial ice in the Himalayas will affect 300 million people relying on snowmelt for the water supply. (See attached, "On the roof of Peru, Omens in the Ice; Retreat of Once-Mighty Glacier signals Water Crisis, Mirroring Worldwide Trend," July 29, 2006, A1.)

(a) Is it your position that nothing should be done in the developed world to control its fossil fuel energy use while we wait for development to reach these poor people who are directly suffering today from the effects of climate change?

(b) **What do you propose to protect the poor people of the world today from the effects of climate change, particularly as it relates to their water supply and ability to raise crops to feed their families?**

This is an important issue to me and I will strive to provide a policy-relevant answer. Thank you for addressing an issue that has considerable import to millions.

The questions above are introduced with a *Washington Post* news article describing the apparent plight of Peruvians who depend on annual snow/glacial melting for a portion of their water needs. These types of articles generally present dramatic assertions and tend to highlight whatever is alarmist and attention-grabbing. After all, the ability of the media to survive is dependent on how many people's attention may be grabbed. Assertions are not science. Science is numbers (as Lord Kelvin said.)

Tropical glaciers have been advancing and retreating for thousands of years, and are not exceptionally good indicators of temperature. (Note for example: Scientists Unravel Mystery of Growing Glaciers, 24 Aug 2006, *Guardian Unlimited*, describing the growth of glaciers in the western Himalaya, Karakoram and Hindu Kush mountains.) In Fig. 6.1 of the NRC Surface Temperature Reconstruction Report (2006 of which I was a co-author), ice cores of three glaciers are shown for South America and three for the Tibetan Plateau. Two of the 6 show an increase in the proxy temperature since 1000 A.D. while the other 4 show level or declining trends. In particular, the glacier identified in the *Post* article (Quelccaya) shows a long decline (cooling) to about 1800 with a rise to about 1950 and fairly level since then which doesn't match human-induced climate change theories well at all. Dr. Lonnie Thompson, who studies this glacier more than anyone, indicated to me that he believed this glacier was about 1,500 years old. Thus, it appears that these glaciers advance and retreat on many time scales and should not be depended upon for the long term. This is what the numbers suggest.

A society which depends on the annual melting of "glacial" ice is therefore dependent on an erratic system. The following letter to the editor addresses the problems of the Peruvian water situation, noting that ineffective water management rather than global warming is the problem.

> Peru Shows Why Water Privatization Is Needed
>
> *Washington Post*
> Sunday, August 6, 2006; B06
>
> Doug Struck reported on the water crisis in Lima, Peru, and on the role that accelerated glacier melting has played in recent years ["On the Roof of Peru, Omens in the Ice," front page, July 29]. But more than a billion people throughout the developing world lack access to clean water, and that is largely due to the dismal performance of the public sector, which is in charge of 97 percent of formal water distribution in poor countries. Water shortages are even common in Cherrapunji, India, which has been described as the wettest place on Earth.
>
> In Lima, a quarter of the city's 8 million people don't have piped water. The article quotes an engineer at Peru's public monopoly who suggests that if the utility did connect those 2 million people, there would not be enough water to serve them. The article does not mention that some 40 percent of the water piped through the public utility is lost to leakages and otherwise unaccounted for.
>
> Peru's public water utility has failed to serve a huge percentage of the population for decades. Privatization would increase access to water, reduce death and diseases, and introduce accountability and rational pricing, as countless cases of successful water privatization around the world have shown. The first to benefit would be Lima's poor, who currently pay exorbitant black-market prices for water.
>
> IAN VÁSQUEZ
>
> Director
> Project on Global Economic Liberty

Cato Institute
Washington

The main problem in poor agricultural societies like Peru is that the country's institutions and regulations encourage wasteful water usage in rural areas that particularly harm the poor. Agricultural productivity is mainly undermined by major factors (lack of property rights, closed economies, civil wars, state marketing boards, erratic macroeconomic policy, low growth, bad infrastructure, etc.) that have nothing whatsoever to do with global warming. In areas like Peru where glacier melting seems to have reduced water supply, it would be far cheaper to pay for a range of solutions (a system of dams and irrigation, relocation of some vulnerable citizens, etc.) than it would to implement alternatives that would reduce growth in both rich and poor countries and in the end have no impact on the problem. So the better approach is to encourage locally-focused solutions at a far smaller cost than top-down energy suppression measures which in reality will not impact the climate.

In summary, alarmist articles, such as was as attached with these questions, are not designed to give hard scientific information from which policy can be made. The real issues in this arena often boil down to how public water management entities have failed to store, allocate and distribute water effectively, efficiently and sustainably.

(1a) Answer

As indicated in my testimony, it is my view that people should be given greater access to energy produced by the most efficient and clean means possible because energy provides longer and better lives. At present, much of the poor’s energy is produced from biomass burning (wood, dung) which destroys habitat and fouls the air with toxic smoke. In that context, energy from fossil fuels can be an environmental and humanitarian step forward. Though expensive and intermittent, other sources, such as solar or wind, could help fill part of the gap. However, cost, reliability and base-load power requirements are three factors that must be considered and which tend to work against solar and wind.

I do not subscribe to the notion that climate change (about which we can do anything about) is causing these people serious problems today. Tropical glaciers are known to have advanced and retreated many times in the past. People who are dependent on a particular status quo of a dynamical system like mountain glaciers are operating in a belief-system that the actual climate cannot guarantee. The present retreat of several of the glaciers in this part of the tropics leads one to hope these people can adapt to such variability. (But note above the growth of glaciers in South Asia.) Their water still falls as rain and snow, and capturing that water for dry spells is a prudent plan to pursue. The issue of water policy goes far beyond Peru and the impacts of climate change (see below).

(1b) Answer

Let me first say that the future distribution and quantity of rainfall is unknown. Rainfall patterns have been notoriously variable over the centuries as evidenced by paleo-climate research during the period when no human-influence on climate was possible. Additionally, rainfall in general is more important than temperature for sustaining life.

Climate models are unable to confidently predict where the rain may increase, decrease or stay the same. Further, efforts to “control” climate change are misguided as we have no way to confidently determine how a particular policy for controlling greenhouse gases will impact precipitation.

Water policy is a vast and complex issue with climate variability being only one component. The political aspects of water availability are significant and the growth of water-dependent systems (human and agricultural) in desert areas is going to be a challenge to sustain whatever the climate does (see introductory comments to these answers). In the U.S. for example, we know that creating the availability and performance of an acre-foot of fresh water in California (where over 80% goes to irrigation) costs about 15 times that of creating the same acre-foot in Alabama. Where then should the country invest its funds for the most benefit, both financially and environmentally?

The policy-relevant issues for a political body are to determine (1) where and how much water there is, (2) who owns the water and therefore who controls its use, (3) what uses are sustainable environmentally, financially and politically, (4) what infrastructure may be built to use the available water efficiently, confidently and sustainably, and (5) what incentives are available to pay for (4).

I suspect water will become more and more commoditized in the future, so that some investment will come from the commercial sector to store and distribute water. How governments, especially poor governments, take advantage of such investments to provide clean water for human consumption (and a great leap forward in health care) will be done on a country-by-country basis, but I cannot predict how effective that process will be. U.S. policymakers could facilitate the reduction of water crises by helping governments answer these 5 questions.

(2) Your published work on satellite-derived lower tropospheric temperature data was used for several years as evidence that there is no global warming, since it appeared to show that the temperatures in the tropics were actually cooling. In 2005, Dr. Carl Mears and Dr. Frank Wentz of Remote Sensing Systems in Santa Rosa, California, published an article in Science magazine showing that, because of orbital drift and decay that was not controlled for in your study, the temperature measurements were gradually taken later and later in the day when temperatures were cooling. The article also found a mathematical error in your work. When corrected, the data pointed to an increase in tropical temperatures, not a decrease.

Is you original work still being used as evidence that there is no global warming? Have you corrected this work?

There are a number of issues intermingled in these comments and questions that need clarification. Beginning in June 1998 and for every month since then, the University of Alabama in Huntsville (UAH) global temperature measurements reported positive global trends for all versions of the lower tropospheric (LT) temperature dataset. The tropical trends were not different from a zero trend when Mears and Wentz began looking at the methodology of our version 5.1 (v5.1). As noted in my testimony they discovered an artifact of our adjustments for satellite drift which created a cooling error in the tropics for LT. (We produce other temperature products which used the same methodology to account for this drift but which were not affected by this artifact.)

A fair bit of confusion arose when Mears and Wentz published the discovery of this error in August 2005 and in the same publication introduced a new LT dataset of their own. The implication of this publication was that the error they found was the difference between our old dataset and their new dataset which was significant, about 0.10 °C/decade. In other words, the impression given in the article was that their new dataset represented a corrected version of our old dataset. Unfortunately, this was not the case. The actual impact of the error in UAH's v5.1 was not addressed in their paper. As Roy Spencer and I published in *Science* magazine later in 2005, the effect of that error was small, +0.035 °C/decade (at that time from +0.090 to +0.125 °C/decade), being within our originally published error margin assigned to v5.1. In the tropics, the effect was to increase the trend from +0.00 to +0.05 °C/decade.

We corrected the error in May 2005 and with the publication of Mears and Wentz put the data on a public website in August 2005, though it was provided to several scientists before that date. This new version, v5.2, has been publicly available since that time. So there are two LT datasets with somewhat differing trends, UAH's and RSS's.

Of interest to the committee is the fact I will have two papers to be published shortly which indicate UAH v5.2 is highly consistent with independent temperature measurements of the LT layer. These papers show that it is very likely that the tropical atmosphere is warming at a rate equal to or less than that of the surface, a characteristic no climate model that we have examined replicates. Thus, there is evidence that the theoretical ideas of how the large-scale atmosphere should be responding to the enhanced greenhouse effect, as embodied in climate models, still have shortcomings.

As to the first question, we provide only the latest version of our data to the public. And, since 1998 any version of our lower tropospheric dataset would have shown a positive global trend. Thus, if someone is using UAH data to claim no global warming, I would speculate they are likely using pre-1998 data or are somehow altering the data to make that conclusion. I don't know of any current claims to that effect, and UAH has been forthright in reporting positive trends (and the likelihood that at least part of that positive trend is due to enhanced greenhouse gases) these past 8 years.

In answering the second question, the discussion above describes the events that led to the correction of the drift error and UAH's corrected data have been publicly available since August 2005. However, one should be aware that datasets are always subject to revision, and we look forward to v6.0 of our current dataset, though there will be little change in the outcome relative to v5.2.

RESPONSE FOR THE RECORD OF DR. RALPH J. CICERONE, PRESIDENT, NATIONAL ACADEMY OF SCIENCES

Responses to questions following the July 27, 2006 hearing before the
Subcommittee on Oversight and Investigations
Committee on Energy and Commerce
U.S. House of Representatives

Ralph J. Cicerone, Ph.D.
President, National Academy of Sciences

The Honorable Bart Stupak

1. We have heard the view expressed that the global warming that we are experiencing today is part of a sinusoidal wave that reflects the earth's normal warming and cooling patterns. Do you agree with that view? If not, please explain why.

No, I do not agree. Let me explain.

In geological history, there are some examples of quasi-periodic variations in the Earth's climate that must have been natural. Changes in the Earth's orbit around the Sun, known as Milankovitch cycles, are associated with climate variations over long timescales, such as the ice ages that have occurred over the last few million years. Also, the internal dynamics of the tropical ocean and atmosphere give rise to the El Niño phenomenon, a warming of the central and eastern tropical Pacific Ocean that is associated with climate impacts across the globe every 2 to 5 years. However, while the El Niños affect large regions, they are not global.

In contrast, no known natural phenomenon can explain the observed warming of the past 30 years, which has been continuous, rapid and widespread. Furthermore, the magnitude and pattern of this recent warming matches what we expect based on the observed increase in greenhouse gas concentrations and other human activities.

2. Please describe the peer review process that was used for the National Academy of Sciences (NAS) report on global temperature reconstruction.

Every report prepared by a National Research Council committee is reviewed by a group other than its authors. This independent review distinguishes the NRC from many other organizations offering scientific and technical advice on important national issues. The purpose of review is to assist the authoring committee in making its report as accurate and effective as possible.

The review of the report *Surface Temperature Reconstructions for the Last 2000 Years* followed the National Academies' normal, rigorous peer-review process. The review was overseen by the Report Review Committee (RRC), made up of approximately 30 members of the National Academy of Sciences, National Academy of Engineering, and Institute of Medicine. The Board on Atmospheric Sciences and Climate (BASC), in consultation with the RRC, appointed a group of 13 independent reviewers with a broad range of expertise and diverse perspectives on the issues addressed in the report. The identity of reviewers is considered to be privileged information during the review process. Anonymous review is intended to encourage individual reviewers to express their views freely and to permit the authoring committee to evaluate each comment on its merits without regard for the position or status of the reviewer. The names and affiliations of the reviewers were made public when the report was released, but their comments remain confidential.

The draft report was sent to the 13 reviewers only after all authors indicated that they were satisfied with its form and content. Reviewers received the complete report (including front matter, preface, executive summary, and appendices) along with the committee's statement of task and the NRC review guidelines. Reviewers were asked to provide written comments on any and all aspects of the draft report, including the accuracy of the committee's analysis and the responsiveness of the committee to its charge. The authors were required to respond in writing to every review comment, revising the report where appropriate. These responses were evaluated by a review monitor appointed by the RRC and by a review coordinator appointed by the NRC Division on Earth and Life Studies. The report was not released until after the review monitor, the review coordinator, and all members of the authoring committee approved the revised draft. Once the review process was successfully completed, no changes (other than minor editorial corrections) were made to the approved text.

3. Please describe the key elements of academic peer review, such as who picks the reviewers, whether comments can be submitted anonymously, and who decides whether the paper can be published if there are negative comments that the authors do not accept.

Each scientific journal has its own policies for peer review, but there are some standard features among them. Generally, submitted manuscripts are assigned to an editor who has expertise on the general topic. These editors conduct a first level review of the manuscript to determine if it meets basic criteria for publication in the journal (e.g., length, appropriateness of subject matter, etc.). Some manuscripts are rejected outright at this stage.

If a manuscript is considered suitable for the journal, it will be sent out for peer review. Usually, two or more reviewers are chosen by the editor. In some cases, the journal may ask the author(s) to suggest potential reviewers who have the appropriate expertise, but the editor or an associate editor will make the ultimate decision. Reviewers are typically asked to submit comments on the manuscript and to make a recommendation on its suitability for publication in the journal. Once the reviewers' comments have been submitted, the editor decides whether to accept the manuscript for publication (perhaps with some minor revisions), ask the author(s) to make significant revisions and submit the paper for re-review (in which case the process is repeated), or reject the paper. In the case that two reviewers disagree about the suitability of a manuscript for publications, the editor may solicit an additional tie-breaking review or ask the author(s) to prepare a rebuttal to the critical reviews. Usually all of the review comments are shared with the author(s), regardless of the decision to publish, but the identity of the reviewers is kept anonymous throughout the process, even after publication, unless a reviewer specifically requests for their anonymity to be waived.

In prestigious journals such as *Science* and *Nature*, the vast majority of manuscripts (up to 95%) are rejected at some point in this process. Acceptance rates in other journals vary considerably but are usually higher. For example, acceptance rates at the various journals published by the American Geophysical Union, including the Journal of Geophysical Research, range from about 50-80%.

4. During the hearing, you referenced information on CO_2 concentrations in Antarctica going back 650,000 years. Is this information quantitative or qualitative?

High quality, quantitative measurements of CO_2 concentrations are available from ice cores in Antarctica extending back to about 650,000 years before present (Siegenthaler, U. et al., 2005. Stable Carbon Cycle-Climate Relationship During the Late Pleistocene. *Science, 310*, p. 1313-1317). These measurements are obtained by analyzing the air extracted from small bubbles trapped in the ice, which reflect atmospheric composition at the time when the snow that formed each layer was compacted into ice. Because CO_2 is well-mixed in the atmosphere, the CO_2 concentration at Antarctica is representative of the globally-averaged atmospheric concentration. The measurements by Siegenthaler et al. show that CO_2 did not exceed 300 ppmv (parts per million by volume) during the 650,000 years that preceded the industrial era. Present concentrations of CO_2 in the atmosphere are about 380 ppmv, and current projections indicate that the level will reach 450 ppmv by mid-century.

When evaluating CO_2 data from ice cores, there are two types of uncertainties to consider: (1) uncertainty in the concentration of CO_2 for any particular measurement, which tends to be small, perhaps a range of a few ppmv; and (2) uncertainty about dating the measurements, which can be as large as 1300 years for the earliest part of the record. This second source of uncertainty complicates efforts to synchronize measurements of CO_2 (and other gases, such as methane) with ice core measurements of temperature, which are calculated based on the properties of the ice rather than the trapped air bubbles. Thus, an open research question is whether the rises and falls in greenhouse gas concentrations lead or lag the rises and falls of temperature associated with glacial-interglacial cycles (e.g., Mudelsee, M. 2001. The phase relations among atmospheric CO_2 content, temperature, and global ice volume over the past 420 ka. *Quaternary Science Reviews, 20.* 583-589). This question is especially pertinent for the temperature increases after ice ages, which often occurred over only a few thousand years. Most analyses indicate that temperature increases have preceded increases in CO_2 when the Earth warms after an ice age (e.g., Hansen and Sato, 2004. Greenhouse gas growth rates. *Proceedings of the National Academy of Sciences, 101(46)*, p. 16109-16114.). However, this should not be interpreted to suggest that the current increase in CO_2 concentrations will not produce further global warming, only that other processes (such as orbital forcing or changes in ocean circulation) may have initiated the rebound in global-mean temperature after ice ages.

5. Did the NAS Surface Temperature Reconstruction Committee find any temperature reconstructions that show global or Northern Hemisphere temperatures in the medieval period higher than recorded temperatures today?

The paucity of proxy data for periods prior to about 1600 A.D., especially in the tropics and the Southern Hemisphere, limited the committee's confidence in statements regarding the global mean temperature of the past few decades compared to medieval times. Several proxies indicate that the area around Greenland was warmer between about 1000 and 1200 A.D. than it is today. There is also evidence for warm temperatures during medieval times from other regions of the world. However, several studies suggest that these warm anomalies appear to have occurred at different times and at different places rather than being hemispherically or globally synchronous, and also appear to have been offset by cold anomalies in other regions. Although it is difficult to quantify the full uncertainty associated with estimates of surface temperature prior to about 1600 A.D., all of the large-scale surface temperature reconstructions that the committee examined support the assertion that global-mean temperatures during the last few decades of the 20th century were unprecedented over at least the past 1,000 years, and a larger fraction of geographically diverse proxy records experienced exceptional warmth during the late 20th century than during any other extended period from 900 A.D. onward. Hence the committee found it plausible (or in other words, no evidence exists to refute the claim) that "the last few decades of the 20th century were warmer than any comparable period over the last millennium." This statement can be more strongly applied to the Northern Hemisphere than to the globe because there is very little proxy data from the Southern Hemisphere before about 1600 A.D.

RESPONSE FOR THE RECORD OF MR. STEPHEN MCINTYRE, TORONTO, ONTARIO, CANADA

Response of Stephen McIntyre to Questions in Connection with Testimony to the Subcommittee on Oversight and Investigation, House Energy and Commerce Committee

QUESTIONS FROM HON. BART STUPAK

Figure 4.3 in Dr Wegman's report is represented to be a re-computation of Figure 3 from MM05b. Please answer the following questions based on your computation of Figure 3 in MM05b.

1) ***Is the way the data is centered the only difference between the two panels in Figure 4.3?***

The purpose of Figure 3 of McIntyre and McKitrick [GRL -2005] was to illustrate the difference between "conventional" principal components methodology, as Mann et al 1998 (MBH) had said that they used, and the methodology actually used in MBH.

There were two other differences in addition to centering.

There are two standard options in principal components algorithms - use of a covariance matrix or use of a correlation matrix. The ITRDB North American tree ring network ("NOAMER"), which is illustrated in Figure 3, consists of tree ring chronologies already standardized to common dimensionless units, a point noted in Huybers [2005] as follows:

> NOAMER records are standardized chronologies [Cook and Kairiukstis, 1990], reported as fractional changes from mean tree ring width or maximum ring density after correcting for the effects of increasing tree age

For networks denominated in common units, most statistical texts [Rencher 1992, 2002; Overland and Preisendorfer, 1982; North et al 1982 - see discussion in McIntyre and McKitrick 2005c] recommend use of the covariance matrix. However, instead of using the covariance matrix, MBH had divided the tree ring chronologies by the detrended standard deviation in the calibration period.

This procedure is related to, but not identical to using a correlation matrix. First, the division is not by the standard deviation over the entire period (which is equivalent to using a correlation matrix), but only in the calibration period; and the detrending within the calibration period has a further impact. Although these steps were not described in MBH98, we reported these steps in McIntyre and McKitrick [GRL -2005] as follows (and applied these in the calculation of the top panel):

> Each tree ring series was transformed by subtracting the 1902–1980 mean, then dividing by the 1902–1980 standard deviation and dividing again by the standard deviation of the residuals from fitting a linear trend in the 1902–1980 period. The PCs were then computed using singular value decomposition on the transformed data.

2) Were the same R commands used to carry out the PC analyses for the upper and lower panels?

The top panel was calculated using the R function svd after de-centering the data and dividing by its calibration period detrended standard deviation, as described above. The bottom panel was calculated using the R function princomp with the default option of the covariance matrix. The code was archived at the time at the following location ftp://ftp.agu.org/apend/gl/2004GL021750,

Both panels could have been calculated using only one R function prcomp, (which has a different option than princomp) by using the center=FALSE option in the top panel on data de-centered as discussed above and by using the center=TRUE for the bottom panel.

3) Was the upper panel processed based on a correlation matrix? Was the lower frame processed on a covariance matrix? If the answer to both questions is yes, does this not have the effect of comparing standardized data with non-standardized data?

The upper panel replicated the original calculation by Mann et al, which did **not** use a correlation matrix, either in the original code or in our replication of it. It carried out singular value decomposition (svd) on the de-centered data, rather than on either a correlation or covariance matrix. The bottom panel was calculated on a covariance matrix.

Tree ring chronologies in the ITRDB network are already *standardized* to dimensionless units. So all of the various calculations are comparisons of *standardized* data.

The issue of whether to divide chronologies by their standard deviation (i.e. using a correlation matrix rather than a covariance matrix) is a separate question, which should have been addressed by the original authors. As noted above, the statistical procedure recommended in standard references is not to divide series denominated in common units by their standard deviation. The issue was considered in passing by the NAS panel, who stated:

> In this case, an argument can be made for using the variables without further normalization. (p 101)

The purpose of Figure 3 was to compare the most plausible interpretation of the stated methodology with the actual methodology - which had not been accurately reported in the original article. All the data had been standardized.

QUESTIONS FROM HON. ED WHITFIELD

1) ***In his testimony, Dr Mann cites articles by Huybers and by von Storch and Zorita, which specifically address your published work. For the record, did you have an opportunity to respond in the literature to these studies? If so, what was your response? (Please provide a brief description, including abstracts if available.)***

We responded to both Huybers and Von Storch and Zorita in the peer-reviewed literature [McIntyre and McKitrick 2005c, 2005d]. (The form of Reply did not include an Abstract.)

Huybers argued that using a correlation matrix to calculate the PC1 in Figure 3 led to less difference with the MBH PC1 than using a covariance matrix. Huybers argued that correlation PCs were a more appropriate calculation than either covariance PCs or the MBH method. In response, we showed 8 different possible calculations. In one such example, we observed that the standardization using autocorrelation-consistent standard deviations yielded a PC1 which happened to be similar to the covariance PC1. We observed that the correlation PC1 obtained from a network excluding the controversial bristlecone pines was similar to the covariance PC1 (i.e. not hockey-stick shaped). We observed that the differences between these various permutations and combinations depended on the weight assigned to bristlecones. We stated once again that we ourselves did not propose covariance PCs as a method of "fixing" the bias in Mann's methodology and stated that the obligation to establish the validity of any of these methods lay with the authors proposing the use of the methodology for climate studies. (Also see the related discussion above in the reply to Hon. Bart Stupak.)

Huybers also argued that our simulations showing a higher benchmark for the RE statistic in MBH circumstances did not properly reflect a re-scaling step within MBH methodology. In response, we performed new simulations in which we took 1000 simulated PC1s saved from the simulations described by MM05; for each PC1 in turn, we made a ''proxy network'' of 22 series with the other 21 being white noise (replicating the 22 series of the MBH98 AD1400 network). We then used MBH98 methodology on the proxy network, including inverse regression of the proxies. After calculating the reconstructed temperature principal component (RPC), we scaled the variance of the RPC to the ''observed'' variance of the temperature principal component prior to calculating a NH average, from which we calculated an RE statistic. The 99% quantile was 0.54, down slightly from 0.59 as found by MM05.

Von Storch and Zorita argued that, in a network of pseudoproxies (which were designed to all contain a signal plus white noise), the biased Mann PC methodology did not have a material deleterious impact. In our Reply, we agreed that the impact of the flawed Mann methodology was much less in a "tame" network of the type assumed in the pseudoproxy network as designed by Von Storch and Zorita.

However, we argued that these pseudoproxies did not replicate essential features of the North American tree ring network, most notably, the potential contamination of bristlecone pine chronologies by non-climatic effects, and, as a matter of fact, the PC methodologies led to quite different results for the actual North American network.

2) ***You state in your testimony that "much relevant data" to the Mann et al work did not become available until the 2004 Corrigendum in Nature, and then, only after a formal complaint to Nature. Dr Mann testified that "our entire data set was available on the World Wide Web several years before" 2004. Were these the data that you were referring to in your testimony?***

Nature required Mann et al to issue a new Supplementary Information (SI) as part of the 2004 Corrigendum. The new Supplementary Information included, among other things, the following information not available in the original Supplementary Information: the temperature data set used in MBH98 from which gridcell standard deviations and temperature principal components were calculated; the number of principal component series retained for each network in each calculation step; and the proxy data used in the calculations. The Corrigendum itself identified 35 sites that were not used in the calculations, although they were listed in the original Supplementary Information as being used.

Some essential information remains unavailable to this date - most notably, the reconstructions for the individual steps, particularly the AD1400 step in controversy.

There is some question about when proxy data at Mann's University of Virginia website became publicly available. The first public reference to the directory containing this data was in November 2003 in Mann's response to McIntyre and McKitrick 2003. Mann has made claims that the directory became public in July 2002.

However, in April 2003, during my inspection of Mann's FTP site, I had been unable to locate a directory for MBH98 proxy data. Accordingly, I emailed Mann and asked him for the location of the proxy data used in MBH98, Mann replied that he had forgotten where the data was, but undertook that his associate Scott Rutherford would locate it. Rutherford said that the data did not exist in any one place, but that he would get it together for us. A few weeks later, he provided a url at Mann's FTP site from which I downloaded data.

Following publication of McIntyre and McKitrick 2003, Mann identified a different url at his FTP site, containing a data archive which differed in material respects from the data archive previously identified by Mann's associate, although many individual series were identical.

It is my belief that the directory that, in late October 2003 or early November 2003, this directory was moved from its previous location in a private directory that was not searchable by third parties without access to the complete URL and file name to a publicly searchable directory.

3) ***Did you write the complaint to Nature? If so, what was your specific complaint and would you please supply a copy for the record, as well as any relevant response from Nature?***

We submitted a Materials Complaint in late 2003 (see Appendix A), listing 10 items, including inaccurate disclosure of principal components methodology, incorrect listing of tree ring sites used in principal components calculations and requesting results of individual calculation steps.

In February 2004, Nature sent us a response from Mann et al to the Materials Complaint (see Appendix B) and notified us that they would require Mann et al to issue a Corrigendum together with a new Supplementary Information. In March 2004, they sent us a copy of the proposed Corrigendum, attached as Appendix C. Following receipt of the draft Corrigendum, I sent Nature a list of detailed comments on deficiencies in the Corrigendum (see Appendix D). They replied that most of these comments would not be addressed since "for reasons of space constraints, we insist that such publications are as concise as possible" (see Appendix E). In July 2004, the Corrigendum (Appendix F) was published with one material change from the draft Corrigendum (Mann et al asserted that "None of these errors affect our previously published results", a claim not made in the draft Corrigendum.)

After inspection of the Corrigendum SI in July 2004, we determined that it failed to resolve many of the issues raised in our original Materials Complaint and, on August 10, 2004, we submitted a fresh complaint to Nature (Appendix G), including a request for the still unavailable results of individual steps, including the 15th century step in controversy. A month later, Nature replied, refusing to intervene and saying that disclosure of results of individual steps was up to Mann. We still have not obtained results of the individual steps.

4) ***You note in your testimony that the IPCC does not carry out any due diligence on articles. Have you had an opportunity to conduct due diligence on articles as an outside reviewer of the IPCC draft assessments? If so, what has been your experience with conducting such analysis of the articles referenced in IPCC documents?***

I have personally acted as an expert reviewer only for the IPCC Fourth Assessment Reports (IPCC 4AR) and was not involved in the Third Assessment Report. As an expert reviewer for IPCC 4AR, I attempted to conduct due diligence on two then-unpublished articles cited in IPCC 4 AR: D'Arrigo et al 2006 and Hegerl et al 2006.

In my capacity as a reviewer, I asked the IPCC Technical Services Unit (TSU) to provide me with supporting data for the two then unpublished articles, including such elementary information such as the identity of the sites used by Hegerl et al 2006. The TSU refused to provide me with this data saying that they would not perform "secretarial" services me and referred me to the original authors. I then requested the information from the original authors, but could obviously not do so on an anonymous basis. The authors refused to provide the information. Shortly thereafter, Susan Solomon, Chairman, IPCC Working Group 1, wrote to me as follows:

> it is inappropriate to cite a function as a reviewer in the IPCC process as entitling you to access to additional information from authors of the unpublished papers available at our web site, which you have also done.

Solomon asserted that such inquiries were a breach of the confidentiality agreement under which IPCC reviewers were permitted access to unpublished papers and stated:

> The IPCC rules for reviewing draft reports have served the scientific and policy communities well for numerous past international assessment rounds. If there is further evidence that you can not accept them, or if your intent is to use your access to the review process to challenge them, then we will not be able to continue to treat you as an expert reviewer for the IPCC

In his answers to Senator Inhofe in 2003, Mann also commented on IPCC due diligence procedures (or lack of procedures) in the following terms:

> *30. Did IPCC carry out any independent programs to verify the calculations that you made in MBH98 or MBH99? If so, please provide copies of the reports resulting from such studies.*
>
> *Answer: It is distinctly against the mission of the IPCC to "carry out independent programs", so the premise of the question is false. However, the IPCC's author team did engage in a lively interchanges about the quality and overall consistency of all of the papers as the chapter was drafted and revised in the course of review.*

QUESTIONS FROM HON. MARSHA BLACKBURN

1) ***What is your opinion on surface records and satellite data as temperature proxies?***

I have no opinions on the quality of surface records and satellite data. I strongly object to the failure of the authors of the CRU temperature data to comprehensively archive their data and methods, especially since they have been funded for many years by the Department of Energy.

2) ***Please explain to this committee the difficulty you have had in verification and replication of studies, other than Mann, the NAS report used to examine the Medieval Warm Period.***

The studies illustrated in Figure S-1 of the NAS report showing the Medieval Warm period are: Esper et al 2002; Mann and Jones 2003; Moberg et al 2005. Hegerl et al 2006 is only shown back to 1251. (Two other studies, Huang et al 2000 and Oerlemans et al do not provide results prior to 1500.)

Esper et al 2002. The original article did not archive site chronologies or provide accurate site identifications. I first requested this information from Esper in 2003, but failed to receive any response. In 2005, I asked *Science* to intervene and obtain the unarchived site chronology and site measurement information. Over the next 9 months, I received partial and piecemeal responses. Eventually, after over 20 email inquiries to *Science,* I received site chronologies and measurement data for 13 of 14 sites; the Esper chronology for the Mongolia site is still outstanding. I have been unable to replicate important aspects of Esper's results and requested either source code or methodological clarifications sufficient to permit replication. Esper has refused to provide this information and *Science* has not required him to do so.

Moberg et al 2005. Moberg provided adequate digital references for 16 of 18 series. However he failed to provide digital versions or digital references for the other 2 series, both necessary for replication and refused to provide them when requested. In late 2005, I filed a Material Complaint with Nature. Nature required Moberg et al to provide the two missing series (which they did) and a Corrigendum was published by Moberg et al in February 2006. I have asked Moberg to explain a discrepancy between the pubished version of one series and the version illustrated at Nature, but this presently remains unresolved.

Hegerl et al 2006: In fall 2005, as noted above, in my capacity as an IPCC 4AR reviewer, I requested identification of the sites used in this study. Hegerl refused to provide this information. The study was released in preprint version only in September 2006 and my consideration of this study is still preliminary. The study was released without a supplementary data archive.

Two other studies considered by the NAS panel, but not illustrated in Figure S-1 are Osborn and Briffa 2006 and Thompson's ice core data.

Osborn and Briffa 2006: This has an archive of site chronologies as used, However, supporting measurement data was not archived for several key series used in this (and many other) studies: Yamal, Taimyr and the Tornetrask update. I asked Osborn and Briffa for this information and they refused. I then asked *Science* to obtain this data for me. They refused to require Briffa and Osborn to archive the supporting measurement data.

Thompson: The NAS panel illustrated and relied on ice core data collected by Lonnie Thompson and associates. As of 2004, Thompson had not archived any information in respect to the key Himalayan ice cores, some of which was obtained as long ago as 1987 and had not responded to any inquiries. In 2004, I complained to *Climatic Change*, which had recently published an article by Thompson. As a result, Thompson provided a digital listing of the data graphed, but failed to provide a complete archive of isotope and chemical information collected from the ice core. IN 2005, I complained to Science, which had published many articles by Thompson. I received a listing of δO18 values for Kilimanjaro, but no other data. I have re-iterated this request to *Science* on numerous occasions without any success.

Because the NAS Panel had relied to a considerable extent on unarchived data, in August 2006, I sent a letter to Ralph Cicerone, President of the National Academy of Sciences, asking him to request the various authors to archive data used in the NAS Panel report. The letter is attached as Appendix I. In September 2006, Cicerone refused to involve himself (see Appendix J).

Following Cicerone's refusal, I then wrote to Gerald North, Chairman of the NAS Panel, sending him the same request as I had sent to Cicerone and asking him to request the information from the various authors. North agreed to send the requests. However, as of Oct. 31, 2006, North had not reverted to report any success in obtaining data from any of the authors and I have received no new information from the various authors.

3) What is your opinion on these other studies in the NAS Report? Do they follow Mann's methodologies?

In my opinion, the other studies are also flawed to a degree that one cannot rely on them to establish a temperature reconstruction for the last millennium with any confidence.

Mann's methodologies are applied to a considerable extent in these other studies.

One of Mann's most important methodologies was his introduction of bristlecone and foxtail pine growth chronologies into proxy reconstructions. As early as the 1980s, these trees were known to have experienced an anomalous pulse in growth in the 20th century, which specialists had concluded was unrelated to temperature, and speculated that it was due to CO2 fertilization. The IPCC Second Assessment Report warned of this potential problem in tree ring chronologies. The NAS Panel recently re-stated that strip-bark forms should be avoided in temperature reconstructions - a finding that disqualifies many, if not most, bristlecone and foxtail sites, as strip-bark samples were specifically sought out in many sampling programs and no sampling programs for bristlecones and foxtails that I am aware show that they have avoided strip-bark samples.

Although multiproxy reconstructions prior to MBH98 (e.g. Bradley and Jones, 1993) had avoided the use of problematic bristlecone chronologies, after MBH98, nearly all studies incorporated Mann's methodology of using bristlecones and foxtails, including the studies illustrated by the NAS Panel: Mann and Jones 2003; Hegerl et al 2006; Moberg et al 2005; Esper et al 2002; Osborn and Briffa 2006, as well as many other studies including Crowley and Lowery 2000 and Rutherford et al 2005.

In addition to Mann's reliance on potentially flawed bristlecone and foxtail chronologies, the NAS Panel and the Wegman Panel criticized Mann's principal components methodology, which tended to enhance the weighting of bristlecones in his North American tree ring network. Mann's North American PC1, in addition to its use in MBH98-99, is used in Mann and Jones 2003, Rutherford et al 2005 and in the recent studies, Osborn and Briffa 2006 and Hegerl et al 2006.

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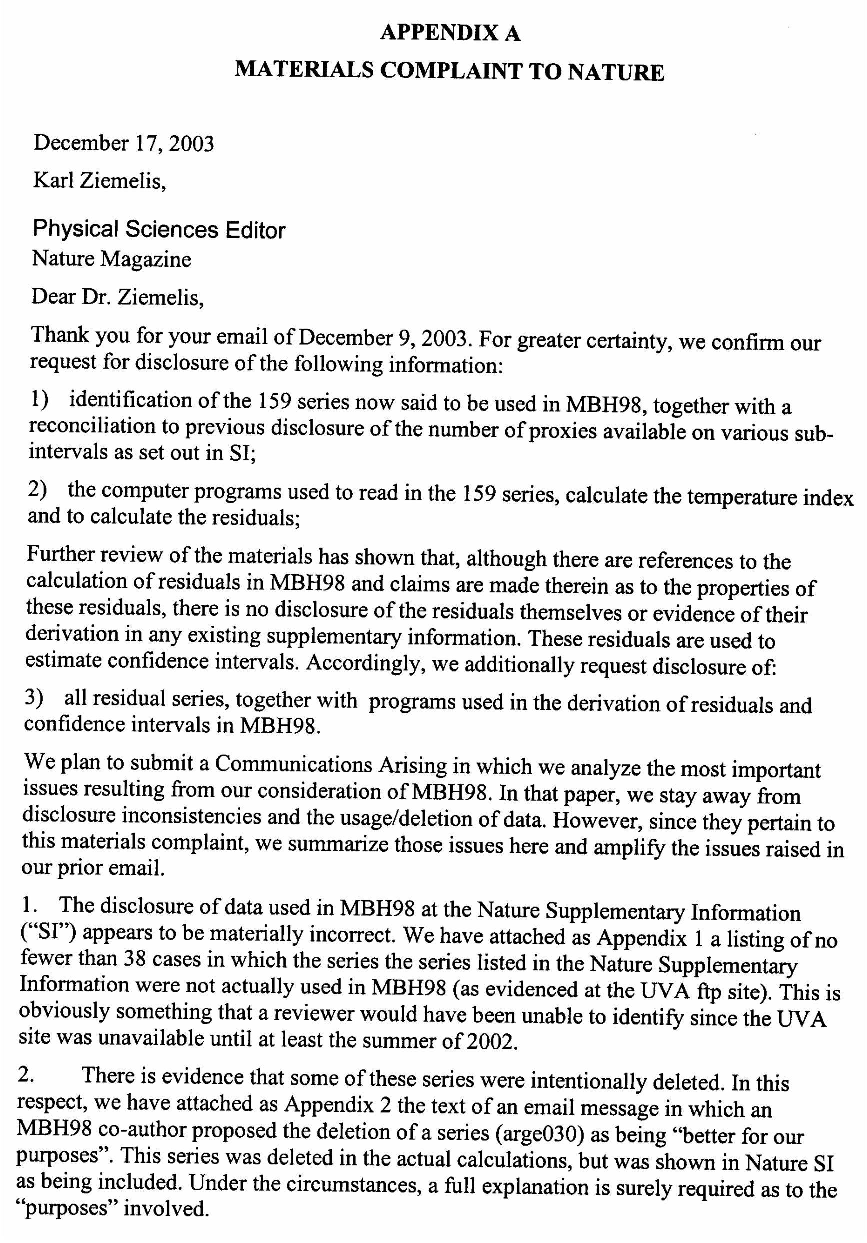


APPENDIX A

MATERIALS COMPLAINT TO NATURE

December 17, 2003

Karl Ziemelis,

Physical Sciences Editor
Nature Magazine

Dear Dr. Ziemelis,

Thank you for your email of December 9, 2003. For greater certainty, we confirm our request for disclosure of the following information:

1) identification of the 159 series now said to be used in MBH98, together with a reconciliation to previous disclosure of the number of proxies available on various sub-intervals as set out in SI;

2) the computer programs used to read in the 159 series, calculate the temperature index and to calculate the residuals;

Further review of the materials has shown that, although there are references to the calculation of residuals in MBH98 and claims are made therein as to the properties of these residuals, there is no disclosure of the residuals themselves or evidence of their derivation in any existing supplementary information. These residuals are used to estimate confidence intervals. Accordingly, we additionally request disclosure of:

3) all residual series, together with programs used in the derivation of residuals and confidence intervals in MBH98.

We plan to submit a Communications Arising in which we analyze the most important issues resulting from our consideration of MBH98. In that paper, we stay away from disclosure inconsistencies and the usage/deletion of data. However, since they pertain to this materials complaint, we summarize those issues here and amplify the issues raised in our prior email.

1. The disclosure of data used in MBH98 at the Nature Supplementary Information ("SI") appears to be materially incorrect. We have attached as Appendix 1 a listing of no fewer than 38 cases in which the series the series listed in the Nature Supplementary Information were not actually used in MBH98 (as evidenced at the UVA ftp site). This is obviously something that a reviewer would have been unable to identify since the UVA site was unavailable until at least the summer of 2002.

2. There is evidence that some of these series were intentionally deleted. In this respect, we have attached as Appendix 2 the text of an email message in which an MBH98 co-author proposed the deletion of a series (arge030) as being "better for our purposes". This series was deleted in the actual calculations, but was shown in Nature SI as being included. Under the circumstances, a full explanation is surely required as to the "purposes" involved.

3. MBH98 states that "the dendroclimatic data used were carefully screened for conservative standardization and sizeable segment lengths." We have attached as Appendix 3 the text of an email in which the term "wild guess" was used to describe the inclusion or exclusion of certain series. In this case, the deletion of various series is recommended, but this recommendation is not implemented in the work shown at the FTP site.

4. MBH have deleted portions of at least 3 series without explanation to the reader. In each case, the FTP site contains a parallel series with the correct data. The deletions include the first 70 years of the Central England historical temperature series, the first 25 years of the Central Europe historical series and the first 2 years of chin04. In the first 2 series, the issue is not merely that early decades were removed but that summer (JJA) data was substituted for annual data without notice to the reader.

5. MBH have recently stated that the UVA FTP site is the repository of data actually used in MBH98. This site contains at least 10 series not used in MBH98 (see Appendix 4), including an instrumental temperature series with decreasing 20th century temperatures.

6. Some series in the North American PC region occur in duplicate versions, as summarized in Appendix 5. There is no rational description of the reasons for inclusion or exclusion as individual proxies.

7. The SWM region used in MBH98 includes a site (Spruce Canyon CO), which is not in the site roster of the original article and which is in the roster of the NOAMER region. This apparently small point has a significant impact on 15th century values and we refer to this in our Communication Arising.

8. Some data versions used in MBH98 were obsolete when the paper was published. Many more versions are now obsolete. The SI refers to WDCP as a reference for tree ring data, but this reference is inaccurate given the material differences between the version used in MBH98 and the version actually archived. Again, the differences can be material and we refer to one such example in our Communication Arising. There are other inaccuracies in the references of MBH98: for example, the citation Bradley-Jones 1992 does not contain several instrumental sites referred to in MBH98.

9. The disclosure of methodology for calculating temperature principal components is inaccurate. MBH98 describe their methodology as "conventional". Conventional principal components fail with missing data. The underlying data set contains much missing data and some other procedure was necessarily used.

10. The disclosure of methodology for calculating tree ring principal components is inaccurate. Again MBH98 methodology is not "conventional". In this case, the FTP site contains computer programs which show that the data was transformed in ways not disclosed in MBH98. These undisclosed transformations have a material impact on the final results. We discuss this in our Communications Arising.

The last examples are of particular significance. As noted above, we have requested complete disclosure of the computer programs used in MBH98. Items (9 and 10) above shows that the disclosure of methods in MBH98 was either incomplete or misleading with respect to the principal components calculations. Given the influence of MBH98, meticulous verification should be possible and this cannot be accomplished with the requested disclosure.

We believe that the other issues identified herein merit careful consideration as well.

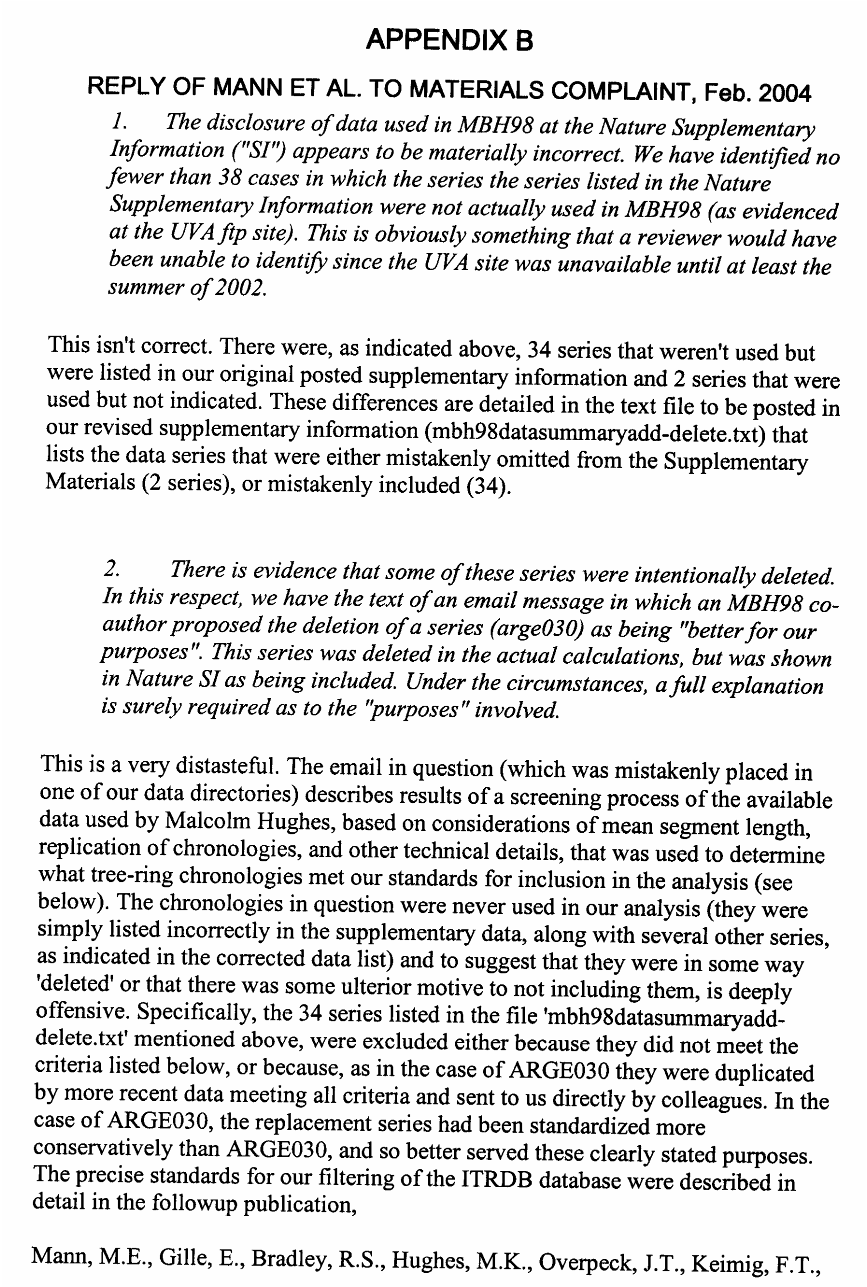
Yours truly,

Stephen McIntyre

Ross McKitrick

Table 1
Series Listed at Nat SI, but not used at UVA

Region	Number	Identifications
AUSTRAL	1	newz056
NOAMER:	22	ak006, ak006x, ca070, wa019, wa025, wa027, wa033, wa039, wa041, wa071, wa074, wa086, wa088, wa091, cana053, cana053x, cana096, cana096x, cana099, cana106, cana110 and cana111x
SOAMER:	7	arge030, arge060, arge065, chil015, chil016, chil017 and chil018
STAHLE/SWM ("Southwest-Mexico"):	2	10 sites listed at NatSI; 11 at UVA (2 series per site)
VAGANOV:	1	#55
MISC:	5	fran003, ital015, ital015x, spai026 and spai047
TOTAL	38	



APPENDIX B

REPLY OF MANN ET AL. TO MATERIALS COMPLAINT, Feb. 2004

> *1. The disclosure of data used in MBH98 at the Nature Supplementary Information ("SI") appears to be materially incorrect. We have identified no fewer than 38 cases in which the series the series listed in the Nature Supplementary Information were not actually used in MBH98 (as evidenced at the UVA ftp site). This is obviously something that a reviewer would have been unable to identify since the UVA site was unavailable until at least the summer of 2002.*

This isn't correct. There were, as indicated above, 34 series that weren't used but were listed in our original posted supplementary information and 2 series that were used but not indicated. These differences are detailed in the text file to be posted in our revised supplementary information (mbh98datasummaryadd-delete.txt) that lists the data series that were either mistakenly omitted from the Supplementary Materials (2 series), or mistakenly included (34).

> *2. There is evidence that some of these series were intentionally deleted. In this respect, we have the text of an email message in which an MBH98 co-author proposed the deletion of a series (arge030) as being "better for our purposes". This series was deleted in the actual calculations, but was shown in Nature SI as being included. Under the circumstances, a full explanation is surely required as to the "purposes" involved.*

This is a very distasteful. The email in question (which was mistakenly placed in one of our data directories) describes results of a screening process of the available data used by Malcolm Hughes, based on considerations of mean segment length, replication of chronologies, and other technical details, that was used to determine what tree-ring chronologies met our standards for inclusion in the analysis (see below). The chronologies in question were never used in our analysis (they were simply listed incorrectly in the supplementary data, along with several other series, as indicated in the corrected data list) and to suggest that they were in some way 'deleted' or that there was some ulterior motive to not including them, is deeply offensive. Specifically, the 34 series listed in the file 'mbh98datasummaryadd-delete.txt' mentioned above, were excluded either because they did not meet the criteria listed below, or because, as in the case of ARGE030 they were duplicated by more recent data meeting all criteria and sent to us directly by colleagues. In the case of ARGE030, the replacement series had been standardized more conservatively than ARGE030, and so better served these clearly stated purposes. The precise standards for our filtering of the ITRDB database were described in detail in the followup publication,

Mann, M.E., Gille, E., Bradley, R.S., Hughes, M.K., Overpeck, J.T., Keimig, F.T., Gross, W. , Global Temperature Patterns in Past Centuries: An interactive presentation, Earth Interactions, 4-4, 1-29,2000.

including the specific following discussion:

> As noted by MBH98 (and references therein) non-climatic influences related to intrinsic tree growth trends, difficulty in their removal, and the combining of different tree segments, make the lowest frequency (ie, century and longer timescale) variations potentially somewhat suspect in studies that rely heavily on dendroclimatic indicators.
>
> In selecting data from the International Tree-Ring Data Bank (ITRDB), MBH98 set criteria designed to minimize these problems. Starting with the full data bank available in 1997, they identified 1589 site chronologies, each representing a unique combination of species, variable (e.g. ring width or maximum latewood density) and location. Only the 251 chronologies that met the following criteria were retained:
>
> - Reliable information on the methods used to remove biological trend was available;
> - The median length of the individual segments used to build the chronology was greater than 150 years;
> - The mean correlation of these individual segments with the site chronology was greater than 0.5;
> - The first year of the chronology was before AD 1626, and it contained at least 8 segments by 1680;
> - The last year was after 1970, and there were still 8 segments after 1960.
>
> Of the 251 ITRDB chronologies that met these criteria, 229 were ring-width chronologies, and 22 maximum latewood density.

This article is available online,

http://www.ngdc.noaa.gov/paleo/ei/

with the specific discussion above here:

http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html

3. MBH98 states that "the dendroclimatic data used were carefully screened for conservative standardization and sizeable segment lengths." We have the text of an email in which the term "wild guess" was used to describe the inclusion or exclusion of certain series. In this case, the deletion of various series is recommended, but this recommendation is not implemented in the work shown at the FTP site.

At the time this work was done (1996, 1997) few tree-ring data meeting these criteria were available in the ITRDB for Siberia. However, as we were preparing the dataset, we obtained a 1996 book by three of Dr Hughes' Russian colleagues, containing listed mean tree-ring chronologies for 61 sites in northern Siberia. This was, until very recently the way Russian colleagues made their data publicly available. These are the "Vaganov" data mentioned above. Given Dr Hughes' long-standing collaboration in field and laboratory with the authors of the Russian book, his detailed knowledge of their working methods, and his own direct experience of working with the same species, with these colleagues, in the same region, he recommended that we fill this major spatial gap by digitizing these data, and also made recommendations on which should be used.. The complainants quote Malcolm Hughes out of context from an email pre-dating our publication. Our procedure was in fact as objective and rigorous as possible.

> *4. MBH have deleted portions of at least 3 series without explanation to the reader. In each case, the FTP site contains a parallel series with the correct data. The deletions include the first 70 years of the Central England historical temperature series, the first 25 years of the Central Europe historical series and the first 2 years of chin04. In the first 2 series, the issue is not merely that early decades were removed but that summer (JJA) data was substituted for annual data without notice to the reader.*

Each of these claims is false or disingenuous, and again they are distasteful. The claim that MBH98 unjustifiably eliminated the earlier part of the Central England temperature (CET) record is unfounded. The decision by MBH98 not to use this record prior to 1730 is in keeping with previous studies (e.g. Bradley and Jones, 1993) and is based on the existence of inhomogeneities that exist in the record prior to the 1730s (Manley, 1953;1974). Manley (1974), for example, describes a gap from 1707 to 1722 in daily data from the region of Central England he was seeking to cover, and used an adjusted version of data from the Netherlands to fill this gap. Since 1722 falls within the stepwise reconstructions for 1700-1980 and 1730-1980, 1730-1980 is the first interval of our step-wise reconstruction that includes the indicator.

The use of the summer mean version of these series was in keeping with the use of these series by Bradley and Jones (1993). The MBH98 reconstruction, furthermore, is completely insensitive to the use (as in Bradley and Jones, 1993) of summer (rather than annual) mean CET and Central European temperature series. We have updated the references for these two series in the revised supplementary information to "Bradley and Jones, 1993" to avoid confusion, and have indicated that summer mean, rather than annual means, of the series were used by MBH98. We find it peculiar that the complainants would raise this particular complaint, when the interval over which their putative reconstruction differs from ours (the 15th century) predates either of these two series in any case.

5. MBH have recently stated that the UVA FTP site is the repository of data actually used in MBH98. This site contains at least 10 series not used in MBH98, including an instrumental temperature series with decreasing 20^{th} century temperatures.

Nowhere have we indicated that every single individual series in those directories were used in our analysis. The directories include the tree-ring data used to construct PC series of the various proxy networks, as well as the networks of the PC series themselves, and there are some proxy data that are not listed in the supplementary information, and were in fact never used. We do not see why this is a valid point of criticism. We must stress that we were never under any known requirement to post these data in the first place, and only did so as a service to our scientific colleagues. If *Nature* had asked for all of the proxy data used at the time we published our paper, we would have, of course, provided all those we were at liberty to pass on.

6. Some series in the North American PC region occur in duplicate versions. There is no rational description of the reasons for inclusion or exclusion as individual proxies.

This claim is incorrect with one exception. An analysis of the data file of all 415 individual series used (available through the revised supplementary information) reveals two series that are indeed the same: the Jacoby et al Northern Treeline series #11 (as provided to the authors by Gordon Jacoby) and North American ITRDB series 'cana036.txt'. The latter was only used by MBH98 after AD 1450, and elimination of the series from the North American ITRDB dataset used by MBH98 yields no detectable influence on the MBH98 reconstruction (Mann et al, manuscript in review). Two other series directly provided by David Stahle (series 'swmxdfew11.dat' and 'swmxdfew01.dat') are the same prior to AD 1488. The MBH98 reconstruction, again, is robust to the exclusion of either or both series from the data set (as shown elsewhere).

Indeed, the MBH98 method is, by its very construction, robust with respect to whether a moderate amount of redundant information is used in the predictor network, so the criticisms here are irrelevant to the question of whether or not the complainants claimed 'correction' of the MBH98 reconstruction is valid (which, as we have shown elsewhere, it most certainly is not).

7. The SWM region used in MBH98 includes a site (Spruce Canyon CO), which is not in the site roster of the original article and which is in the roster of the NOAMER region. This apparently small point has a significant impact on 15th century values.

The corrected list of proxy data has been provided with our revised supplementary information. Our reconstruction is in no way, despite the claim made by the complainants, sensitive to whether or not this particular tree-ring series is used (as shown elsewhere). Any sensitivity to whether these data are used in the analysis performed by McIntyre and McKitrick almost certainly arises from the fact that the authors have censored approximately 80% of the proxy data used by MBH98 prior to AD 1450 and 70% prior to AD 1500 in their analysis, yielding in principle much greater sensitivity to the small number of remaining data actually used by them over that time interval.

> *8. Some data versions used in MBH98 were obsolete when the paper was published. Many more versions are now obsolete. The SI refers to WDCP as a reference for tree ring data, but this reference is inaccurate given the material differences between the version used in MBH98 and the version actually archived. Again, the differences can be material. There are other inaccuracies in the references of MBH98: for example, the citation Bradley-Jones 1992 does not contain several instrumental sites referred to in MBH98.*

This statement is absurd. We listed the specific data used by us (albeit with some typos, and incorrect references, as noted) in the supplementary information, and provided all of the data on our data site. We did not indicate there, or elsewhere, that all of the tree ring data used were available in the NOAA databank. The authors appear to be unaware of the fact that not all proxy data shared between fellow scientists are placed in the public data archives, nor are same versions of the proxy data always made available publicly. In general, there is no requirement for scientists to post their proxy data in these public archives, although they are encouraged to do so.

In at least one instance that we have pointed out, the complainants have confused certain unrelated data from the NOAA website with data that were in fact never made available by the original authors to the NOAA website, but which were made available to us. The complainants, based on a thorough misunderstanding of how PC series of tree-ring data were calculated, and other mistakes, have replaced our network with a highly depleted version of the network, in which roughly 80% of the proxy indicators used by MBH98 prior to AD 1600 were eliminated by them in their supposed reproduction of our analysis.

The Bradley and Jones (1992) chapter was used as the best available reference for long instrumental temperature and precipitation records. That doesn't mean that every single instrumental record used (and described in our supplementary information) was shown in the article. We have updated this reference with a description of the specific source of the long instrumental data series and means by which they were gridded, in the revised supplementary information.

> *9. The disclosure of methodology for calculating temperature principal components is inaccurate. MBH98 describe their methodology as "conventional". Conventional principal components fail with missing data. The underlying data set contains much missing data and some other procedure was necessarily used.*

This represents the complainants' fundamental mistake of using a different version of the CRU surface temperature data site from the one that we used. The pattern of missing data in the two datasets is different, owing to the use of different reference periods. Small gaps in the instrumental record we used (an older CRU surface temperature dataset which goes back to 1856) were filled by linear interpolation, as described in our revised supplementary information.

> *10. The disclosure of methodology for calculating tree ring principal components is inaccurate. Again MBH98 methodology is not "conventional". In this case, the FTP site contains computer programs which show that the data was transformed in ways not disclosed in MBH98. These undisclosed transformations have a material impact on the final results.*

Each of these statements in incorrect. A conventional PCA was indeed used. The authors apparently failed to take note of the stepwise procedure used by us, and described in our paper. This procedure allows PC series to be calculated independently for each sub-interval (e.g. 1820-1980, then 1780-1980, ..., 1400-1980) to allow for the use of an increasing number of data in the different sub-networks increasingly later in time. The misunderstanding of this procedure led to them eliminating roughly 80% of the proxy indicators used by us prior to AD 1600, the primary reason for the spurious result that they have reported. Precise details regarding how the data were standardized are provided in the revised supplementary information. We have shown elsewhere that the MBH98 reconstruction is in fact entirely robust with respect to whether or not the proxy series were standardized by the detrended or raw calibration period variance.

Once again, all of the original proxy data used, and all of the PC series used, were available on the public ftp site from July 2002, though the complainants did not download and use the correct data. The new, revised ftp site provides the data and listings of data in a thoroughly documented manner such that similar mistakes should not be possible in the future.

APPENNDIX C

DRAFT MBH CORRIGENDUM, MARCH 2004

corrigendum

Global-scale temperature patterns and climate forcing over the past six centuries

Michael E. Mann, Raymond S. Bradley & Malcolm K. Hughes

Nature 392, 779–787 (1998).

It has been drawn to our attention (S. McIntyre and R. McKitrick) that the listing of the 'proxy' data set in the Supplementary Information published with this Article contained several errors. In Table 1 we provide a list of the records that were either mistakenly included in the Supplementary Information, or mistakenly left out. A small number of other corrections of the original listing include (see Table 1) corrections of the citations originally provided, or corrections of the start years for certain series.

Table 1 **Errors in 'proxy' data set listing in ref. 1**

Series (34) listed in original Supplementary Information but not used in ref. 1.*
FRAN003; ITAL015 and ITAL015X; SPAI026 and SPAI047; NEWZ056; ARGE030, ARGE060 and ARGE065; CHIL015, CHIL016, CHIL017 and CHIL018; AK006 and AK006X; CA070; CANA053, CANA053X, CANA096, CANA096X, CANA099, CANA106 and CANA110; WA019, WA025, WA027, WA033, WA039, WA041, WA071, WA074, WA086, WA088 and WA091
Series (2) used in ref. 1 but not listed in original Supplementary Information
Unpublished Southwest US/Mexico Density series (D. W. Stahle, personal communication)
Unpublished Southwest US/Mexico Latewood Width series (D. W. Stahle, personal communication)
Additional minor corrections
(1) The Central England and Central European temperature records used by ref. 1 were the summer season versions of these series as used by ref. 2.
(2) The 'long instrumental' series used in ref. 1 are station temperature and precipitation station data from the NOAA Climate Data centre gridded at 5° latitude/longitude resolution.
(3) The start year for the 'Central Europe' series of ref. 1 is AD 1525.
(4) The 'Western North America Dendro density' series used in ref. 1 should properly be attributed to ref. 3.
(5) The Stahle *et al.* Southwestern/Mexico late wood width and maximum density data used in ref. 1 should properly be attributed to ref. 4 (the formal reference was not available at the time of ref. 1), or, in two cases, unpublished data (D. W. Stahle, personal communication).
(6) For one of the 12 'Northern Treeline' records of Jacoby *et al.* used in ref. 1 (the 'St Anne River' series), the values used for AD 1400–03 were equal to the value for the first available year (AD 1404).

* These series, all of which come from the International Tree Ring Data Bank (ITRDB), met all the tests used for screening of the ITRDB data used in ref. 1 (see ref. 5), except one—namely, that in 1997, either it could not be ascertained by the authors how these series had been standardized by the original contributors, or it was known that the series had been aggressively standardized, removing multidecadal to century-scale fluctuations.

The full, corrected listing of the data is supplied as Supplementary Information to this corrigendum. Also provided as Supplementary Information are a documented archive of the complete data (instrumental and 'proxy' climate series) used in our original study, and an expanded description of the methodological details of our original study. ☐

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1. Mann, M. E., Bradley, R. S. & Hughes, M. K. Global-scale temperature patterns and climate forcing over the past six centuries. *Nature* **392,** 779–787 (1998).
2. Bradley, R. S. & Jones, P. D. "Little ice age" summer temperature variations: their nature and relevance to recent global warming trends. *The Holocene* **3,** 367–376 (1993).
3. Briffa, K. R. *et al.* Fennoscandian summers from AD500: temperature changes on short and long timescales. *Clim. Dyn.* **7,** 111–119 (1992).
4. Stahle, D. W. *et al.* Experimental dendroclimatic reconstruction of the Southern Oscillation. *Bull. Am. Meteorol. Soc.* **79,** 2137–2152 (1998).
5. Mann, M. E. *et al.* Global temperature patterns in past centuries: An interactive presentation. *Earth Inter.* **4-4,** 1–29 (2000).

Supplementary Information accompanies this corrigendum on **www.nature.com/nature.**

APPENDIX D
COMMENTS TO NATURE ON PROPOSED CORRIGENDUM

From: Steve McIntyre

Sent: March 17, 2004 12:08 AM

To: Ashman, Dinah

Subject: Fw: Nature corrigendum Mann02478

Dear Dinah,

I have attached the enclosed email as a word document, which may be easier to read.

Regards, Steve McIntyre

I apologize for the length of this email, but sometimes it's hard to write short. Regardless of the disposition of the comments below, we appreciate the steps which Nature has already taken and the ongoing consideration. The following observations are sent to you on the basis that it's easier to deal with all pertinent issues while the file is open. To expedite matters, I have taken the liberty of attaching a list of confirmations and clarifications which I believe to be required, which you may use as you see fit.

Listing of Series

1. The original SI lists 61 Vaganov series as being used, but, in fact, only 60 series were used, with series 55 being excluded. This is not mentioned in the correction, but should be. This presumably has a different explanation than the exclusion of the ITRDB series. The SI does not identify the Vaganov series.
2. The original SI identified 10 Stahle SWM sites; the site for the two "unpublished" Stahle SWM series is not identified in the Corrigendum. It needs to be identified, especially since Item 6 in the response letter by Prof. Mann forwarded to me by Heike Langenburg on Feb. 27, 2004 (the Response Letter) states that there is substantial duplication of values in several SWM series, indicating the possibility of duplicated series.
3. The Response Letter stated that one of the excluded series, "arge030", was excluded because a more recent and more conservatively standardized replacement series was available "directly" from a colleague. In the South American network involved, it is the more recent series that have been excluded and the older series which have been used. We are unable to identify the "more recent" series used to replace this series.
4. We strongly urge that you ensure that the SI attached to the correction of these discrepancies shows the application of the criteria listed in the asterisked explanation for the discrepancies. In particular, these are the criteria listed by Professor Mann in ref. 5 or his previous response:
    1. the availability of "reliable information" on the methods used to remove biological trend;

(2) the median length of the individual segments used to build the chronology, showing that this exceeds 150 years;

(3) The mean correlation of these individual segments with the site chronology, showing that these exceed 0.5;

(4) The first year of the chronology, showing that this was before AD 1626,

(5) The number of segments by 1680, showing at least 8;

(6) The last year of the chronology, showing that this was after 1970;

(7) The number of segments after 1960, showing that this was at least 8 segments;

(8) Whether the series was deleted because a replacement series was available "directly" from a colleague. If this criterion is used, full particulars on the replacement series should be provided including all of the information listed in 1-7 above, together with evidence showing that the replacement series was more "conservatively" detrended.

We tested out these rules on Professor Mann's data, and found numerous examples where they break down. For instance, site cana153, a Jacoby treeline series, exhibited extremely low correlations (<0.045) between individual segments and the site chronology, violating criterion (3). We made an inquiry about the low correlations to the originating author, Professor Rosanne d'Arrigo. She discovered thereupon that the wrong site chronology had been published and promptly notified WDCP to withdraw the series. It is hard to see how the above quality control rules could have been applied as claimed by Professor Mann, as this problem would surely have come to light sooner. Based on our investigation, regarding the MBH data archive, relative to the criteria set out above:

- 39 series had not commenced by 1626;
- 22 sites did not have 8 trees by 1680;
- 171 sites had less than 0.5 mean correlation of the individual trees with the site chronology.

MBH Region	# Sites at SI	Sites not located at WDCP	# Sites not used	Different site chrono-logy	# Not present in 1626	Less than 8 trees in 1680	Mean correlation <0.5
OK	14	0	0	14	12	14	4
SWM	20	0	*!	20	2	4	9
NOAMER	233	1	21	61	3	3	117
SOAMER	18	0	7	0	0	0	15
AUSTRAL	16	0	1	0	1	0	11
VAGANOV	61	61*	1	NA	20	NA	NA
MISCELLANEOUS	19	0	5	3	1	1	15
	381	62	35	98+	39	22	171

Additional minor corrections - comments

5. The original SI attributed the long instrumental series referred to in minor correction (2) to Bradley and Jones (1993). Without reference to the original citation, the nature of the correction is not clear. Also, the citation as it stands does not identify the NOAA Climate Data centre series used – is it GHCN v1, GHCN v2 or some other iteration. The MBH response letter states "We have updated this reference with a description of the specific source of the long instrumental data series and means by which they were gridded, in the revised supplementary information." A little more detail in the line item in the Corrigendum would be helpful.
6. The language in minor correction (3) is very unclear. The original SI states that the "earliest available" year for the Central Europe series is 1550, while the earliest available year in the cited publication is 1525. The first year actually used in MBH98 is 1550 – which is at odds with the language of this item. I would suggest language like: "The start year for the Central Europe series of ref. 1 was 1550, applying the version used in ref. 2, while the start year for the Central Europe series in the reference cited in the original SI was 1525."
7. This has several errors in a very short space. The original SI listed earlywood widths and latewood widths (not maximum density). Ref. 4, now provided as the supposedly correct citation for the SWM series, does not list any latewood width or maximum density series, but does list earlywood width and total width series. The sites listed in Ref 4. do not coincide with the sites listed in the original SI, with some of the sites listed in the original SI not listed in Ref. 4 and some sites listed in Ref. 4 not listed in the original SI. This needs to be dealt with properly. Finally, as discussed in prior correspondence, several series in this dataset have identical values in the early years. The sites for these series need to be clearly identified and any duplication corrected.
8. Jacoby et al, use 11 northern treeline sites, not 12 as referred to here.

Other unlisted data corrigenda

9. Although (3) above discusses the correction for the "earliest available" start year for Central Europe, there is a similar issue for Central England, not referred to. The original SI stated that the "earliest available" year was 1730, while the earliest year in the citation was 1659. This should also be clarified along the lines of the language suggested above.
10. There is a similar issue for the series chin004, in which the earliest year in the citation is 2 years earlier than the first year used in MBH98 and a similar clarification should be made.
11. There are many geographical errors in the original SI in respect to the grid cell locations of the long instrumental series, especially in the precipitation series. The Paris France precipitation series is incorrectly located in a New England grid cell. These are known to Mann et al., are not listed in the errata, but should be identified and dealt with at this time.
12. The geographical locations of the Central Greenland stack in the original SI is incorrect.
13. The geographical locations of the "npatagonia" and "cpatagonia" series are reversed in the versions used at the FTP site.
14. We believe that that there are errors in the listing of the total number of indicators stated in Nature to have been used on an interval-by-interval basis. For example, from

the FTP site information, it appears that 3 non-principal component series start in the AD1450 interval; a Vaganov PC1 is also stated in the SI to have commenced in this interval, making 4 in total, but the total number of indicators is shown as increasing only by 3. Also, MBH have stated that 159 series were used; we are unable to replicate this number with the number of indicators stated in Nature to have been used on an interval-by-interval basis. I suggest that you ask Prof. Mann to check his calculations on the number of indicators used and to include any required corrections at this time.

Methodological corrigenda

In his prior response and in other recent comments, Prof. Mann has referred to a number of corrections to the methodology description, which have not been listed in the corrigendum (and which would seem to require an explicit reference.) These include the following corrections to existing methodological descriptions:

15. MBH's response letter stated: "Small gaps in the instrumental record we used (an older CRU surface temperature dataset which goes back to 1856) were filled by linear interpolation, as described in our revised supplementary information." This is actually a pretty material issue. The original methodological description in MBH98 stated that "conventional" principal components were used – which require no missing data. There is actually a lot of missing data in the CRU dataset. The gaps are not necessarily small. For example, in the current CRU version, 4 grid cells used in MBH98 are completely empty. While the versions are different and we do not presently have access to the CRU version in MBH98, we would be very surprised if there were only "small gaps" in these data cells in the older version and no data at all in the current version. Thus, MBH98 has to have some methodology to deal with large gaps. Since the original description was not fully accurate, we think that this deserves at least a line item in the corrigendum and hopefully comprehensive explanation in the revised SI, as indicated in the MBH response letter.
16. MBH98 stated that for the temperature data "the mean was removed, and the series was normalized by its standard deviation". Recently, Mann et al. stated that they used "de-trended gridpoint standard deviations" to normalize temperature data. Again, in view of the inaccurate prior description, a line item in the Corrigendum would appear to be warranted.
17. Mann et al. recently stated that 159 series were used in MBH98 calculations. The figure of **159 series** is not itself stated in Nature or the SI thereto, where a total of 112 indicators is mentioned. This is an acutely important point, as the failure to identify the correct number of series means that, before now, researchers could not have replicated the MBH98 results. This surely merits a separate line item in the corrigendum.
18. Related to 3 above, MBH changed rosters of tree rings in certain calculation intervals. The SI needs to show the exact number of principal components retained in each network in each period. There is no such information in MBH98. Despite careful study of MBH98 and specific requests to Prof. Mann, we were unable to discern this methodology within the MBH98 descriptions and strongly believe that it warrants a

line item in the Corrigendum. (Also Prof. Mann has made statements recently that the principal components were re-calculated for every index calculation interval – which, on the basis of the FTP site, was not the case. While these recent statements are not the responsibility of Nature, clarification is much needed on this matter.)

19. Mann et al. state in their response letter that "Precise details regarding how the [tree ring data] data were standardized are provided in the revised supplementary information". This acknowledges that such information was not provided in MBH98 or the SI thereto. Since the "standardization" procedures carried out for the tree ring principal component calculations are far from conventional and were undisclosed, a separate line item is likewise indicated.

Supplementary Information

20. We originally requested the following information in connection with MBH98:
    (1) the identity of the 159 series now said to be used in MBH98 calculations;
    (2) identification of the PC proxy rosters (and sub-directory calls) on a period-by-period basis.;
    (3) residual series, together with programs used in the derivation of residuals and confidence intervals in MBH98;
    (4) programs used in MBH98 calculations;

Items (1) and (2) were referred to above, but (3) is important for the full consideration of the stepwise methods of MBH98, especially for the AD1400 interval. We believe that the following information should be included in the new SI:

1. the results of the application of the quality control criteria set out in ref. 5, showing the compliance of the various series with each listed criterion;
2. The standardization information used by the authors in 1997 to distinguish between retained and excluded series;
3. The identity of the 159 series now said to be used in MBH98 calculations;
4. identification of the number of principal component series used in each interval for each of the 6 tree ring networks;
5. the results of each of the stepwise calculations – in particular, the AD1400 step, including residuals;
6. The original CRU dataset used in MBH98 – both for the entire grid and for the NH average in the verification period;
7. the correct geographic locations of the various instrumental temperature and precipitation series;

These are all required to carry out replication of the procedures in MBH98. Rather than continuing ongoing requests and disputes over the provision of this data, it makes far more sense to provide the data in the revised SI in a systematic basis.

We have also requested that the MBH98 programs be included in the SI. A reviewer of an article which we submitted on this topic to Nature stated that "there is a wide margin of uncertainty that could be resolved only by looking in detail into the whole data set and the whole software used by the authors." In our revision, we plan to include all the software which we used in out reconstruction in order to facilitate this reconciliation. It would make a lot of sense for Mann et al. to do the same.

Conclusion

Under the circumstances, I suggest that you ask Prof. Mann to warrant that, to the best of his knowledge, the corrigenda are complete. While we have examined his data set diligently and have identified a number of discrepancies, the issuance of a correction, for an erratum known to the authors, should not be dependent on whether we have brought it to your attention.

Again, we appreciate your consideration and apologise for the length of these comments, but we could see no obvious way of shortening them. For some of the methodological descriptions which we have referred to, we require them for our study of MBH98.

I have taken the liberty of re-stating the above comments in the form of a draft letter to Professor Mann, which you may use or disregard as you see fit.

List of Confirmations for Prof. Mann

1. Confirm whether 61 Vaganov series were used in your calculations as stated in the original SI. (We have been advised that series 55 was not used.) If not, please amend the list of (34) series and the asterisked explanation.
2. Please include identification of the 61 Vaganov series.
3. Please identify the "more recent" replacement series provided directly from a colleague used as a replacement series for arge030, as described in your Response Letter.
4. Confirm that the asterisked explanation for exclusion of listed series is a complete explanation, including application to series arge030.
5. Provide an identification of the location of the two unpublished Stahle/SWM series, which were used but not listed. In this connection, please confirm that the duplication of early values, acknowledged in Item 7 of your Response Letter, are not errata in the unpublished data.
6. Please ensure that the SI attached to the correction of the listing discrepancies completely shows the application of the criteria listed in the asterisked explanation for the discrepancies, including, in particular, the following criteria listed in the Response Letter (as stated in ref. 5) or his previous response:
    1) the availability of "reliable information" on the methods used to remove biological trend;

2) the median length of the individual segments used to build the chronology, showing that this exceeds 150 years;
3) The mean correlation of these individual segments with the site chronology, showing that these exceed 0.5;
4) The first year of the chronology, showing that this was before AD 1626,
5) The number of segments by 1680, showing at least 8;
6) The last year of the chronology, showing that this was after 1970;
7) The number of segments after 1960, showing that this was at least 8 segments;
8) Whether the series was deleted because a replacement series was available "directly" from a colleague. If this criterion is used, full particulars on the replacement series should be provided including all of the information listed in 1-7 above, together with evidence showing that the replacement series was more "conservatively" detrended.

7. In the Corrigendum minor correction (2), please identify the exact NOAA data set used, together with the data set stated to have been used in the original citation.
8. Please clarify Corrigendum minor correction (3) along the following lines: "The start year for the Central Europe series of ref. 1 was 1550 (not 1525 as stated in the Corrigendum), applying the version used in ref. 2, while the start year for the Central Europe series in the reference cited in the original SI was 1525."
9. Please provide a similar correction in respect to the Central England series and for the chin004 series, if applicable.
10. Please confirm that SWM series used in MBH98 included maximum density and latewood width series, as stated in the Corrigendum, rather than earlywood width and latewood width series as stated in the original SI. If the SWM sites listed in the revised SI differ from the sites listed in the original SI, then please include a comment to this effect in the Corrigendum. If this is not the case, please confirm that all the sites listed in the SI are listed in the amended citation (ref. 4) and conversely, that all the series listed in ref. 4 are included in the SI. We are advised that, notwithstanding the Corrigendum, ref. 4 does not contain any references to maximum density or latewood widths. Would you please confirm that ref. 4 does in fact report on maximum density and latewood widths, as stated in the Corrigendum.
11. Would you please confirm that there are no geographical errors in the original listing of instrumental or other series. If there are any such errors, would you please ensure that there is a reference in the Corrigendum to these errata.
12. Would you please confirm that the total number of indicators in the original article and SI on an interval-by-interval basis are correct. If not, would you please include an item in the Corrigendum.
13. For methodological description items discussed in the revised SI, would you please confirm that none of the procedures described are inconsistent with the description in MBH98 and that MBH98 contained no material omissions in its methodological description. If there were any inaccuracies or material omissions in MBH98, would you please insert a line item in the Corrigendum for each such item. Without limiting

this question, we ask for specific confirmation below on several methodological issues brought to our attention.

14. For temperature data, would you please confirm that in your calculations "the mean was removed, and the series was normalized by its standard deviation". If the series was in fact normalized by its detrended standard deviation, would you please include an appropriate line item in the Corrigendum.
15. For the calculation of temperature principal components, would you please define what is a "small gap" as mentioned in your Response Letter. Would you please confirm the comment in your Response Letter that only "small gaps" as so defined have been interpolated. We suggest a line item in the Corrigendum disclosing the interpolation procedure.
16. Would you please advise us whether 159 series were used in MBH98 calculations. If this is the case, would you please include a line item in the Corrigendum referring to this and sufficient information in the SI that the 159 series can be completely identified.
17. Would you please confirm that the revised methodological description in the new SI includes an explanation of how the number of principal component series retained in each network for each interval are determined.
18. Would you please confirm that the new SI enables the determination of the 251 series mentioned in ref. 4 and also the 415 series referred to in question 6 of your Response Letter.
19. Would you please confirm that the new SI describes the procedure used to standardize tree ring data. If this procedure is material, please include a reference in a line item pursuant to (12) above.
20. Would you please confirm that, (a) to your knowledge, there are no remaining errata in data or data citations in MBH98 which have not been disclosed; (b) to your knowledge, the methodological description contained in the new SI is sufficient to enable replication of your results, using the data provided in the new SI or cited from public archives.

APPENDIX E

REPLY FROM NATURE TO COMMENTS ON CORRIGENDUM

From: Langenberg, Heike
Sent: March 26, 2004 10:58 AM
To: Stephen Mcintyre
Subject: RE: Nature corrigendum Mann02478

Dear Dr McIntyre,

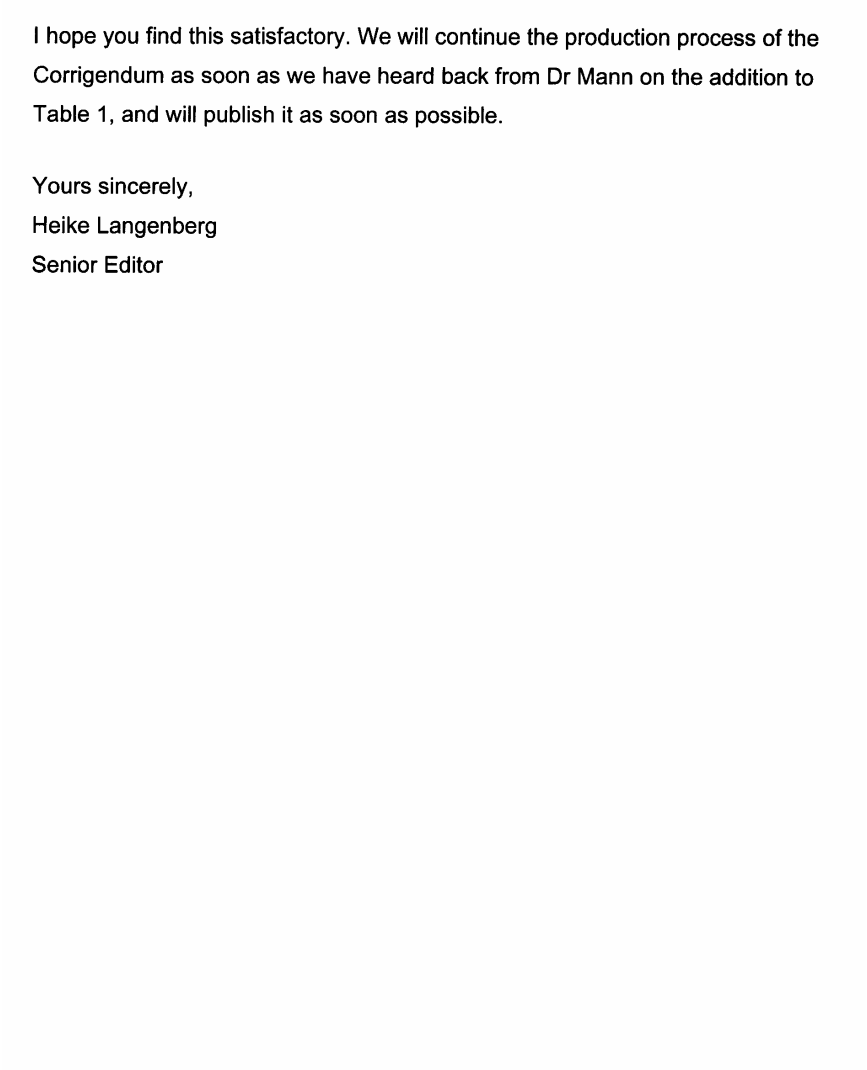
Thanks for your further notes regarding your materials complaint, which my colleague Dinah Ashman has forwarded to me for assessment.

In response to the points you raise, we have asked Dr Mann to include the time series Vaganov #55 in Table 1 of the Corrigendum. We feel that the additional issues mentioned under "Listing of the Series" are not directly relevant to the materials complaint. Instead you question the consistency of the methods used, which is not the subject of a Corrigendum.

You also make a number of additional comments to the Corrigendum, but for reasons of space constraints, we insist that such publications are as concise as possible. We feel that the current version, together with the Supplementary Information explicitly listing the data sets and methods used, clearly establishes which data were used in the paper.

Thank you for offering to review the new Supplementary Information for us.

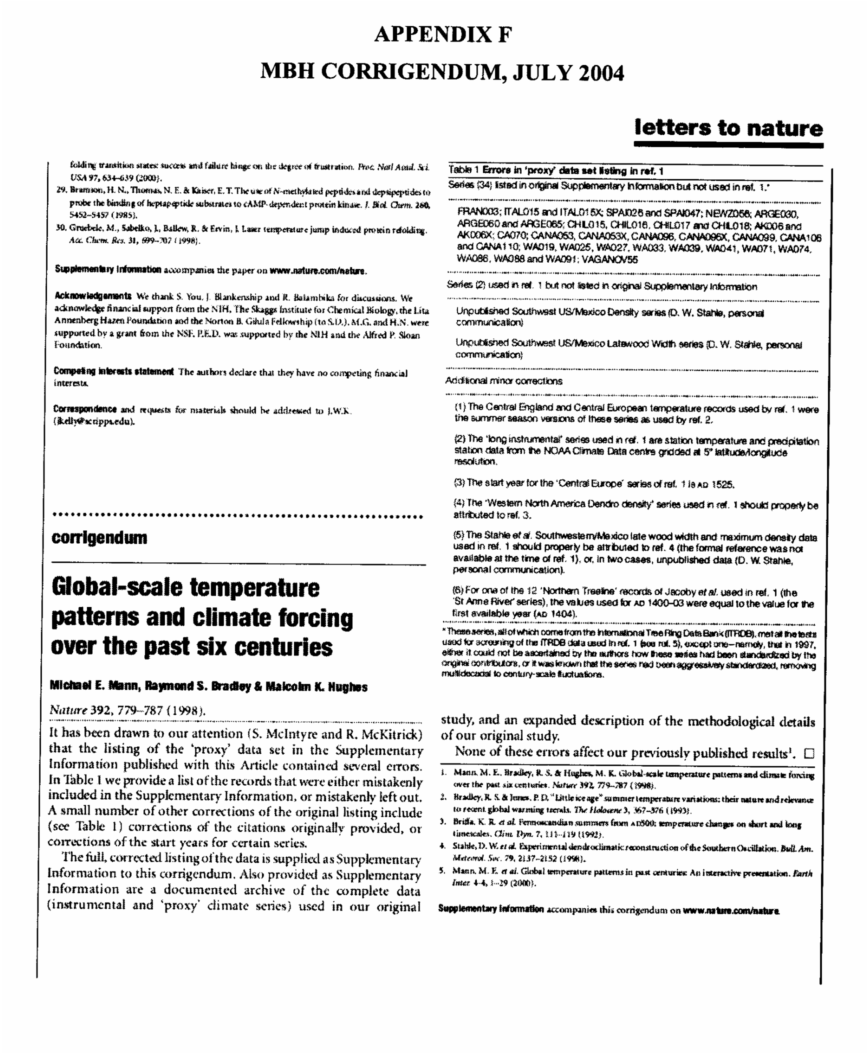
We appreciate that you give us the option, but as you anticipate, we prefer to handle the matter differently.



I hope you find this satisfactory. We will continue the production process of the Corrigendum as soon as we have heard back from Dr Mann on the addition to Table 1, and will publish it as soon as possible.

Yours sincerely,
Heike Langenberg
Senior Editor

APPENDIX F
MBH CORRIGENDUM, JULY 2004



folding transition states: success and failure hinge on the degree of frustration. *Proc. Natl Acad. Sci. USA* 97, 634–639 (2000).
29. Bramson, H. N., Thomas, N. E. & Kaiser, E. T. The use of *N*-methylated peptides and depsipeptides to probe the binding of heptapeptide substrates to cAMP-dependent protein kinase. *J. Biol. Chem.* 260, 5452–5457 (1985).
30. Gruebele, M., Sabelko, J., Ballew, R. & Ervin, J. Laser temperature jump induced protein refolding. *Acc. Chem. Res.* 31, 699–707 (1998).

Supplementary Information accompanies the paper on **www.nature.com/nature**.

Acknowledgements We thank S. You, J. Blankenship and R. Balambika for discussions. We acknowledge financial support from the NIH, The Skaggs Institute for Chemical Biology, the Lita Annenberg Hazen Foundation and the Norton B. Gilula Fellowship (to S.D.). M.G. and H.N. were supported by a grant from the NSF. P.E.D. was supported by the NIH and the Alfred P. Sloan Foundation.

Competing interests statement The authors declare that they have no competing financial interests.

Correspondence and requests for materials should be addressed to J.W.K. (jkelly@scripps.edu).

……

corrigendum

Global-scale temperature patterns and climate forcing over the past six centuries

Michael E. Mann, Raymond S. Bradley & Malcolm K. Hughes

Nature 392, 779–787 (1998).

It has been drawn to our attention (S. McIntyre and R. McKitrick) that the listing of the 'proxy' data set in the Supplementary Information published with this Article contained several errors. In Table 1 we provide a list of the records that were either mistakenly included in the Supplementary Information, or mistakenly left out. A small number of other corrections of the original listing include (see Table 1) corrections of the citations originally provided, or corrections of the start years for certain series.

The full, corrected listing of the data is supplied as Supplementary Information to this corrigendum. Also provided as Supplementary Information are a documented archive of the complete data (instrumental and 'proxy' climate series) used in our original study, and an expanded description of the methodological details of our original study.

None of these errors affect our previously published results[1]. ☐

Table 1 **Errors in 'proxy' data set listing in ref. 1**

Series (34) listed in original Supplementary Information but not used in ref. 1.*
FRAN003; ITAL015 and ITAL015X; SPAI026 and SPAI047; NEWZ056; ARGE030, ARGE060 and ARGE065; CHIL015, CHIL016, CHIL017 and CHIL018; AK006 and AK006X; CA070; CANA053, CANA053X, CANA096, CANA096X, CANA099, CANA106 and CANA110; WA019, WA025, WA027, WA033, WA039, WA041, WA071, WA074, WA086, WA088 and WA091; VAGANOV55
Series (2) used in ref. 1 but not listed in original Supplementary Information
Unpublished Southwest US/Mexico Density series (D. W. Stahle, personal communication)
Unpublished Southwest US/Mexico Latewood Width series (D. W. Stahle, personal communication)
Additional minor corrections
(1) The Central England and Central European temperature records used by ref. 1 were the summer season versions of these series as used by ref. 2.
(2) The 'long instrumental' series used in ref. 1 are station temperature and precipitation station data from the NOAA Climate Data centre gridded at 5° latitude/longitude resolution.
(3) The start year for the 'Central Europe' series of ref. 1 is AD 1525.
(4) The 'Western North America Dendro density' series used in ref. 1 should properly be attributed to ref. 3.
(5) The Stahle *et al.* Southwestern/Mexico late wood width and maximum density data used in ref. 1 should properly be attributed to ref. 4 (the formal reference was not available at the time of ref. 1), or, in two cases, unpublished data (D. W. Stahle, personal communication).
(6) For one of the 12 'Northern Treeline' records of Jacoby *et al.* used in ref. 1 (the 'St Anne River' series), the values used for AD 1400–03 were equal to the value for the first available year (AD 1404).

* These series, all of which come from the International Tree Ring Data Bank (ITRDB), met all the tests used for screening of the ITRDB data used in ref. 1 (see ref. 5), except one—namely, that in 1997, either it could not be ascertained by the authors how these series had been standardized by the original contributors, or it was known that the series had been aggressively standardized, removing multidecadal to century-scale fluctuations.

1. Mann, M. E., Bradley, R. S. & Hughes, M. K. Global-scale temperature patterns and climate forcing over the past six centuries. *Nature* 392, 779–787 (1998).
2. Bradley, R. S. & Jones, P. D. "Little ice age" summer temperature variations: their nature and relevance to recent global warming trends. *The Holocene* 3, 367–376 (1993).
3. Briffa, K. R. *et al.* Fennoscandian summers from AD500: temperature changes on short and long timescales. *Clim. Dyn.* 7, 111–119 (1992).
4. Stahle, D. W. *et al.* Experimental dendroclimatic reconstruction of the Southern Oscillation. *Bull. Am. Meteorol. Soc.* 79, 2137–2152 (1998).
5. Mann, M. E. *et al.* Global temperature patterns in past centuries: An interactive presentation. *Earth Inter.* 4-4, 1–29 (2000).

Supplementary Information accompanies this corrigendum on **www.nature.com/nature**.

APPENDIX G
RE-ITERATED COMPLAINT, AUGUST 2004

Aug 10, 2004
McIntyre to Ziemelis

Dear Karl,

We are disappointed that Nature has decided not to publish our submitted Communication, especially as the principal grounds appear to be the small word allotment in the Communications Arising section. We respectfully disagree with the conjecture that our work would be of interest to only a few specialists. The original Mann et al. paper has been widely applied and our previous commentary attracted considerable public interest. Be that as it may, the referees expressly encouraged us to continue our analysis of MBH98 and of multiproxy calculations generally and one of them expressly stated that our efforts should not be "hampered".

In this spirit we are writing to reiterate long-standing requests for data and results from MBH98, which we have already communicated on several occasions. You had stated that these requests would be resolved in the new SI, but unfortunately this is not the case. While you are undoubtedly weary of this correspondence, our original request for disclosure was reasonable and remains reasonable. It is only the unresponsiveness of the original authors that is placing a burden on you and your associates. Some of these items have been outstanding for 7 months. They were not attended to in the new SI and need to be dealt with promptly.

In particular, we still seek:

1. the results of the 11 "experiments" referred to in MBH98, including:
    (a) the temperature principal components (69 series for all 11 steps);
    (b) the NH temperature reconstruction (11 series from the start of each calculation step to 1980);
    (c) the residuals (11 series from the start of each calculation step to 1980).
2. a list of the 159 series said to have been used in MBH98.
3. source code.

Since their claims of skill in reconstructing past climates depend on these "experiments" and their estimation of confidence intervals is based on the residual series, it is unnecessary to explain why these data are of interest. Again, we have repeatedly requested this data.

The new SI contains listings of the series used in the stepwise reconstruction. We collated the available series and were only able to identify 139 distinct series and we are concerned at the discrepancy. Since considerable emphasis has been placed by Mann et al. on the need to use 159 series, we re-iterate our request to see the actual list of these series – again a longstanding request.

The code is evidently at hand since it was used to generate the Supplementary Information for Mann's recent response to our submission to Nature. It is surprising that, instead of simply providing this code, the new SI only provides a verbal description of it (AlgorithmDescription.txt) which still does not suffice to permit exact replication. We do not understand why Nature has acquiesced in this.

In light of these continuing disclosure problems, as well as points raised in the exchange over our Communication, we are obliged to ask the following questions, which are either not clarified in the new SI or result from the new SI itself:

4. Mann et al. stated (in several places) that they carried out fresh calculations of principal component series for each region and each calculation interval. But the SI shows principal component series are not freshly calculated in every region and every calculation interval. What, then, is the criterion for deciding when to carry out a fresh calculation of the PC series in a region and calculation interval? Also, the present description of this process in the new SI is inaccurate and needs to be corrected.
5. In the Corrigendum, Mann et al. purport to explain the discrepancy between the listing of series in the original SI and the series actually used by reference to criteria stated in Mann et al (2000), which states that "clear a priori" criteria were used for proxy selection. But while the quality control rules ostensibly used for screening selected proxies are stated in the reference, the *a priori* criteria themselves were not stated in the reference or the new SI. What are the "clear a priori" criteria used for proxy selection in MBH98?
6. Mann et al. state that they use an "objective criterion" to decide how many principal component series to retain for each region and each calculation step. In the SI, they refer to consideration of both Preisendorfer's Rule N and to a Scree Test but do not state their "objective" criterion. Preisendorfer's Rule N describes simulations from white noise series. The Supplementary Information to Mann et al.'s second reply describes a simulation process based on red noise modeled with lag-one autocorrelation – a quite different procedure. Can you obtain a provide an exact and replicable description of the procedure used to decide the retention of principal component series?
7. In the Corrigendum, the use of an 11th site in the SWM group is admitted, but the source of the 11th site is stated only as Stahle, pers. comm., and no location is given. This is unsatisfactory, especially since two sites (yielding 4 series) have identical values for the first 120-125 years and it appears probable that the Stahle, pers. comm. data is simply a different edition of one of the other sites. Can you please obtain an identification of the location of the Stahle pers. comm. site?
8. The Corrigendum acknowledges that the citation for instrumental series attributed to Jones and Bradley (1992) was incorrect and stated that the series came from NOAA. There is no additional information in the SI. This new citation does not meet any data citation criteria since NOAA has thousands, if not hundreds of thousands, of different series. Can you please obtain an exact data citation?

9. Can you please obtain a listing of the 219 gridcells said to have been used in the verification process? We are unable to locate 219 gridcells with continuous data from 1854-1901 in the newly provided temperature data.

Again, we acknowledge that these requests can appear tiresome. But from our perspective, we are only seeking to obtain disclosure of MBH98 data and methods under Nature's stated policies- in particular that readers should be able to identify the data used and computational procedures applied. We have recently received requests for assistance from other parties, who have developed an interest in replicating the results of MBH98 and have advised them of what we perceive as the principal outstanding issues in obtaining disclosure from Mann et al.

In light of both Nature's policies, the comments from referees and your own prior commitments, we do not anticipate that there will be any problem in promptly requesting and obtaining the listed information, especially since some matters have been outstanding for so long. It would be helpful if you were able to provide us with an anticipated schedule so that we have a bring-forward date and do not have to trouble you with repeated requests on this matter.

Regards,

Steve McIntyre and Ross McKitrick

APPENDIX H
REPLY BY NATURE TO AUGUST 2004 COMPLAINT

Sept. 7, 2004
Ziemelis to McIntyre

Dear Steve,

My apologies once again for the length of time that it has taken us to get back to you. In contrast to your own views on the matter, your request for information is not one that we felt could be dealt with in a straightforward fashion, as it goes substantially beyond what we would normally expect authors to provide. In other words, it would involve more than simply insisting on compliance with our editorial policies, and as such we needed to consider your request very carefully before deciding how best to proceed - and for the reasons that I explained in my previous email, we were not able to do this immediately. And I am sorry to have to say that the upshot of these deliberations is that we feel that we have now done all that can be reasonably expected of us, when it comes to facilitating your interactions with Professor Mann. I will endeavour to explain in a bit more detail why we consider this to be so.

With reference to your three main points, I will deal with them in reverse order.

On the issue of source codes, we do not take the view that these are something that in general should automatically be provided on request - the decision of whether or not to do so normally rests with the authors of such codes. What we do consider to be a reasonable requirement is that the authors provide a detailed description of the procedures used, and this is indeed what Professor Mann has supplied in the corrected Supplementary Information (at our instigation, following your original communication with us).

Regarding the question of whether or not 159 series were used in MBH98, this we feel is an issue quite separate from the material that we have published and over which we are in a position to demand a response. Professor Mann has given us the clear understanding that the corrected Supplementary Information now lists *all* of the series used in the paper, and this list is consistent with statements in the original publication (MBH98). The fact that he has separately emphasised to you the need for a number of series greater than those listed in the Supplementary Information is, we feel, something that you should continue to pursue directly with him (along with your other requests for clarification).

And with regard to the additional experimental results that you request, our view is that this too goes beyond an obligation on the part of the authors, given that the full listing of the source data and documentation of the procedures used to generate the final findings are provided in the corrected Supplementary Information. (This is the most that we would normally require of any author.)

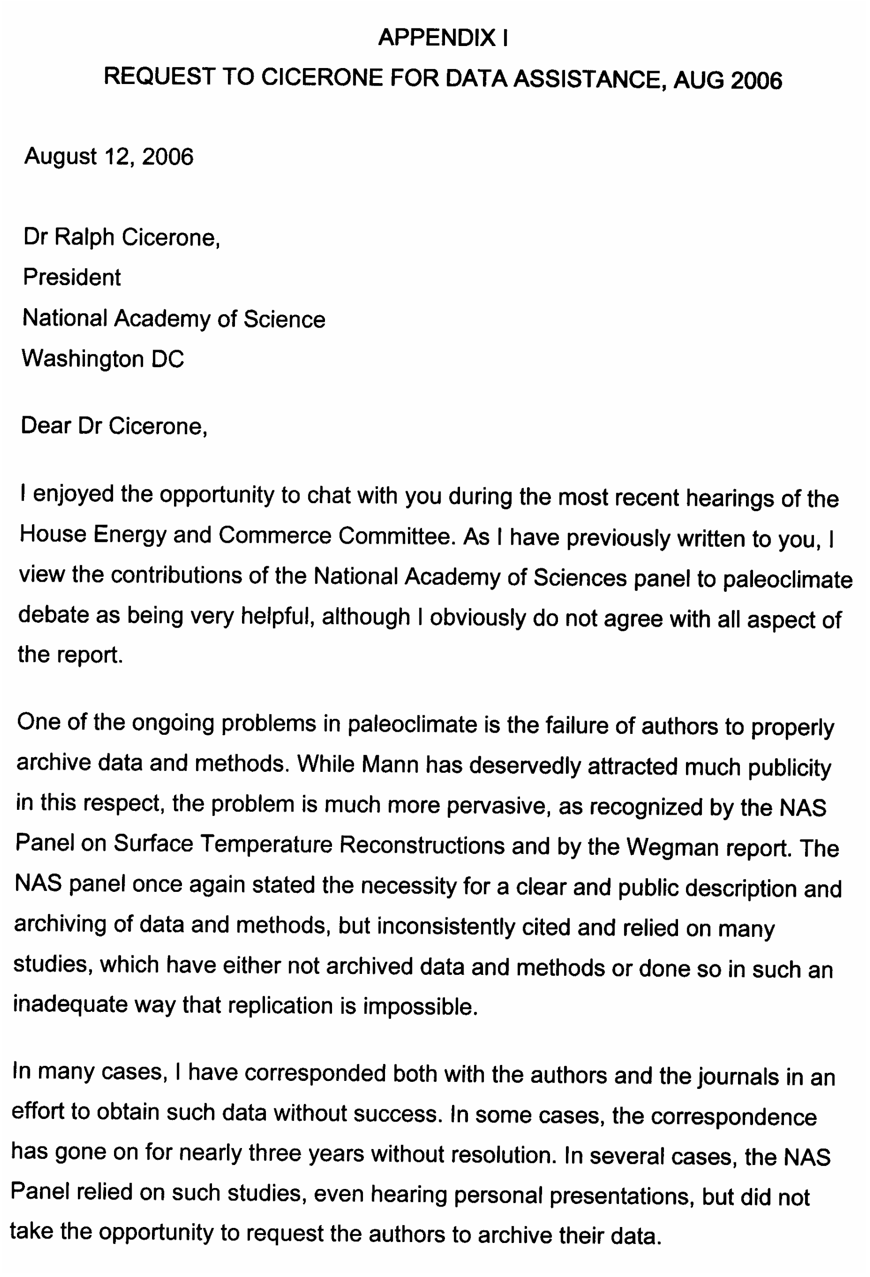
I do not want to give you the impression that we are dismissing your concerns out of hand. From the outset, our main concern has been to rectify the potential errors in the original MBH98 publication and to provide the data and materials used on our permanent website - the root causes of your initial frustrations. Indeed, I hope that you are at least in part reassured by the efforts that we (and Professor Mann) went to to rectify these problems in the form of the Corrigendum and the extended Supplementary Information. But having now rectified these problems, we feel that our role in the matter has concluded.

Of course, I realise that you will not be happy with this response. But once again, we feel that we have now taken our usual editorial procedures to a satisfactory conclusion. And let me assure you that we are fully appreciative of your input, which has undoubtedly enhanced the usefulness of the original paper for all readers.

Finally, we do hope once again that your ongoing interactions with Professor Mann reach a satisfactory outcome for all the parties involved.

Best regards,

Karl



APPENDIX I

REQUEST TO CICERONE FOR DATA ASSISTANCE, AUG 2006

August 12, 2006

Dr Ralph Cicerone,
President
National Academy of Science
Washington DC

Dear Dr Cicerone,

I enjoyed the opportunity to chat with you during the most recent hearings of the House Energy and Commerce Committee. As I have previously written to you, I view the contributions of the National Academy of Sciences panel to paleoclimate debate as being very helpful, although I obviously do not agree with all aspect of the report.

One of the ongoing problems in paleoclimate is the failure of authors to properly archive data and methods. While Mann has deservedly attracted much publicity in this respect, the problem is much more pervasive, as recognized by the NAS Panel on Surface Temperature Reconstructions and by the Wegman report. The NAS panel once again stated the necessity for a clear and public description and archiving of data and methods, but inconsistently cited and relied on many studies, which have either not archived data and methods or done so in such an inadequate way that replication is impossible.

In many cases, I have corresponded both with the authors and the journals in an effort to obtain such data without success. In some cases, the correspondence has gone on for nearly three years without resolution. In several cases, the NAS Panel relied on such studies, even hearing personal presentations, but did not take the opportunity to request the authors to archive their data.

However, now that the NAS has relied on these studies, it is of paramount importance that these studies are closely examined to determine if their conclusions are robust, or have limitations such as the NAS panel described for Mann's work.

I believe that a letter to authors who have refused to archive data and methods in a complete manner, coming from you in your capacity as President of the National Academies, which has just published a study relying on their reports, might be effective in achieving the mutually desired goal of inspiring the authors to archive their data and methods. In Lonnie Thompson's case, since some of the results have recently been published in the Proceedings of the NAS, the request could also be made via the journal.

In an Appendix to this letter, I have set out missing and pertinent data for six authors. Considering all of the above, I request that you promptly write to each of the authors asking that they promptly archive the data at the World Data Center for Paleoclimatology or other archive acceptable to the NAS. Thank you for your consideration.

Yours truly,

Stephen McIntyre

cc: Dr. Gerald North

APPENDIX TO CICERONE LETTER

1. Lonnie Thompson

For most sites for which anything has been archived, Thompson has only archived decadal δO18 data for a portion of the core. Ice cores contain much additional information. For all ice cores and pits from Dunde, Guliya, Dasuopu, Puruogangri, Quelccaya, Huascaran and Sajama, please request:

a) isotope and chemistry information by sample;
b) a detailed description of methods used to date ice cores;

For Quelccaya,

c) a list of all organic samples given identification numbers, together with radiocarbon dates for those samples that have been dated.

2. Rosanne D'Arrigo

Rosanne D'Arrigo presented to the NAS panel in March and D'Arrigo et al 2006 was relied upon by the NAS panel. The underlying data consists of measurement data and site "chronologies". Please request:

a) Site chronologies for all sites used in D'Arrigo et al 2006;
b) If some of the measurement data used in D'Arrigo et al 2006 has been archived at WDCP, exact data citations linking the regional groups in D'Arrigo et al 2006 to any archived measurement data.
c) For the sites in D'Arrigo et al 2006 where the measurement data is unarchived, the measurement data.
d) Any presently undocumented protocols used in D'Arrigo et al 2006 to make RCS chronologies from the measurement data;
e) a location map for the 1982 sampling location for the Gaspé site and for the 1992 update of the Gaspé site;
f) measurement data for the 26 sites collected for Jacoby and D'Arrigo 1989, but not included in their composite.

3. Gabrielle Hegerl

Gabrielle Hegerl presented to the NAS panel and Hegerl et al 2006 was relied upon by the NAS panel. Please request:

a) Identification of the sites used in this study.
b) If the data versions used in Hegerl et al 2006 are currently archived, exact data citations complying with AGU data citation policies i.e. a data citation to a digital file not to a generic print publication.
c) If unarchived data versions are used in Hegerl et al 2006, the digital versions as used, together with provenance.
d) detailed methodological description, including the step in which confidence intervals in Hegerl et al 2006 (Nature) are calculated.. In this case, source code would be desirable as descriptions to date cannot be followed.

4. Jan Esper or Edward Cook

The NAS panel relied on Esper et al 2002. After prolonged correspondence with Science and their eventual intervention, most, but not all, of Esper's data has been obtained. However, some is still missing. Science has been unsuccessful in clarifying the methodology. Once the NAS panel disbanded, *Science* ceased to be responsive on this file. Please request:

a) the Tarvagatny Pass chronology version and measurement data version as used in Esper et al 2002;
b) Confirmation by Esper that the measurement data archived in May 2006 by Lisa Graumlich is the same data as used in Esper et al 2002, or preferably, the measurement data for the two foxtail sites as used in Esper et al 2002
c) the criteria used by Esper to distinguish between linear and nonlinear sites;
d) the criteria used by Esper to decide on which cores to remove from a site data set.

e) detailed methodological information (preferably source code) which is sufficient to produce both the chronologies as used by Esper and their final result.

5. Tim Osborn or Keith Briffa

The NAS panel cited Osborn and Briffa 2006, featuring their data set in a major graphic. In this case, Science has been relatively cooperative, but again have been unable or unwilling to obtain a complete data archive or methodological description. Please request:

a) measurement data for the Tornetrask, Taimyr, Yamal and Jasper sites, which were relied on for chronologies used in Osborn and Briffa 2006. (The same data also was used in Hegerl et a 2006; D'Arrigo et al 2006, both also cited by the NAS Panel.)
b) an explanation why the Yamal data set was substituted for the updated Polar Urals data;
c) stations used in CRUTEM2 and HadCRU2 respectively for the gridcell 37.5N, 117.5W;
d) methodology used in HadCRU2 which resulted in supposedly "spurious" temperature values for 1870-1887 for the gridcell 37.5N, 117.5W;

6. Michael Mann

Mann was a coauthor of several studies cited by the NAS Panel, including Mann et al 1998, 1999, Mann and Jones 2003 and Rutherford et al 2005. Please request:

a) Identification of the 387 MXD series used in Rutherford et al 2005 (and previously in Briffa et al 2001 and other studies), including for each series the WDCP(ITRDB) identification where available, and, where unarchived, the digital versions of the unarchived series together with underlying measurement data.

b) The weights for each record in Mann and Jones 2003 and the method used to determine weights for each of the records.

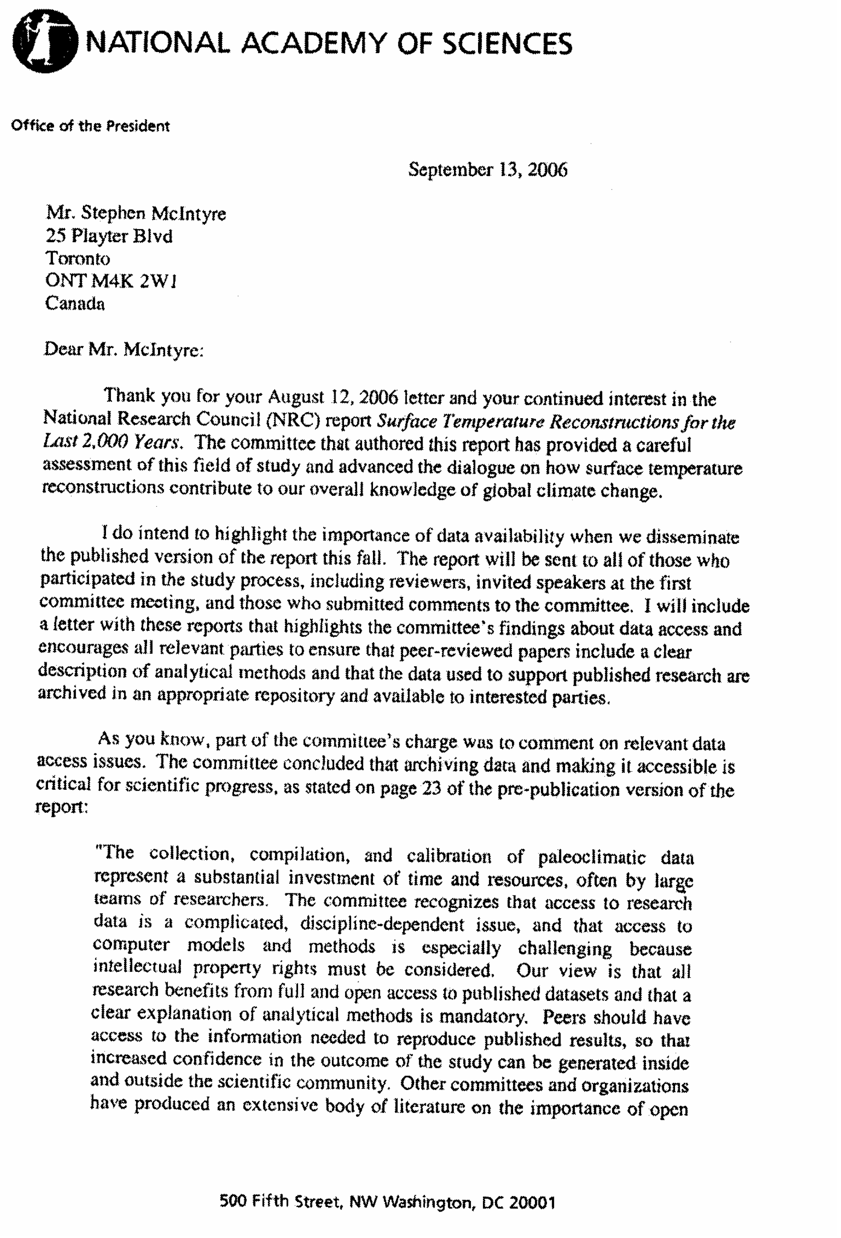
c) A statistical reference for the method used to calculate confidence intervals in MBH99, together with source code for this step;

d) A description of methodology used in MBH98 to calculate the number of principal components retained in each tree ring network step, sufficient to yield the retention of two PCs for the Vaganov AD1600 network and 9 PCs for the Stahle/SWM AD1700 network. Source code would be desirable.

e) Source code sufficient to demonstrate the retention of tree ring sites according to the criteria set out in MBH Corrigendum of 2004.

APPENDIX J

REPLY BY RALPH CICERONE, SEPT 2006



NATIONAL ACADEMY OF SCIENCES

Office of the President

September 13, 2006

Mr. Stephen McIntyre
25 Playter Blvd
Toronto
ONT M4K 2W1
Canada

Dear Mr. McIntyre:

Thank you for your August 12, 2006 letter and your continued interest in the National Research Council (NRC) report *Surface Temperature Reconstructions for the Last 2,000 Years*. The committee that authored this report has provided a careful assessment of this field of study and advanced the dialogue on how surface temperature reconstructions contribute to our overall knowledge of global climate change.

I do intend to highlight the importance of data availability when we disseminate the published version of the report this fall. The report will be sent to all of those who participated in the study process, including reviewers, invited speakers at the first committee meeting, and those who submitted comments to the committee. I will include a letter with these reports that highlights the committee's findings about data access and encourages all relevant parties to ensure that peer-reviewed papers include a clear description of analytical methods and that the data used to support published research are archived in an appropriate repository and available to interested parties.

As you know, part of the committee's charge was to comment on relevant data access issues. The committee concluded that archiving data and making it accessible is critical for scientific progress, as stated on page 23 of the pre-publication version of the report:

> "The collection, compilation, and calibration of paleoclimatic data represent a substantial investment of time and resources, often by large teams of researchers. The committee recognizes that access to research data is a complicated, discipline-dependent issue, and that access to computer models and methods is especially challenging because intellectual property rights must be considered. Our view is that all research benefits from full and open access to published datasets and that a clear explanation of analytical methods is mandatory. Peers should have access to the information needed to reproduce published results, so that increased confidence in the outcome of the study can be generated inside and outside the scientific community. Other committees and organizations have produced an extensive body of literature on the importance of open

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> access to scientific data and on the related guidelines for data archiving and data access (e.g., NRC 1995). Paleoclimate research would benefit if individual researchers, professional societies, journal editors, and funding agencies continued to improve their efforts to ensure that these existing open access practices are followed."

The issue of access to scientific data is of ongoing interest to the National Academies, which includes the National Academy of Sciences, National Academy of Engineering, Institute of Medicine, and the National Research Council. A number of previous NRC reports have addressed data access issues across a variety of disciplines. In the past five years alone the National Academies have published the following reports on this topic: *Expanding Access to Research Data: Reconciling Risks and Opportunities* (2005), *Open Access and the Public Domain in Digital Data and Information for Science: Proceedings of an International Symposium* (2004), *The Role of Scientific and Technical Data and Information in the Public Domain: Proceedings of a Symposium* (2003), and *Access to Research Data in the 21st Century: An Ongoing Dialogue Among Interested Parties* (2002).

As the STR committee stated, data access is now the responsibility of individual researchers, professional societies, journal editors, and funding agencies and practices vary across disciplines. The NRC does not have authority over all of these entities, so I cannot command compliance to requests such as yours. As I mentioned in my testimony before the Subcommittee on Oversight and Investigations of the House Committee on Energy and Commerce this past July, we are currently preparing to launch a comprehensive study that will address data access and data integrity issues across a broad range of academic disciplines.

Sincerely,

Ralph J. Cicerone

Ralph J. Cicerone
President, National Academy of Sciences
Chair, National Research Council

RESPONSE FOR THE RECORD OF DR. EDWARD J. WEGMAN, DIRECTOR, CENTER FOR COMPUTATIONAL STATISTICS

Response of Dr. Edward Wegman to Questions Posed by the Honorable Mr. Bart Stupak in Connection with Testimony to the Subcommittee on Oversight and Investigations

Preamble: In order to set the context for my responses, I would like to make a few observations. I have been a professional statistician for some 38 years. I have served as editor of the *Journal of the American Statistical Association* and served as coordinating editor, associate editor, member of the editorial board and a number of other editorial roles for many journals during this time period. I am currently on the Board of Directors of the American Statistical Association as the publications representative and will become the Chair of their Publications Committee as of 1 January, 2007. I am thoroughly familiar with the benefits as well as the drawbacks associated with peer review. I recognize that scientists are also human beings and share the desire for acceptance and adulation that we all desire.

In addition to these editorial roles, I have served as senior executive at the Office of Naval Research where I was a program manager for the mathematical and computer sciences. In this role, I not only evaluated proposals for research funding, but I also had very significant interdisciplinary interactions with many other discipline areas including oceanography and meteorology. Indeed, I was the initial funding agent for the first two conferences on statistical climatology held respectively in Hachioji, Japan in 1979 and Sintra, Portugal in 1983. The history page on this meeting series, http://cccma.seos.uvic.ca/imsc/history.shtml, can verify that I was on the scientific program committee for the Portuguese meeting. Although some individuals, including individuals writing editorials in the popular press, have attempted to portray me as uninformed and naïve on such matters, I am not. For example, I have known about mixing of gases in the atmosphere since my high school days[1]. But I was asked to testify as a statistician as to the correctness of the Mann-Bradley-Hughes (MBH) methodology and not to offer my beliefs and opinions on anthropogenic global warming (AGW). For this reason, during my oral testimony, I refused to become drawn into the debate about AGW.

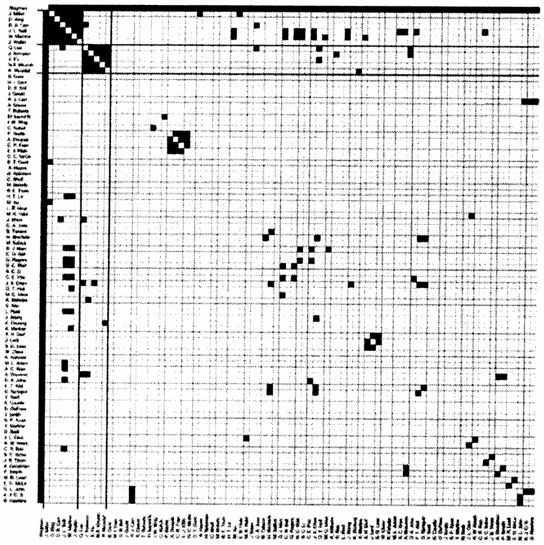
1. **You stated in your testimony that the social networking analysis that you did concerning Dr. Mann and his co-authors represented a "hypothesis" about the relationships of paleoclimatologists. You said that the "tight relationship" among the authors could lead one to "suspect that the peer review process does not fully vet papers before they are published." Please describe what steps you took that proved or disproved this hypothesis.**

---

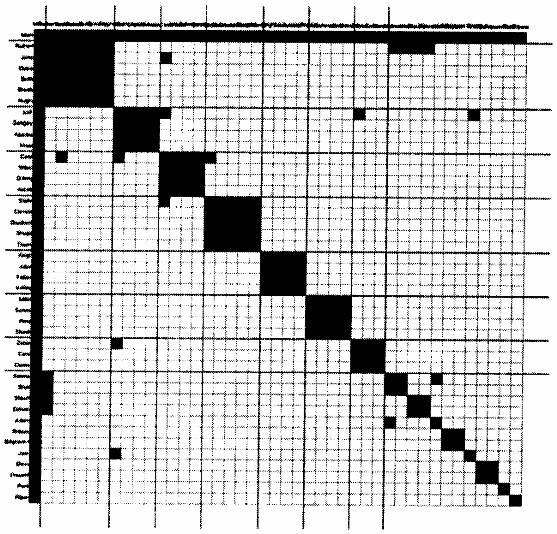
[1] The honorable Mr. Waxman addressed a question to Dr. Mann concerning an offhand remark I made about carbon dioxide being heavier than air. My remark was in response to graphic displayed in the first hearing by the honorable Mr. Inslee showing infrared radiation being reflected by the greenhouse gasses in the upper atmosphere. My response was not intended as a serious piece of testimony nor intended to represent my state of knowledge of atmospheric mixing.

Ans: Social network analysis is a powerful tool with a more than 50-year history of making obvious potentially hidden social relationships. In the case of our analysis, we took a social relationship to be a co-author relationship. This type of relationship does not imply friendship or any other social relationship. Our social network analysis identified the fact that there are several intensively coupled groups within the paleoclimate community. A group of individuals that are completely connected (a technical term in graph theory) meaning that every individual has one or more co-author relationships with every other member of the group is called a *clique* in mathematical graph theory. This is a technical term and is not to be interpreted in the usual English language meaning of a clique. There are a number of cliques in the paleoclimate community, most of which Dr. Mann belongs to. Obviously because peer review is typically anonymous, we cannot prove or disprove the fact that there are reviewers in one clique that are reviewing other members of the same clique. However, the subcommittee did miss the opportunity to ask that question during the testimony, a question I surely would have asked if I were in a position to ask questions. Within my own discipline, it is the case that highly regarded individuals are reviewed with somewhat less scrutiny than lesser known figures. I would like to close this response by noting that I was asked in the pre-hearing phase if such a social network analysis had ever been done for anyone else. This was a good question. I have since undertaken to have a student of mine do a similar social network analysis of my co-author relationships.

I have some 101 co-authors with approximately 200 total publications.



Dr. Wegman's co-author matrix showing very few completely connected 'cliques.'



Dr. Mann's co-author matrix showing strong 'clique' behavior,
i.e. many groups of closely connected co-authors.

My co-author network matrix is shown together with that of Dr. Mann. There is a clear difference in the way we relate to our co-authors. In my case most of my co-authors are younger than me and my role has been as a mentor-scholar, which is reflected by the social network. In Dr. Mann's case, there is exhibited a strong tendency to work with different cliques of closely connected co-authors. This is what one might think of as an entrepreneurial network. The difference is striking. The co-author network of Dr. Mann was developed by my student John T. Rigsby and is the same one as in our report. My co-author network was developed by my student Walid Sharabati. The complete report on my social network of co-authors by Mr. Sharabati is contained in Appendix A.

2. **How did you demonstrate that in a small discipline, such as paleoclimatology, the peer review process "is likely to have turned up very sympathetic referees"?**

Ans: It is precisely in a small specialized discipline that the likelihood of turning up sympathetic referees is highest. Within a small, focused discipline, there simply are fewer referees available. Also, there is always the possibility of the discipline becoming extinct or irrelevant. The referees have a vested interest in seeing that research is published, especially if there is a strong consensus. It has been my experience both in journals as well as with the awarding of grants that staying close to the consensus opinion is most likely to result in funding or publications because the reviewers like to see work that is similar to their own and work that reinforces their position. Peer review, while often taken to be a gold standard, is in fact very conservative and radical new ideas are much less likely to be funded or published. Again, because peer review is typically anonymous, I cannot "prove" that there are sympathetic reviewers, but I maintain that my 38 years of experience in scientific publication gives me exceptionally strong intuition and insight into the behaviors of authors and reviewers.

3. **Is it your position that every published scientific article that is subsequently determined to have an error in methodology or statistics, such as in the case of Dr. Mann and Dr. Christy, is a result of a failure of the peer review process?**

   Ans: Science is a human endeavor and there will always be errors. The peer review process is an attempt to keep errors to a minimum and uphold the integrity of the scientific literature. Yes, I believe when an error escapes the notice of the peer reviewers, it is a failure of the process. Indeed, the process is prone to failure with the increasing number of outlets for research as well as the limited supply of editors and reviewers. Ultimately, however, it is the responsibility of the authors to acknowledge and correct errors in a timely fashion rather than to argue that an error doesn't make any difference because the answer is correct.

4. **In its recently published report entitled "Network Science," the National Research Council stated that "there was a huge gap between what we need to know about networks to ensure the smooth working of society and the primitive state of our fundamental knowledge." The Army commissioned the study to determine if it should fund a "new field of investigation" called network science. Do you disagree with the conclusions of the Council?**

   Ans: I do agree with the National Research Council report. Indeed, it was I who brought this report to the attention of the Subcommittee. But let me be clear. The NRC report focuses on networks in a very broad sense, not only social networks such as we used, but also networks of neurons in our brains, communication networks, computer networks and the like. The command and control network, especially in connection with multi-national forces is of crucial importance to the military as is the understanding of political, religious, and terrorist networks in pacification efforts. The synthesis and abstraction of the common elements of these networks is the goal of *network science*. The blending of the intuitive aspects of networks with the mathematics of graph theory and statistical methodology is the goal of network science. This in no way discounts the value of what has been learned by computationally-oriented social scientists in the development of social networks over the last 30 years. Indeed, the NSA uses social network analysis intensively to exploit signal intelligence in the form of developing views of terrorist networks. Similarly, the analysis of the Enron email traffic using social network analysis uncovered unanticipated figures in that scandal.

5. **You testified that "there is no evidence that Dr. Mann or any of the other authors in the paleoclimate studies had significant interaction with mainstream statisticians." With respect to this statement please answer the following questions:**

    a. **Is this based solely or primarily on the social network analysis described in your report? If it is based on something other that the social network analysis, please describe the basis for this statement.**

    Ans: No, my observation was not based on the social network analysis to any significant degree. We examined the list of references in the paleoclimate papers that we considered to find any evidence that these papers were using contemporary statistical tools, that they were citing the current statistics literature, and that they had basic knowledge of the statistics literature. We examined resumes of the most frequently published authors to understand where and with whom they obtained their statistical training. We examined the composition of the Probability and Statistics Committee[2] of the American Meteorological Society searching for "mainstream statisticians." We examined the scientific programs for the AMS's Conferences on Probability and Statistics in the Atmospheric Sciences[3] for "mainstream statisticians." In every case, while there are a few examples of cooperation, they are the exception. The atmospheric science community, while heavily using statistical methods, is remarkably disconnected from the mainstream community of statisticians in a way, for example, that is not true of the medical and pharmaceutical communities.

    b. **Please explain what you mean by "significant interaction." Does this mean more than coauthoring papers?**

    Ans: Yes, we mean much more than co-authoring papers. We mean the early engagement of statisticians in designing experiments, in developing new statistical methods appropriate for exploiting the data[4], of careful use of probabilistic inference such as confidence limits, and, of course, in the final data analysis. Statisticians, like computer scientists and mathematicians, have the experience of applicability to many fields and, hence, can bring to the table interdisciplinary experiences that many disciplines cannot.

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[2] As I testified earlier, the Probability and Statistics Committee of the American Meteorological Society contained nine members, only two of which belonged to the American Statistical Association one of those being a recent graduate with an assistant professor appointment in a medical school.

[3] At the time of the January, 2006 conference, the 18th Conference on Probability and Statistics in the Atmospheric Sciences, eight of 62 presenters are members of the American Statistical Association and only two hold Ph.D.'s in statistics. Three were graduate students respectively majoring in systems engineering, atmospheric sciences, and statistics. One was a Ph.D. in applied mathematics and two were of unknown background.

[4] For example, most statisticians would not believe that a principal component-like analysis such as CFR was the correct way to analyze the proxy data. It is the case of having a tool and applying it whether or not it is the appropriate tool. Statisticians are constantly inventing new methods appropriate to new datasets.

c. Was this statement based simply on a review of the coauthors of Dr. Mann's paleoclimate articles and the fact that none of the coauthors were members of the American Statistical Association? If not please describe the evidence you used to determine the interactions between paleoclimatologists and "mainstream" statisticians.

Ans: First see our answer to 5.a above. The fact that a co-author is not a member of the American Statistical Association is irrelevant to the cooperation with mainstream statisticians. I am a member of the American Meteorological Society and the American Geophysical Union. This does not make me a mainstream atmospheric scientist or a mainstream geophysicist. It is an indication that I am perhaps a little less naïve in these areas than many statisticians. Similarly, membership in the American Statistical Association is an indicator of interest in statistics, but does not confirm one as a mainstream statistician. As evidence of interactions or lack thereof, we offer the fact that the methodologies used in paleoclimate studies are routine application of existing ideas and that many fundamental statistical issues are overlooked such as issues of randomization of tree samples to assure valid statistical sampling, which, in turn, allows correct inferences on confidence bands.

d. Please provide your definition of "mainstream statistician." Must a statistician belong to the American Statistical Association to be a "mainstream statistician" as you define the term?

Ans: Although there will be exceptions to the guidelines I am proposing here, in my view, a mainstream statistician will have graduated with a doctorate in statistics, will be actively doing research in statistical methodology or actively applying statistical methodology to a related discipline area, and will professionally identify with the statistics profession. There are other statistical societies besides the American Statistical Association such as the Institute of Mathematical Statistics, the (British) Royal Statistical Society, and the International Statistical Institute. Affiliation with one or more of these is normally characteristic of a mainstream statistician. On the other hand, many members of these societies are "interested parties" who are not mainstream statisticians. Also, many members are master's level practitioners who are not contributing to the knowledge base of statistics and who I would not include as mainstream statisticians. Dr. Douglas Nychka at NCAR, Professor Peter Bloomfield at North Carolina State University, and Professor Grace Wahba at the University of Wisconsin, Madison are in my view mainstream statisticians with a demonstrated interest and collaboration in the atmospheric sciences.

e. **Please list all the authors in paleoclimate studies with whom you or your coauthors spoke regarding the paleoclimatologists' training in statistics and their consultations with statisticians (including statisticians who are not members of the American Statistical Association). For each author listed, please summarize the information he or she provided about statistics or statisticians and the paleoclimatology research committee.**

Ans: I spoke with no one in paleoclimate studies. To the best of my knowledge neither have my colleagues. My home university, George Mason University, does have a Ph.D. program in climate dynamics. There are no requirements to take any statistics courses even though their principal interest is in climate modeling. According to the website of the Department of Meteorology at Pennsylvania State University (Michael Mann's institution), there is no requirement to take a statistics course for the Ph.D. in meteorology (except possibly internal courses taught by meteorology faculty). The Department of Environmental Sciences at the University of Virginia (Dr. Mann's previous institution) also does not explicitly specify any statistics courses. The graduate meteorology program at Iowa State University (with one of the strongest statistics departments in the nation) has no statistics requirements except a freshman-level statistics introductory course as a prerequisite to one of their graduate-level courses. The Department of Geology and Geophysics at Yale University where Dr. Mann obtained his Ph.D. also specifies no statistics courses. Indeed, they specify no courses at all except with the concurrence of an advisor[5].

6. **You testified that other scientists or statisticians reviewed your report before it was sent to the Committee on Energy and Commerce, but it was unclear whether you provided a complete list. Please list the people who reviewed your report before it was sent to Committee, including name, title, area of expertise, and university or other affiliation.**

- Professor (emeritus) Enders Robinson, geophysics, Columbia University, elected member of the National Academy of Engineering

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[5] Indeed, we observe in the acknowledgement section of Dr. Mann's dissertation that he credits Professor Jeffrey Park for Dr. Mann's statistical training. Dr. Mann states, "… when my knowledge and skills in time series analysis and statistics were few. He taught me the tools of statistical data analysis." Professor Park is a seismologist, not a statistician. Dr. Park received his doctorate in Earth Sciences from the University of California, San Diego.

- Professor Grace Wahba, statistics, University of Wisconsin, Madison, elected member of the National Academy of Science
- Professor Noel Cressie, spatial statistics, Ohio State University
- Professor David Banks, statistics, Duke University, Editor of Applications Section, *Journal of the American Statistical Association*
- Professor William Wieczorek, geophysics, Buffalo State SUNY
- Dr. Amy Braverman, Senior Scientist, remote sensing, data mining, Jet Propulsion Laboratory (CalTech)
- Dr. Fritz Scheuren, statistics, NORC, University of Chicago, the 100th president of the American Statistical Association
- In addition, we had two other reviewers who asked that their names not be revealed because of potential negative consequences for them.

7. **Prior to sending your report to the Committee on Energy and Commerce, was your report peer reviewed, i.e. did someone other than the authors select the reviewers, were reviewers allowed to submit comments anonymously, was someone other the authors involved in deciding whether the authors' responses were adequate?**

   Ans: Our report was not peer reviewed in the sense you ask. The review process we went through was similar to that employed by the National Research Council. At the NRC, the Committee makes recommendations to the Committee Chair and the Study Director. The list is narrowed and a recommendation is made by the Study Director. This list is approved by a higher-level authority and the document is sent out for review. The reviewers are not anonymous and their names are listed in the document. This was true of the recent North Study on Paleoclimate Reconstruction that was also the subject of our first round of testimony. Because we did not have the NRC structure, we obviously did not have a higher-level review of our list, but to the best of our ability, we acted in good faith to obtain reviews, some of which expressed dissenting opinions. Subsequently, we have been preparing papers that will be peer reviewed for the Applications Section of the *Journal of the American Statistical Association*, another for the journal called *Statistical Science*[6] published by the Institute of Mathematical Statistics, and finally for a more popular outlet called *Chance*. In addition, we are preparing a paper motivated by our social network studies on the styles of co-authorship.

8. **You testified that "the fact is that the peer review process failed in the 1998 paper." Which peer review process were you referring to? Were you referring to the peer review process conducted by the journal that published the 1998 paper?**

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[6] The *Statistical Science* article will have even more rigorous scrutiny than a normal peer review. It will be a discussion paper meaning that discussants will have an opportunity to comment in writing for the audience to see.

Ans: Yes, I was referring to the peer review process at *Nature*, which published the 1998 paper.

9. **Your analysis seems to show that, at least in some instances, when you use the same methodology and the same data, a graph of the results will look like a hockey stick when the data is decentered, but not when the data is properly centered.**

    a. **Is that a correct statement?**

    Ans: Yes. We explicitly looked at the first principal component of the North American Tree Ring series and demonstrated that the hockey stick shows up when the data are decentered, but not when properly centered. We also demonstrated the same effect with the digitized version of the 1990 IPCC curve.

    b. **Does your analysis prove that every time you use improperly centered data and the climate field reconstruction methodology (CFR) and get a hockey stick, the hockey stick will disappear when the data is properly centered? Or does the shape of the graph with properly centered data depend on the data?**

    Ans: The shape of the graph will depend on the underlying data. To reiterate our testimony, the decentering process as used in MBH98 and MBH99 selectively prefers to emphasize the hockey stick shape. This is because the decentering increases the apparent variance of hockey sticks and principal component methods attempt to find components with the largest explainable variance. If the variance is artificially increased by decentering, then the principal component methods will "data mine" for those shapes. In other words, the hockey stick shape must be in the data to start with or the CFR methodology would not pick it up. What we have shown both analytically and graphically in Figure 4.6 is that using the CFR methodology, just one signal when decentered will overwhelm 69 independent noise series. The point is that if all 70 proxies contained the same temperature signal, then it wouldn't matter which method one used. But this is very far from the case. Most proxies do not contain the hockey-stick signal. The MBH98 methodology puts undue emphasis on those proxies that do exhibit the hockey-stick shape and this is the fundamental flaw. Indeed, it is not clear that the hockey-stick shape is even a temperature signal because all the confounding variables have not been removed.

    c. **Does your report prove that "the hockey stick disappears" from MBH98 and MBH99 if one were to fix the decentering? In other words, does your paper prove that "the hockey stick disappears" if the data is properly centered but the rest of the MBH98 and MBH99**

analysis were kept the same (i.e., it relied on the CFR methodology and all the proxy data used by Dr. Mann in MBH98 and MBH99)? If you believe that it does, what level of certainty do you give to this conclusion?

Ans: Our report does not prove that the hockey stick disappears. Our work demonstrates that the methodology is incorrect. Because of the lack of proper statistical sampling and correct inferential methodology, we concluded that the statements regarding the decade of the 1990s probably being the hottest in a millennium and 1998 probably being the hottest year in a millennium are unwarranted. Indeed, I repeatedly testified that the instrumented temperature record from 1850 onwards indicated that there is a pattern of global warming. We have never disputed this. We also believe that there is no dispute between our report and the North report in this regard. Professor North in testimony agreed with our conclusions regarding the incorrectness of the methodology. We in turn agree with the fundamental conclusion of the North report, i.e. that the present era is likely the hottest in the last 400 years. We remain silent on the issues related to anthropogenic global warming.

d. **Does your report include a recalculation of the MBH98 and MBH99 results using the CFR methodology and all the proxies used in MBH98 and MBH99, but properly centering the data? If not, why doesn't it?**

Ans: Our report does not include the recalculation of MBH98 and MBH99. We were not asked nor were we funded to do this. We did not need to do a recalculation to observe that the basic CFR methodology was flawed. We demonstrated this mathematically in Appendix A of the Wegman et al. Report. The duplication of several years of funded research of several paleoclimate scientists by several statisticians doing pro bono work for Congress is not a reasonable task to ask of us. We all have additional responsibilities to the people and agencies that pay our salaries.

10. **In the footnote of your report, you reference papers by Wahl and Ammann (2006) and Wahl et al. (2006) and note that they "are not to the point." I understand that Wahl and Ammann actually examined, among other things, the problem of data decentering, the main focus of your report, and corrected the emulation of MBH98 by recentering the data.**

a. **Did you analyze this work by Wahl and Ammann prior to sending your final report to the Committee on Energy and Commerce? If so, why does your report not alert the reader that these researchers had conducted a reanalysis of the MBH98 that corrected the only statistical methodology error discussed in the "Finding" section of**

your report and that these researchers found that recentering the data did not significantly affect the results reported in the MBH98 paper?

Ans: The Wahl and Ammann paper came to our attention relatively late in our deliberations, but was considered by us. Some immediate thoughts we had on Wahl and Ammann was that Dr. Mann lists himself as a Ph.D. co-advisor to Dr. Ammann on his resume. As I testified in the second hearing, the work of Dr. Ammann can hardly be thought to be an unbiased independent report. It would have been more convincing had this paper been written by a totally independent authority, but alas this is not the case. The Wahl and Ammann paper is largely an attempt to refute the criticisms of McIntyre and McKitrick (MM). The comment we made in our footnote about being 'not to the point" refers to the fact that MM03 and MM05 were not attempting to portray themselves as doing a paleoclimate reconstruction, they not being paleoclimatologists themselves, but were merely pointing out the flaws in the MBH98 and MBH99 papers. There are several comments of interest in the Wahl and Ammann paper. They suggest three areas in which the MBH papers have been subject to scrutiny.

> "First, the MBH reconstruction has been examined in light of its agreement/lack of agreement with other long-term annual and combined high/low frequency reconstructions." Wahl and Ammann (2006, p.3 in the 24 February 2006 draft)

Their conclusion is:

> "The comparison of the MBH reconstruction, derived from multi-proxy (particularly tree ring) data sources, with widespread bore-hole-based reconstructions … is still at issue in the literature." Wahl and Ammann (2006, p.4 in the 24 February 2006 draft)

In other words, the MBH reconstruction does not agree with other widely accepted methodologies for climate reconstruction. Bore hole methods measure a temperature gradient and calculate the diffusion of heat within the bore hole. This method does not have nearly the confounding variables as do tree ring proxies. The second area of scrutiny involves comparison with results from modeling efforts.

> "Second a related area of scrutiny of the MBH reconstruction technique arises from an atmosphere-ocean general circulation model (AOGCM) study …, which also examines the potential loss of amplitude [in the MWP] in the MBH method (and other proxy/instrumental reconstructions that calibrate by using least squares projections of the proxy vectors onto a single- or multi-dimensional surface determined by either the instrumental data or its [their] eigenvectors." Wahl and Ammann (2006, p.4 in the 24 February 2006 draft)

Again the MBH reconstructions do not correlate well with the model-based methods. Wahl and Amman (2006) offer the following explanation.

> "However, a number of issues specific to the modeling situation could arise in this context, including: how realistically the AOGCM is able to reproduce the real world patterns of variability and how they respond to various forcings[7]; the magnitude of forcings and the sensitivity of the model that determine the magnitude of temperature fluctuations …; and the extent to which the model was sampled with the same richness of information that is contained in the proxy records (not only temperature records, but series that correlate well with the primary patterns of variability – including, for example, precipitation in particular seasons." Wahl and Ammann, (2006, p.5 in the 24 February 2006 draft)

This quotation has two interesting facets. First, it seems to call into question the very models that are predicting temperature increases based on CO2 forcings. If these models do not coincide with the MBH reconstructions, then which are we to believe? Second, the quotation implicitly admits what we have observed previously, namely that there are other covariates such as precipitation, which are not teased out in the temperature reconstructions. Thus, what are purported to be temperature reconstructions are contaminated with covariates that reflect temperature indirectly at best and not at all at worst. The third area of scrutiny involves the challenges made by MM.

> "A third area of scrutiny has focused on the nature of the proxy data set utilized by MBH, along with the pre-processing algorithms used to enhance the climate signal-to-noise characteristics of the proxy data." Wahl and Ammann, (2006, p.5 in the 24 February 2006 draft)

We submit that both the mathematical analysis in Appendix A of our report to Congress together with our simulation demonstrate that the decentering method yields incorrect results. The critical issue then becomes the proxies themselves, which MM have challenged. A telling comment from Wahl and Ammann is the following.

> "A further aspect of this critique is that the single-bladed hockey stick shape in proxy PC summaries for North America is carried disproportionately by a relative small subset (15) of proxy records derived from bristlecone/foxtail pines in the western United States, which the authors [MM] mention as being subject to question in the literature as local/regional temperature proxies after approximately 1850 …. It is important to note in this context that because they employ an eigenvector-based CFR technique, MBH do not claim that all proxies used in their reconstruction are closely related to local-site variations in surface temperature." Wahl and Ammann, (2006, p.9 in the 24 February 2006 draft).

This together with the AOGCM quotation reinforces the notion that MBH are attempting to reconstruct temperature histories based on proxy data that are extremely problematic in terms of actually capturing temperature information directly. As we testified, it would seem that there is some substantial likelihood that the bristlecone/foxtail pines are CO2 fertilized and hence are reflecting not temperature at all but CO2 concentration. It is a circular argument to say increased CO2 concentrations are causing temperature increases when temperature increases are estimated by using proxies that are directly affected by increased CO2 concentrations.

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[7] Including presumably forcings from greenhouse gasses such as CO2.

It is our understanding that when using the same proxies as and the same methodology as MM, Wahl and Ammann essentially reproduce the MM curves. Thus, far from disproving the MM work, they reinforce the MM work. The debate then is over the proxies and the exact algorithms as it always has been.

The fact that Wahl and Ammann (2006) admit that the results of the MBH methodology does not coincide with the results of other methods such as borehole methods and atmospheric-ocean general circulation models and that Wahl and Ammann adjust the MBH methodology to include the PC4 bristlecone/foxtail pine effects are significant reasons we believe that the Wahl and Amman paper does not convincingly demonstrate the validity of the MBH methodology.

b. Do you agree or disagree with Wahl and Ammann's finding that the time period used to center the data does not significantly affect the results reported in the MBH98 paper? If you disagree, please state the basis for your disagreement.

Ans: We do disagree. The fundamental issue focuses on the North American Tree Ring proxy series, which Wahl and Ammann admit are problematic in carrying temperature data. In the original MBH decentered series, the hockey-stick shape emerged in the PC1 series because of reasons we have articulated in both our report and our testimony. In the original MBH papers, it was argued that this PC1 proxy was sufficient. We note the following from Wahl and Ammann.

> "Thus, the number of PCs required to summarize the underlying proxy data changes depending on the approach chosen. Here we verify the impact of the choice of different numbers of PCs that are included in the climate reconstruction procedure. Systematic examination of the Gaspé-restricted reconstructions using 2-5 proxy PCs derived from MM-centered, but unstandardized data demonstrates changes in reconstruction as more PCs are added, indicating a significant change in information provided by the PC series. When two or three PCs are used, the resulting reconstructions (represented by scenario 5d, the pink (1400-1449) and green (1450-1499) curve in Fig. 3) are highly similar (supplemental information). As reported below, **these reconstructions are functionally equivalent to reconstructions in which the bristlecone/foxtail pine records are directly excluded** [emphasis added] (cf. pink/blue curve for scenarios 6a/b in Fig. 4). When four or five PCs are used, the resulting reconstructions (represented by scenario 5c, within the thick blue range in Fig. 3) are virtually indistinguishable (supplemental information) and are very similar to scenario 5b." Wahl and Ammann, (2006, p.31, 24 February 2006 draft)

Without attempting to describe the technical detail, the bottom line is that, in the MBH original, the hockey stick emerged in PC1 from the bristlecone/foxtail pines. If one centers the data properly the hockey stick does not emerge until PC4. Thus, a substantial change in strategy is required in the MBH reconstruction in order to achieve the hockey stick, a strategy which was specifically eschewed in MBH. In Wahl and Ammann's own words, the centering does significantly affect the results.

c. **Dr. Gulledge included in his testimony a slide showing the graph of W A emulation of the MBH and MBH-corrected for decentering and the Gaspe tree-ring series. Were you aware of their reanalysis of MBH99 prior to the time you finalized your report? Do you agree or disagree with their reanalysis of MBH99? If you disagree, please state the basis for your disagreement.**

Ans: Yes, we were aware of the Wahl and Ammann simulation. We continue to disagree with the reanalysis for several reasons. Even granting the unbiasedness of the Wahl and Ammann study in favor of his advisor's methodology and the fact that it is not a published refereed paper, the reconstructions mentioned by Dr. Gulledge, and illustrated in his testimony, fail to account for the effects of the bristlecone/foxtail pines. Wahl and Ammann reject this criticism of MM based on the fact that if one adds enough principal components back into the proxy, one obtains the hockey stick shape again. This is precisely the point of contention. It is a point we made in our testimony and that Wahl and Ammann make as well. A cardinal rule of statistical inference is that the method of analysis must be decided before looking at the data. The rules and strategy of analysis cannot be changed in order to obtain the desired result. Such a strategy carries no statistical integrity and cannot be used as a basis for drawing sound inferential conclusions.

11. **Please answer the following questions with respect to Figure 4.3 in your report:**

a. **Is the data centering the only difference between the two panels in that figure?**

Ans: Yes, the centering is the only difference.

b. **Were the same R commands used to carry out the PC analysis for the upper and lower panels?**

Ans: Yes, the same R commands were used except that a parameter indicating centering or not was adjusted.

c. **Was the upper frame processed based on a correlation matrix? Was the lower frame processed based on a covariance matrix? If the answer to both questions is yes, does this not have the effect of comparing standardized with non-standardized data?**

Ans: The correct method for a principal component analysis (that we executed) is to use the covariance matrix and not the correlation matrix. The correlation is a scaled version of the covariance divided by the product of the standard deviations of the individual variables. You are really asking a different processing question when you ask about standardized versus non-standardized data. Because the scale of different proxy series is different, as indicated above PCA will preferentially emphasize series with larger variance as the first principal component. Thus, it is important to ensure that the proxy data all have the same scale. This is a tricky adjustment from a statistical perspective. Simply dividing by the standard deviation is a non-robust procedure if the data have outliers. This appears to be the case with many of the proxy data sets. Thus, a robust estimator of scale must be used. One can observe that we did do the scale adjustment based on the fact that the scales of the Y axes are approximately the same. The underlying assumption is that the time series of proxy data are heteroscedastic, which is a fancy statistical term meaning that they have the same variance (scale) through time. This is also a problematic assumption for serious data analyses, although it is the approximation made by MBH and by us to generate Figure 4.3.

d. Is it appropriate to compare data sets that have more than a tenfold difference in standard deviation among them? Would not such a comparison preferentially select data sets with larger variance, regardless of the climate signal contained in the data sets with the smaller variance?

Ans: No, it is not appropriate and we did not do this. Yes indeed, the PCA would over-represent proxies with a larger variance to the detriment of climate signals in sets with smaller variance. Again, we did not do this.

e. What does PC1 look like if the data are centered correctly and processed based on a correlation matrix instead of a covariance matrix?

Ans: If the scale is adjusted by the standard deviation after first being centered, then the result is a standardized random vector. In this case, the covariance matrix would be identical with the correlation matrix and would look like the bottom panel of our Figure 4.3.

APPENDIX A: Wegman Social Network Analysis

Edward J. Wegman's Author–Coauthor Social Network

Walid Sharabati

Introduction

Social Networks are becoming an important tool in analyzing the behavior of groups of people on the global level (how one or more group interacts with other group(s)) and on the local level (how individuals interact with each other within the same network.) In this report, we will study in depth the author-coauthorship social network of Edward J. Wegman – a prominent professor of Statistics at George Mason University, Fairfax VA, USA.

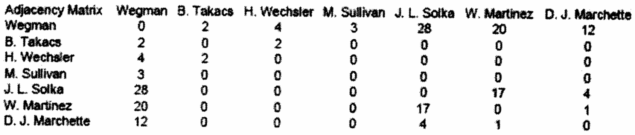
We would like to provide you some background on Dr. Wegman. He received his Ph.D. degree in Mathematical Statistics from University of Iowa in 1968. Immediately after his Ph.D., he went to the statistics department at the University of North Carolina, Chapel Hill, which was one of the leading statistics departments in the world. His early career focused on the development of aspects of the theory of mathematical statistics. In 1978, he went to the Office of Naval Research (ONR) where he was the Head of the Mathematical Sciences Division. He has been in the research and academia field for some time and has published an array of work, which includes over 200 prestigious refereed journals, books, and technical reports, authored individually and with a number of colleagues and Ph.D. students.

In this study, we are going to look at the structure of the network and model its behavior. The objective of this paper is to perform a comprehensive social network analysis on the first-level of Wegman's coauthorship network.

Social Networks can be treated as directed graphs in which actors (individuals) are represented by vertices (nodes) while interactions between actors are represented by edges (ties), which may have weights. There are three basic representations of a graph – the planar graph visualization, the adjacency matrix, and the sparse-graph representation. The tools and methods of analyzing graphs will be covered in detail in Section 3.

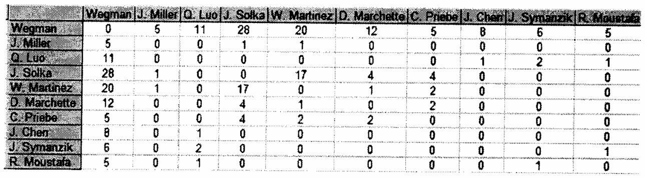
1 Literature Review

It is worth mentioning that no one has yet analyzed Wegman's author-coauthorship social network on any level. This work will provide an independent source to view the network on the different levels and examine how the actors interact with each other. It is the expectation that the methodology and techniques applied will unveil important properties of the network.



Adjacency Matrix	Wegman	B. Takacs	H. Wechsler	M. Sullivan	J. L. Solka	W. Martinez	D. J. Marchette
Wegman	0	2	4	3	28	20	12
B. Takacs	2	0	2	0	0	0	0
H. Wechsler	4	2	0	0	0	0	0
M. Sullivan	3	0	0	0	0	0	0
J. L. Solka	28	0	0	0	0	17	4
W. Martinez	20	0	0	0	17	0	1
D. J. Marchette	12	0	0	0	4	1	0

Figure 1: Snapshot of the proximity adjacency matrix.



	Wegman	J. Miller	Q. Luo	J. Solka	W. Martinez	D. Marchette	C. Priebe	J. Chen	J. Symanzik	R. Moustafa
Wegman	0	5	11	28	20	12	5	8	6	5
J. Miller	5	0	0	1	1	0	0	0	0	0
Q. Luo	11	0	0	0	0	0	0	1	2	1
J. Solka	28	1	0	0	17	4	4	0	0	0
W. Martinez	20	1	0	17	0	1	2	0	0	0
D. Marchette	12	0	0	4	1	0	2	0	0	0
C. Priebe	5	0	0	4	2	2	0	0	0	0
J. Chen	8	0	1	0	0	0	0	0	0	0
J. Symanzik	6	0	2	0	0	0	0	0	0	1
R. Moustafa	5	0	1	0	0	0	0	0	1	0

Figure 2: Snapshot of the proximity adjacency matrix of the coauthors with frequency ≥ 5.

Of all the work that has been done on social networks, very few investigators have considered coauthorship network. Therefore, what we are about to observe in this paper is a brand new approach in the social networks field.

The main purpose of analyzing coauthorship networks is to be able to answer the question of "who-wrote-with-who" with what frequency.

2 Data

Wegman's raw data was collected directly from his personal website and his updated curriculum vitae. Building the adjacency/proximity matrix manually was very cumbersome because one has to consider not only Wegman and his coauthors and the coauthors who wrote with other coauthors, but also the frequency of communication. It is vital to keep track of the record of every coauthor.

Figure 1 is a snapshot of one block of the proximity adjacency matrix built in *MS-Excel*, the original matrix is of size 102 × 102. Notice that the matrix is symmetric due to the fact that relationship among actors are reflexive; i.e. if person A published with person B then this also implies person B published with person A.

Figure 2 shows a list of all coauthors published with Wegman five times or more.

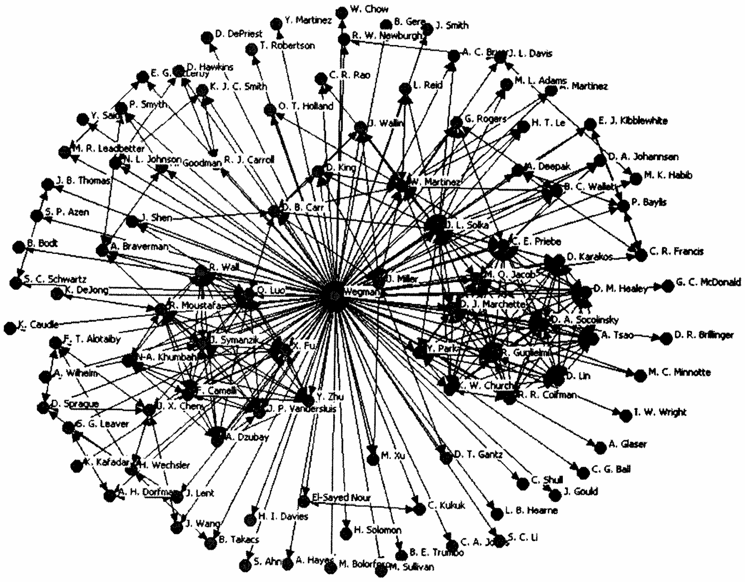


Figure 3: Wegman's author-coauthor social network.

3 Analysis Methods

The methodology adopted in this study consists of both theoretical approach to quantitatively analyze the network, and software approach to simulate and visualize the network. The algorithms and techniques applied to study interactions within the network include centrality measures (node degree and closeness), network partitioning (cliques and clique overlapping), network connectivity (cut-points and bridges), structural equivalence, structural holes, brokerage roles and block-modeling. The software tools used to run and model the network are `UCINET-6` and `Pajek-1.02`.

4 Results and Discussion

We start by exploring Wegman's network with all the actors (coauthors) followed by a detailed analysis pinning down the significant and interesting characteristics of the network. The general structure of the network is based on a weighted digraph consisting of nodes (coauthors) and weighted edges (ties) representing the number of times an actor coauthored

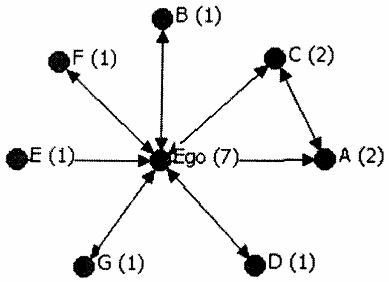


Figure 4: Example of a simple ego-network. Node Ego has degree = 7, nodes A and C have both degree = 2, nodes B, D, E, F and G have all degree = 1.

with Wegman and with other coauthors. Figure 3 shows the basic structure of Wegman's network. The graph suggests that the network is an "ego" network; in which all nodes are connected to one focal node, see Figure 4. In graph theory terms this referred to as a star graph. This star shape is predicted in first-level coauthorship networks since all coauthors are expected to have communicated (ties) with that one main author. The current number of actors coauthored with Wegman is 101. Some coauthors share edges not only with Wegman, but also with other coauthors; more than one name can appear on a publication. One can clearly see the two "clouds" (clump networks) fully connected – complete subgraphs.

Figure 5 shows the matrix representation of Wegman's network, each black square indicates a coauthor-relation. The matrix is generated using `Pajek`.

Figure 6 shows the network in circle layout, the coloring and thickness of both the nodes and edges will be discussed more when we analyze centrality measures and network partitioning. Circle layout is yet another way of displaying the network.

Figure 7 is a random partition of the network with two clusters in grey scale, the darker the color the higher the frequency. Figure 8 is a random partition of the network with three clusters.

4.1 Centrality Measures

In social networks analysis, centrality measures are quantitative tools used for investigating the network; the two centrality metrics that we are applying are node degree and closeness. At the individual level, one dimension of position in the network can be captured through centrality. Conceptually, centrality is straight forward; we want to identify the nodes residing in the "center" of the network.

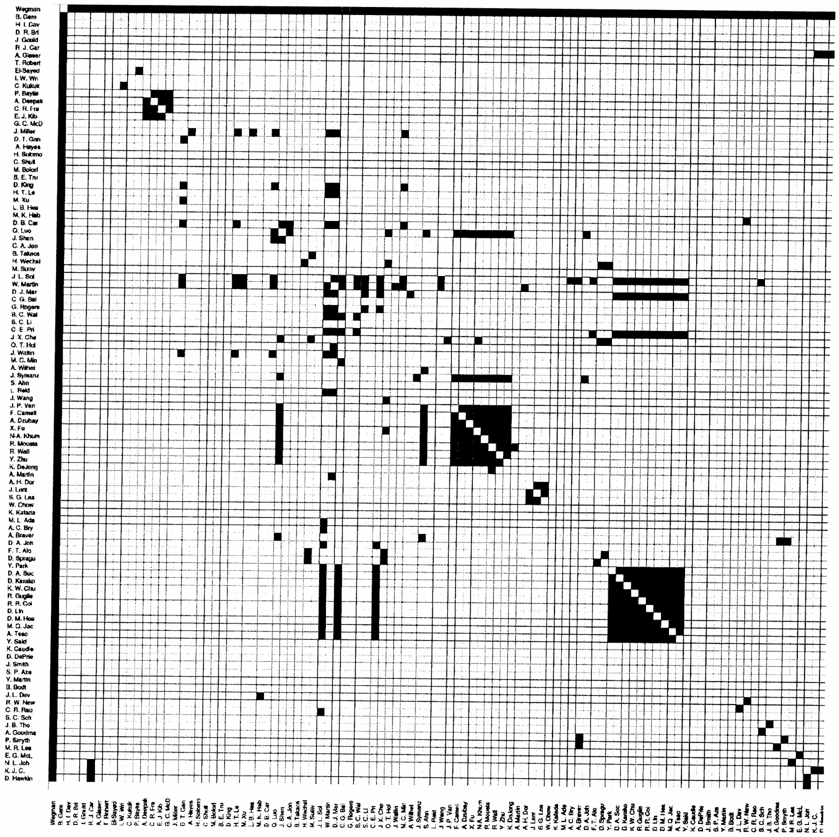


Figure 5: Adjacency matrix of Wegman's network.

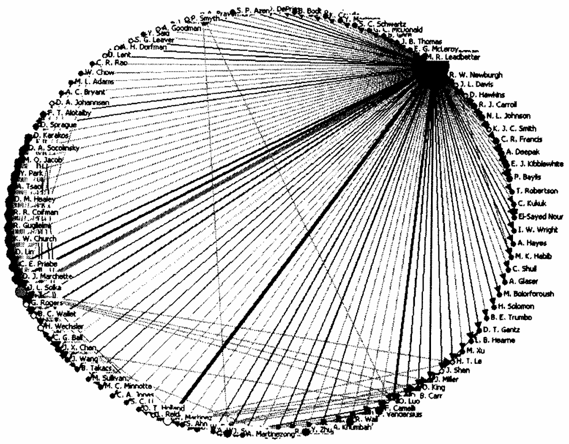


Figure 6: Circle layout of Wegman's network.

Degree of a node is the number of edges that connect it to other nodes; see Figure 4, degree can be interpreted as measure of power or importance of a node, or measure of workload. The actor with most ties is the most important figure in a network. In a simple random graph, degree will have a Poisson distribution [2], and the nodes with high degree are likely to be at the intuitive center. Deviations from a Poisson distribution suggest non-random processes, which is at the heart of current "scale-free" work on networks.

Definition: A graph G, is a collection of nodes N and edges E; $G = \{N, E\}$, where $N = \{n_1, n_2, n_3, \cdots, n_k\}$ and $E = \{e_1, e_2, e_3, \cdots, e_E\}$.

Definition: Node degree; denoted $C_d(n_i)$, is defined by

$$C_d(n_i) = d(n_i) = \sum_j a_{ij}, \quad a_{ij} \in A, \tag{1}$$

where d represents degree measure, and A is the adjacency matrix.

To illustrate the degree measure, we calculate the node degree for the nodes in Figure 4. The table below shows the results.

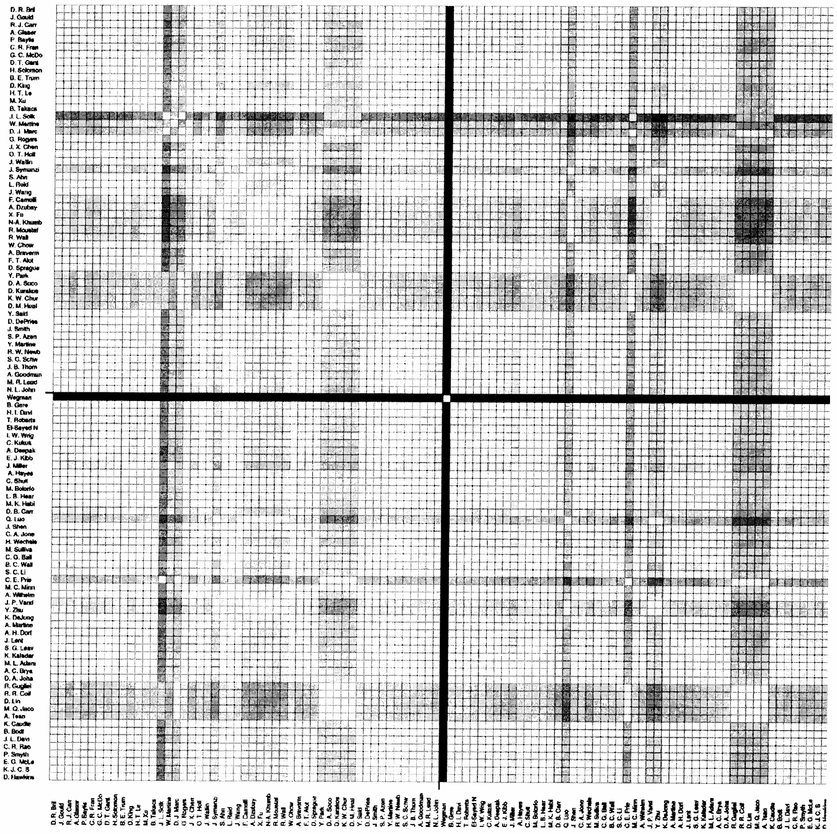


Figure 7: Random partition with two clusters.

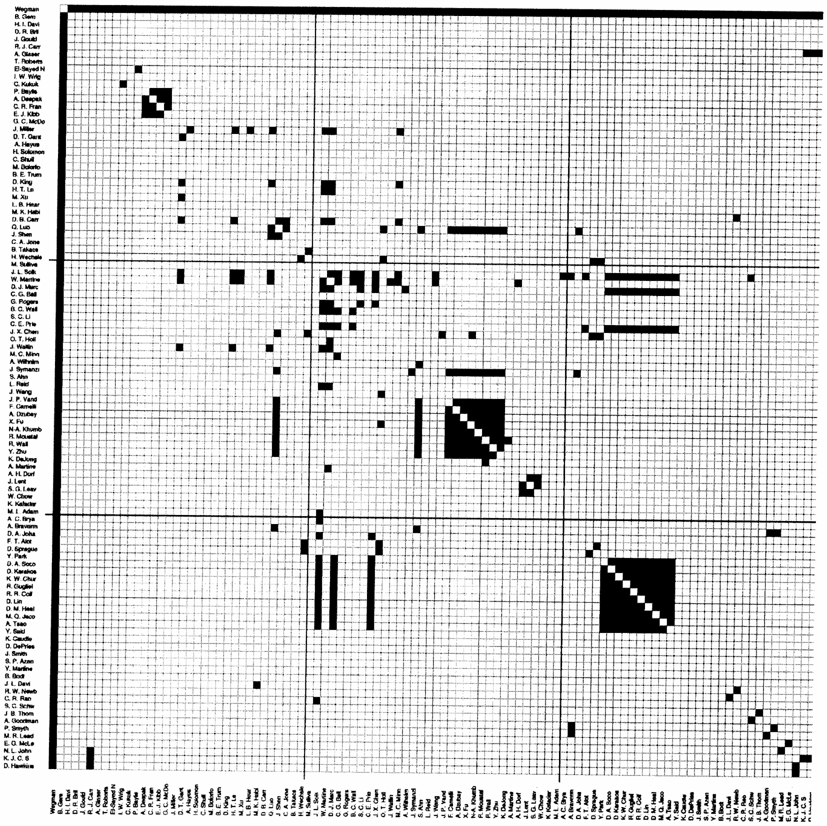


Figure 8: Random partition with three clusters.

Adjacency	Ego	A	B	C	D	E	F	G
Ego	0	1	1	1	1	1	1	1
A	1	0	0	1	0	0	0	0
B	1	0	0	0	0	0	0	0
C	1	1	0	0	0	0	0	0
D	1	0	0	0	0	0	0	0
E	1	0	0	0	0	0	0	0
F	1	0	0	0	0	0	0	0
G	1	0	0	0	0	0	0	0

Degree
7
2
1
2
1
1
1
1

On the other hand, closeness is based on the inverse of the distance of each actor to every other actor in the network. If an actor is close to all other actors then this actor is considered important.

Definition: Closeness; denoted $C_c(n_i)$, is defined by

$$C_c(n_i) = \left[\sum_{j=1}^{k} d(n_i, n_j)\right]^{-1}, \tag{2}$$

where c represents closeness, $d(n_i, n_j)$ is the shortest distance between node i and node j.

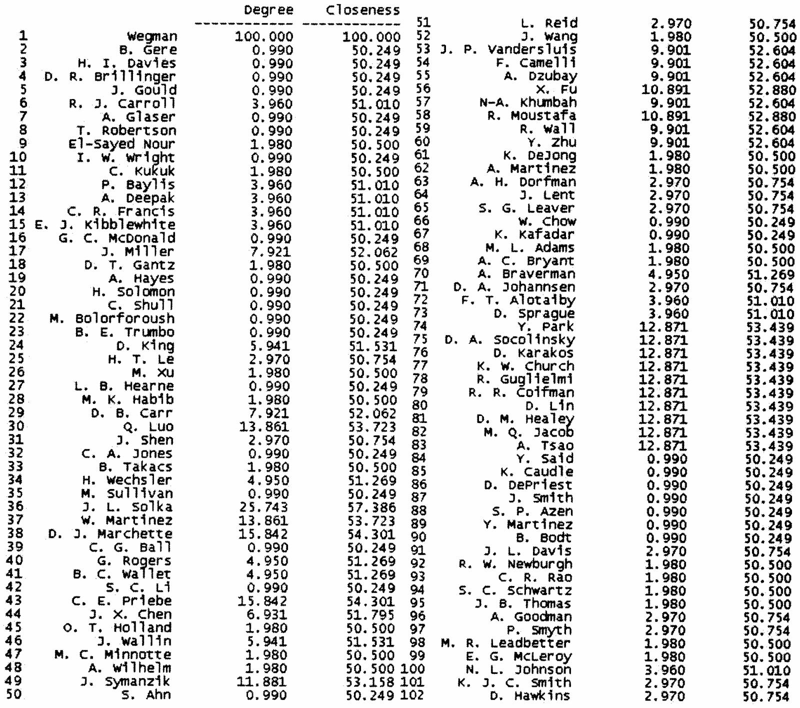
For example, the network in Figure 4 has the following closeness measures:

Distance	Ego	A	B	C	D	E	F	G
Ego	0	1	1	1	1	1	1	1
A	1	0	2	1	2	2	2	2
B	1	2	0	2	2	2	2	2
C	1	1	2	0	2	2	2	2
D	1	2	2	2	0	2	2	2
E	1	2	2	2	2	0	2	2
F	1	2	2	2	2	2	0	2
G	1	2	2	2	2	2	2	0

Sum	Closeness	Normalized
7	0.143	1.000
12	0.083	0.583
13	0.077	0.538
12	0.083	0.583
13	0.077	0.538
13	0.077	0.538
13	0.077	0.538
13	0.077	0.538

Let us apply the centrality measures to Wegman's network, Figure 9 shows a table of centrality measures. In terms of degree (a local measure), aside from Wegman, which is the most central person in the network simply because he wrote with every member of the network; thus he has the highest degree since he is connected to all other nodes, Solka with normalized node degree of 25.743 comes in second place, he is an important figure because he is the actor with most ties. The third place is shared by Marchette and Priebe with normalized node degree of 15.842.

Using closeness metric, Wegman comes in first place, he is the most central person. Solka again has the second highest closeness with a normalized value of 57.386, he is considered important because he is relatively close to all other actors. And finally, Marchette and Priebe



		Degree	Closeness
1	Wegman	100.000	100.000
2	B. Gere	0.990	50.249
3	H. I. Davies	0.990	50.249
4	D. R. Brillinger	0.990	50.249
5	J. Gould	0.990	50.249
6	R. J. Carroll	3.960	51.010
7	A. Glaser	0.990	50.249
8	T. Robertson	0.990	50.249
9	El-Sayed Nour	1.980	50.500
10	I. W. Wright	0.990	50.249
11	C. Kukuk	1.980	50.500
12	P. Baylis	3.960	51.010
13	A. Deepak	3.960	51.010
14	C. R. Francis	3.960	51.010
15	E. J. Kibblewhite	3.960	51.010
16	G. C. McDonald	0.990	50.249
17	J. Miller	7.921	52.062
18	D. T. Gantz	1.980	50.500
19	A. Hayes	0.990	50.249
20	H. Solomon	0.990	50.249
21	C. Shull	0.990	50.249
22	M. Bolorforoush	0.990	50.249
23	B. E. Trumbo	0.990	50.249
24	D. King	5.941	51.531
25	H. T. Le	2.970	50.754
26	M. Xu	1.980	50.500
27	L. B. Hearne	0.990	50.249
28	M. K. Habib	1.980	50.500
29	D. B. Carr	7.921	52.062
30	Q. Luo	13.861	53.723
31	J. Shen	2.970	50.754
32	C. A. Jones	0.990	50.249
33	B. Takacs	1.980	50.500
34	H. Wechsler	4.950	51.269
35	M. Sullivan	0.990	50.249
36	J. L. Solka	25.743	57.386
37	W. Martinez	13.861	53.723
38	D. J. Marchette	15.842	54.301
39	C. G. Ball	0.990	50.249
40	G. Rogers	4.950	51.269
41	B. C. Wallet	4.950	51.269
42	S. C. Li	0.990	50.249
43	C. E. Priebe	15.842	54.301
44	J. X. Chen	6.931	51.795
45	O. T. Holland	1.980	50.500
46	J. Wallin	5.941	51.531
47	M. C. Minnotte	1.980	50.500
48	A. Wilhelm	1.980	50.500
49	J. Symanzik	11.881	53.158
50	S. Ahn	0.990	50.249
51	L. Reid	2.970	50.754
52	J. Wang	1.980	50.500
53	J. P. Vandersluis	9.901	52.604
54	F. Camelli	9.901	52.604
55	A. Dzubay	9.901	52.604
56	X. Fu	10.891	52.880
57	N-A. Khumbah	9.901	52.604
58	R. Moustafa	10.891	52.880
59	R. Wall	9.901	52.604
60	Y. Zhu	9.901	52.604
61	K. DeJong	1.980	50.500
62	A. Martinez	1.980	50.500
63	A. H. Dorfman	2.970	50.754
64	J. Lent	2.970	50.754
65	S. G. Leaver	2.970	50.754
66	W. Chow	0.990	50.249
67	K. Kafadar	0.990	50.249
68	M. L. Adams	1.980	50.500
69	A. C. Bryant	1.980	50.500
70	A. Braverman	4.950	51.269
71	D. A. Johannsen	2.970	50.754
72	F. T. Alotaiby	3.960	51.010
73	D. Sprague	3.960	51.010
74	Y. Park	12.871	53.439
75	D. A. Socolinsky	12.871	53.439
76	D. Karakos	12.871	53.439
77	K. W. Church	12.871	53.439
78	R. Guglielmi	12.871	53.439
79	R. R. Coifman	12.871	53.439
80	D. Lin	12.871	53.439
81	D. M. Healey	12.871	53.439
82	M. Q. Jacob	12.871	53.439
83	A. Tsao	12.871	53.439
84	Y. Said	0.990	50.249
85	K. Caudle	0.990	50.249
86	D. DePriest	0.990	50.249
87	J. Smith	0.990	50.249
88	S. P. Azen	0.990	50.249
89	Y. Martinez	0.990	50.249
90	B. Bodt	0.990	50.249
91	J. L. Davis	2.970	50.754
92	R. W. Newburgh	1.980	50.500
93	C. R. Rao	1.980	50.500
94	S. C. Schwartz	1.980	50.500
95	J. B. Thomas	1.980	50.500
96	A. Goodman	2.970	50.754
97	P. Smyth	2.970	50.754
98	M. R. Leadbetter	1.980	50.500
99	E. G. McLeroy	1.980	50.500
100	N. L. Johnson	3.960	51.010
101	K. J. C. Smith	2.970	50.754
102	D. Hawkins	2.970	50.754

Figure 9: Normalized nodes degree and closeness for all actors.

have a closeness normalized value of 54.301, which puts them in third place. W. Martinez with a normalized closeness value of 53.723 comes in fourth place. Notice that no other coauthor has a normalized closeness value less than 50.249. There are two reasons for that, one is that we are considering only first level coauthorship, which means that most actors will relatively have similar values and close to that one author, and secondly, one should keep in mind that the Wegman's network is a star graph with one main figure in the center. We expect the second level coauthorship network to act differently and provide more insight on the dynamics of this network.

Figure 10 is a modified version of Figure 3 emphasizing nodes degree and tie strength. Nodes color and size are set by the attribute node degree, while edges color and thickness are set by the attribute tie strength, which represents frequency of communication. Color palettes of nodes degree and ties strength are shown in Figure 11. As we mentioned earlier Solka has the second largest node degree, note that Priebe and Marchette have the same node color and size since they are in third place. Also, we can see that W. Martinez and Luo have relatively large nodes with the same node color. Yet, if we consider ties strength instead; clearly Solka has the highest frequency then comes W. Martinez in second place. In parallel, another interesting hidden feature is revealed by the graph, Solka and W. Martinez have the strongest tie among all coauthors, they wrote with each other (17 times) more than any other two coauthors did in the network excluding Wegman. The edges (Solka, Priebe) and (Solka, Marchette) have the same color and thickness, which suggests that Priebe and Marchette coauthored with Solka the same number of times, in fact, they coauthored four times. Note that any two or more nodes or edges have the same color and/or thickness imply that they have the same node degree and/or tie strength.

Definition: Graph diameter is the longest geodesic between any two nodes, where the geodesic is the length of the shortest path between any two nodes.

The diameter of Wegman's network is 2, this is simply because the network is a star graph and because we are just studying the first-level network.

Definition: Graph density is defined as the ratio of number of edges in the graph to the total possible number of edges in a graph.

$$D = \frac{E}{k(k-1)/2} = \frac{2E}{k(k-1)} \tag{3}$$

Using `UCINET-6`, Wegman's network graph density is 0.0986.

4.2 Network Connectivity

Definition: A dyad is a pair of nodes and the edge connecting them.

Definition: A triad is a set of three nodes and the edges connecting them.

A triad is identified by a M-A-N number system of three digits and a letter. The first

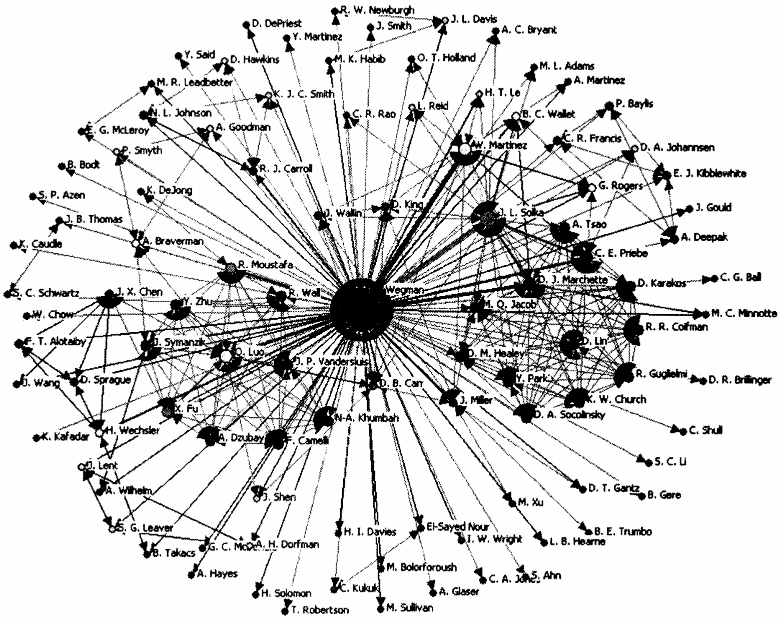


Figure 10: Modified version of Wegman's network. Nodes color and size are set by the nodes degree, while the edges thickness and color are set by the tie strength.

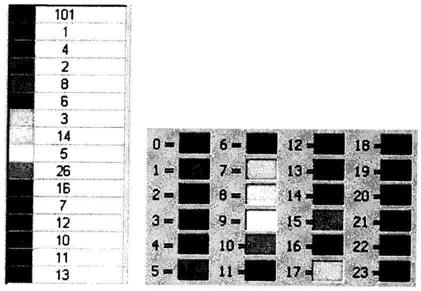


Figure 11: Color palettes of nodes degree and ties strength (left node degree, right tie strength).

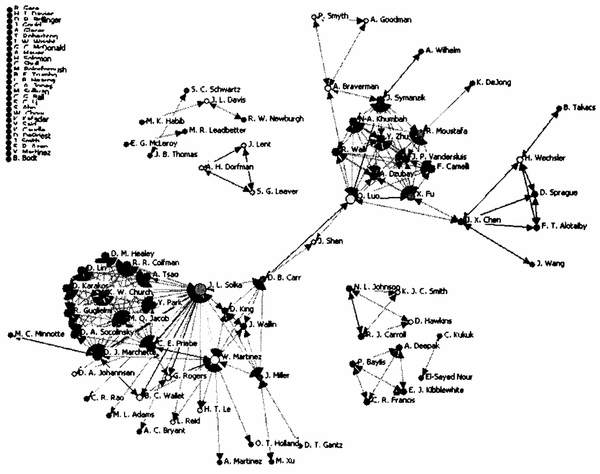


Figure 12: Wegman's network without Wegman.

digit indicates the number of mutual positive dyads (M), the second digit is the number of asymmetric dyads (A), and the third digit is the number of null dyads (N). Sometimes, a letter which refers to the direction of the asymmetric choices is added to distinguish between triads with the same M-A-N labeling digits: D for down, U for up, C for cyclic, and T for transitive [3].

We then examine the network without the star node Wegman in attempt to discover secondary structure and how the network behaves without the main actor. Figure 12 shows this network without Wegman. The graph is now partially disconnected with a number of isolated nodes; in this case, Wegman is considered a cutpoint and the edges {(Carr, Luo), (Carr, Shen)} are considered local bridges. Notice that it is less frequent to find global bridges. Both of Carr and Luo are cutpoints; by removing either of these nodes the subnetwork will be disconnected. Triads are obvious in this network; for instance, the triad 300 (mutual actors agreeing) {(Carr, Shen, Luo)}, which also forms a clique, is vital to the network in the sense that by removing this clique the network will be further disconnected and thus have more than one component (maximal connected subgraph). We will discuss cliques more in section 4.3.

Some actors are still connected with others forming subnetworks. The actors {(Lent, Leaver, Dorfman)} form again the triad 300; also known as triad 16, it refers to the structure with reciprocal links (A writes with B and B writes with A) among three different coauthors.

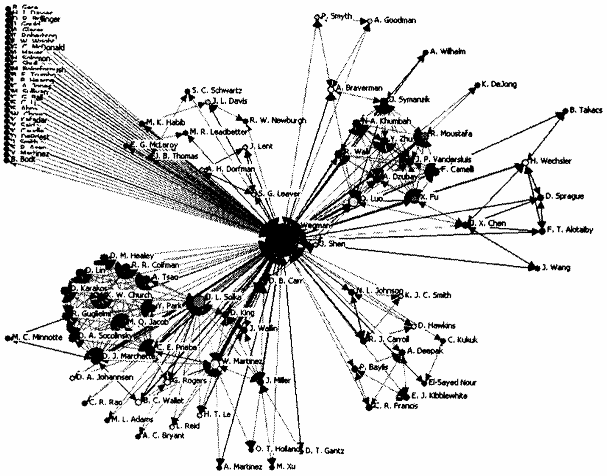


Figure 13: Wegman's network with Wegman.

Figure 13 shows the network with Wegman.

4.3 Cohesive Subgroups

One of the most interesting features in a network that caught structural analysts' attention is secondary sub-structures such as network cohesion. Researchers interested in cohesive subgroups gathered and studied sociometric data on affective ties in order to identify "cliquish" subgroups (face-to-face group). The clique is the foundational idea for studying and analyzing cohesive subgroups in social networks.

Definition: A clique in a graph is a maximal complete subgraph of three or more nodes, mutual dyads (2 nodes) are not considered to be cliques [4].

It consists of a subset of nodes all of which are adjacent to each other, and there are no other nodes that are also adjacent to all of the members of the clique. A clique is a very strict definition of cohesive subgroups. Cliques are a subset of the network in which the actors are more closely and intensely tied to one another than they are to other members of the network and if one actor disappears for any reason, the other two can still write/talk to each other. As an illustration, in Figure 4, the nodes {Ego, A, C} form a clique.

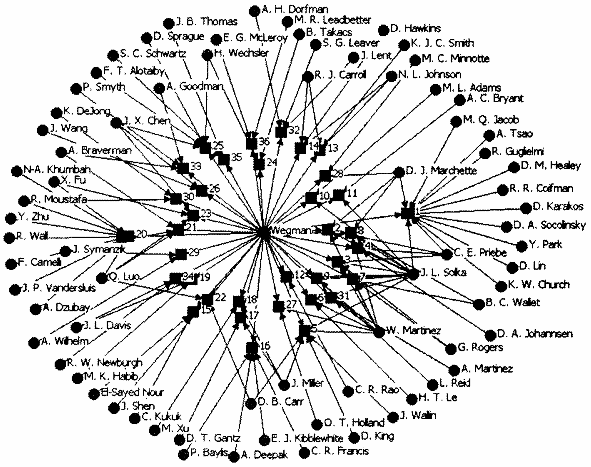


Figure 14: The 36 clique sets in Wegman's network.

In Wegman's network there are 36 cliques, Figure 14 shows all 36 clique sets with the coauthors. For example, in the relations of coauthors, clique number 11 consists of the nodes (Wegman, Solka, Bryant), clique number 9 consists of the coauthors (Wegman, Solka, W. Martinez, Reid), clique number 2 consists of the actors (Wegman, Solka, W. Martinez, Marchette, Priebe). We can also observe that an actor can be a member of one or more clique such as Solka.

Notice that cliques in a graph may overlap. The same node or set of nodes might belong to more than one clique (some cliques contain more than one member in common). Also, there may be nodes that do not belong to any cliques. However, no clique can be entirely contained within another clique, because if it were the smaller clique then it would not be maximal. Figure 15 shows the clique overlap. There is a considerable overlap among the cliques in the coauthor relation, more than one coauthor belongs to one or more cliques.

Cliques are interesting to study because suppose the actors in one network form two non-overlapping cliques; and that the actors in another network also form two cliques, but that the memberships overlap (some people are members of both cliques). Where the groups overlap, we might expect that conflict between them is less likely than when the groups do not overlap [1], this is the case with Wegman, Solka, W. Martinez and Marchette. Where the groups overlap, mobilization and diffusion may spread rapidly across the entire network; where the groups do not overlap, traits may occur in one group and not diffuse to the other.

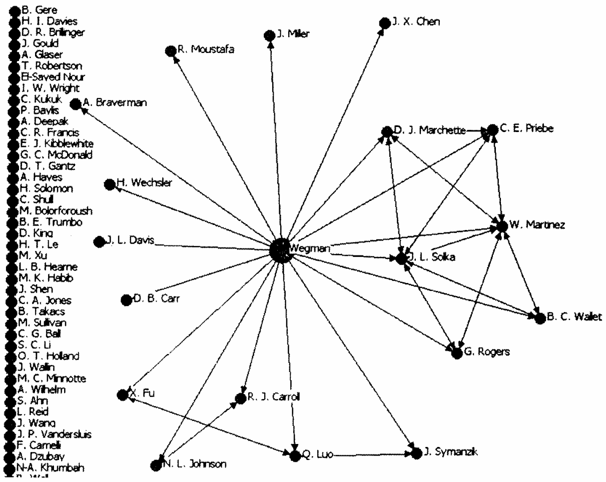


Figure 15: The clique overlap in Wegman's network.

4.4 Structural Equivalence

Definition: Two actors are structurally equivalent if they have the same type of ties to the same people.

We now discuss the method of partitioning actors into subsets so that actors within each subset are closer to being equivalent than are actors in different subsets. One way to display the results of a series of partitions is to construct a dendrogram indicating the degree of structural equivalence among the positions and identifying their members. Each level of the diagram indicates the division resulting from a split of the previous subset [4]. A dendrogram thus represents a clustering of the actors, those actors who are connected by branches low in the diagram are closer to being perfectly structurally equivalent, whereas subsets of actors who are joined only through paths high up the diagram are less structurally equivalent (or are not equivalent at all). In brief, the lowest position in the diagram indicates that every actor is different while the highest position indicates that all actors are the same; what is in between is more important in terms of structural equivalence. Figure 16 shows the cluster diagram of Wegman's network.

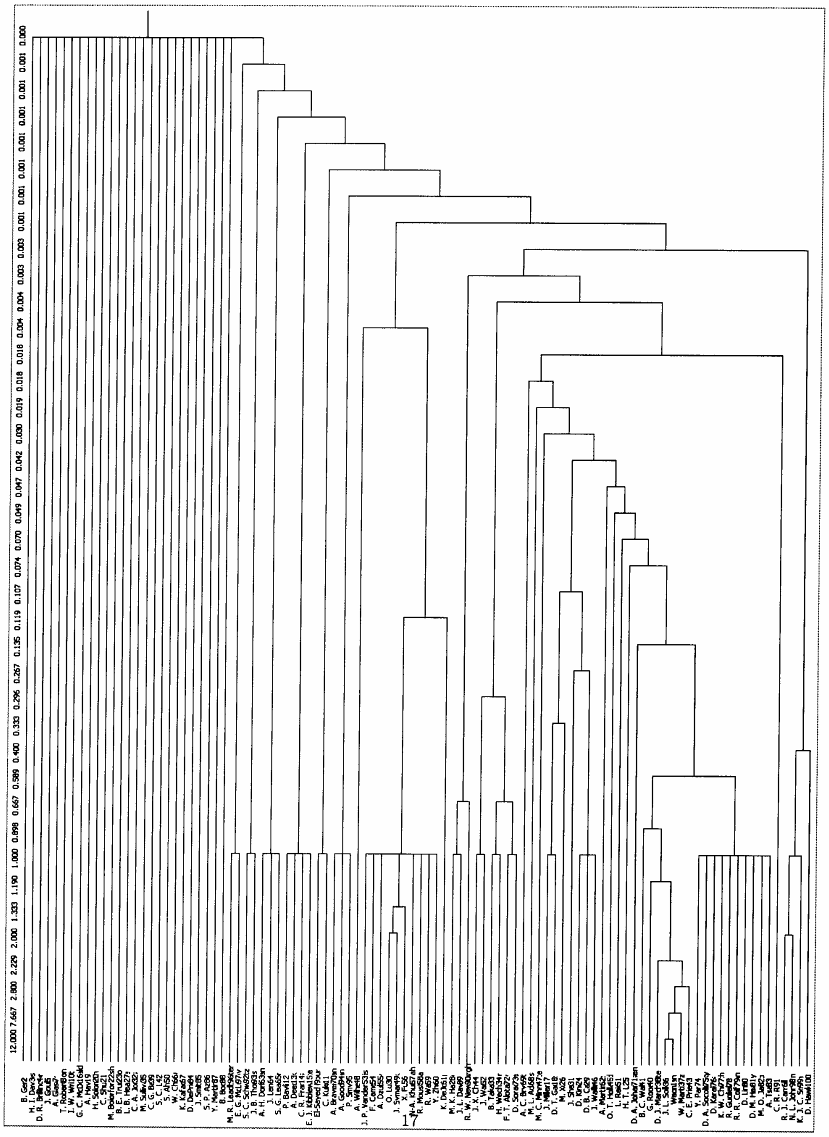


Figure 16: Dendrogram of Wegman's network.

4.5 Blockmodeling

Definition: A blockmodel is the process of identifying positions in the network. A block is a section of the adjacency matrix "a group of people" structurally equivalent. It consists of two things according to Wasserman and Faust [4]:

- A partition of actors in the network into discrete subsets called positions.
- For each pair of positions a statement of the presence or absence of a tie within or between the positions on each of the relations.

A blockmodel is thus a hypothesis about a multirelational network. It presents general features of the network, such as the ties between positions, rather than information about individual actors.

A blockmodel is a simplified representation of multirelational network that captures some of the general features of a network's structure. Specifically, positions in a blockmodel contain actors who are approximately structurally equivalent. Actors in the same position have identical or similar ties to and from all actors in other positions. Thus, the blockmodel is stated at the level of the positions, not individual actors.

Figure 17 shows these two clumps clustered in the upper left corner of the adjacency matrix, there is a total of four clusters in the graph. Each member of these clusters in structurally equivalent. The graph is based on random start blockmodeling applied on the network using structural equivalence while setting the number of cluster to be four.

5 Advanced Analysis

5.1 Discarding Weak Ties

We next present some discussion on the network excluding the edges having weight=1, i.e. all the coauthors who wrote with Wegman only once. The basic concept is that actors who communicated "wrote" with Wegman only one time are most likely to be students who graduated and are no longer connected to Wegman in some sense or coauthors who have weak ties with Wegman at the current time. In conclusion, these are the ones with minimal impact on the network. We will assume that all the coauthors with tie strength being one have not coauthored with Wegman and therefore will be treated as isolated nodes in the network. Figure 18 shows the network without the edges with frequency = 1 together with the corresponding edge weight. As before, nodes' color and size are set by the attribute "node degree" while edges' color and thickness are set by the attribute "tie strength".

Figure 19 shows the cliques not including the nodes with edge weight=1, the number of cliques is 14.

Figure 20 shows the network without Wegman. Clearly, the network is disconnected with fewer components. There are three subgroups that are still relatively strong, these subgroups

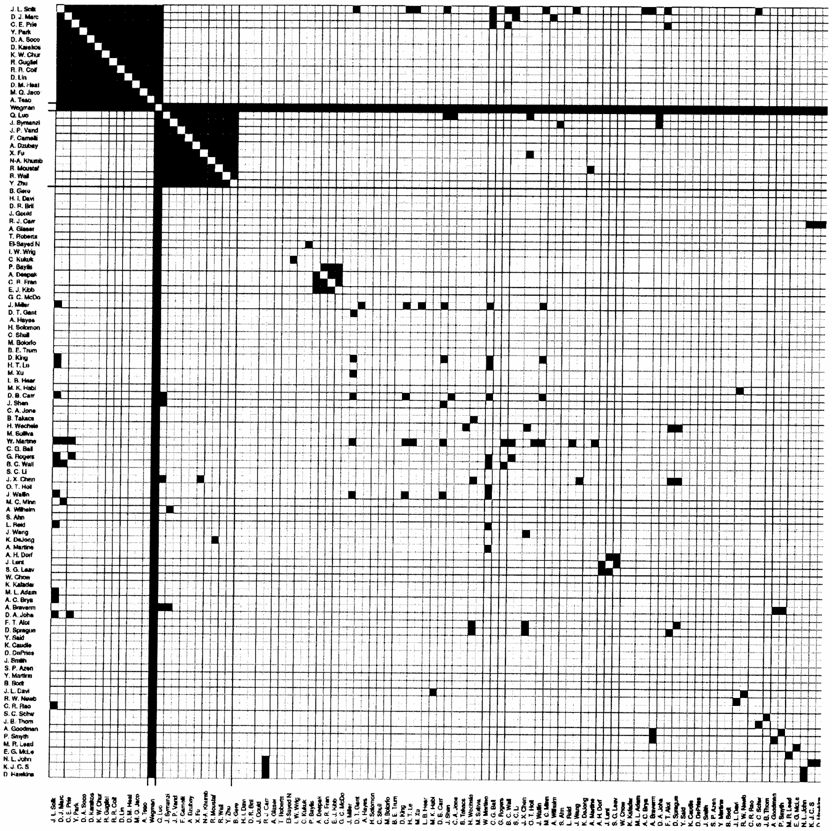


Figure 17: Random blockmodel using structural equivalence with four clusters.

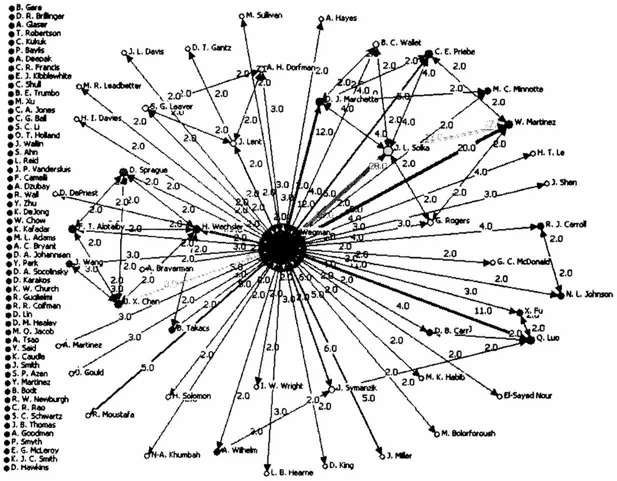


Figure 18: The network with coauthors having tie frequency=1 isolated.

are self-sustainable and can live on their own using "Ecology" terms, they form a separate subnetwork independently of Wegman. In the absence of Wegman, these coauthors can still write with each other. That kind of network is called support network, it has flat structure with few holes and high redundancy. One drawback about the support networks is the increased cost of coordination.

5.2 Discarding Irrelevant Nodes

At this stage, we will remove two of the clump subnetworks (cliques) for the following reasons. The first set of names to be deleted is {Y. Park, D. Socolinsky, D. Karakos, K. Church, R. Guglielmi, R. Coifman, D. Lin, D. Healey, M. Jacob, A. Tsao}; there was a project called Automated Serendipity and the main authors of the paper decided to add these individuals who contributed minimally to the paper. The second set of names to be removed include {R. Wall, Y. Zhu, J. Vandersluis, A. Dzubay, F. Camelli}; these individuals attended a course on Virtual Reality taught by Wegman who decided to credit everyone on the publication even though they did not write anything in the paper.

Figure 21 shows the network with the above nodes being removed. We can characterize typical behaviors and types of interactions in the following manner. Structural holes are obvious characteristics of ego networks; in this network, Wegman plays the role of a brokerage. Structural holes have low redundancy and cause stress because there are too many

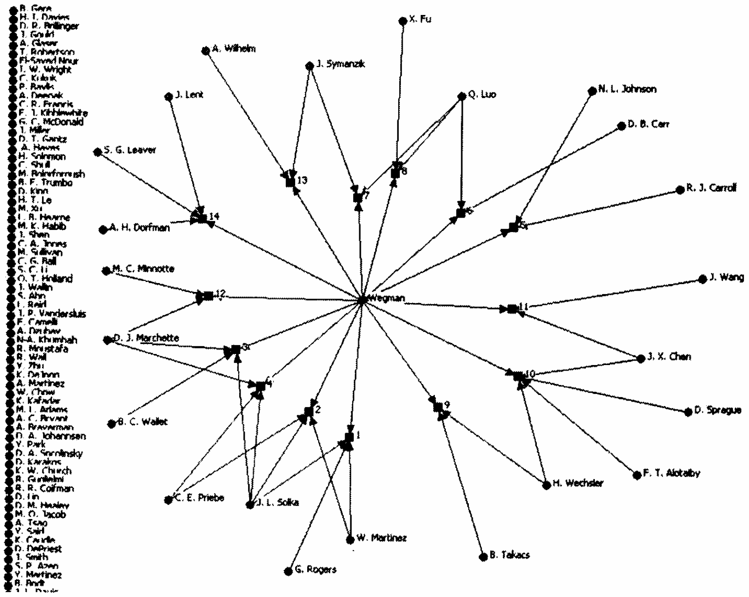


Figure 19: The clique set with coauthors having tie frequency=1 isolated.

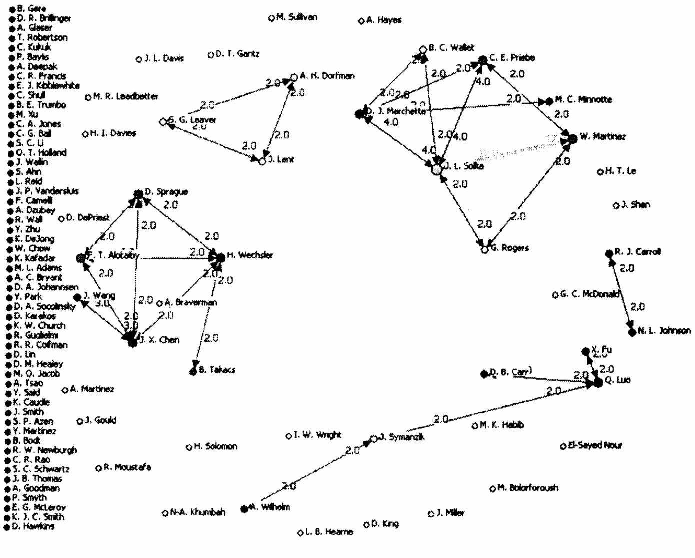


Figure 20: The network without Wegman.

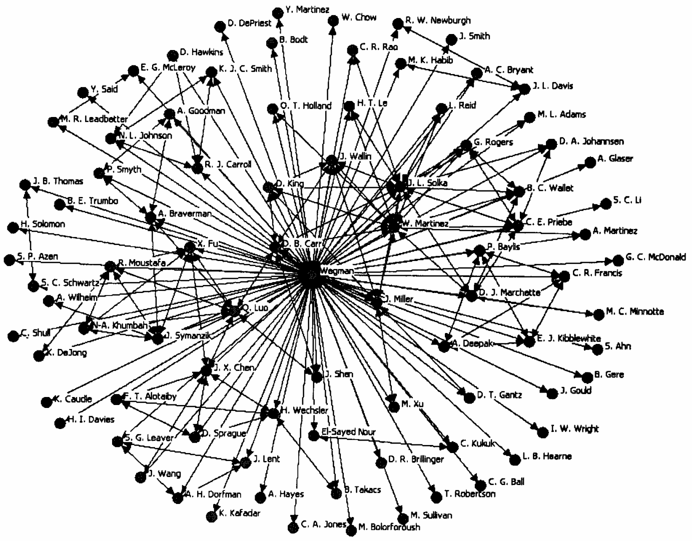


Figure 21: Wegman's network without the two clumps.

nodes connected to the brokerage. The basic form of structural holes is a triad with one edge missing, in which two actors communicate with the same person, but do not communicate with each other. This can be easily seen in Wegman's first-level network, see also Figure 4.

Figure 22 shows the adjacency matrix after the two sets of nodes being removed. Figure 23 shows the network emphasizing node degree and tie strength. Figure 24 shows the 35 cliques sets. Figure 25 shows the proximity (weighted adjacency) matrix in grey scale; the darker the color the higher the frequency.

Lastly, we will apply blockmodeling techniques on the network in its final format. Figures 26 and 27 show three and four clusters respectively of structurally equivalent actors. The method of building the blockmodel is iterative and requires steps; the number of repetitions used to form the blockmodel are set to 50. There are few remarks worth mentioning in this sense. Members of each cluster are structurally equivalent. We can also see that Wegman always forms a separate cluster since he is acting as a brokerage leading to the fact that in the case of two clusters only; Wegman will form one cluster and the rest of the coauthors will form the other. Well, this is expected for the following two reasons – the network is an ego network, and secondly this network resembles only the first-level coauthorship.

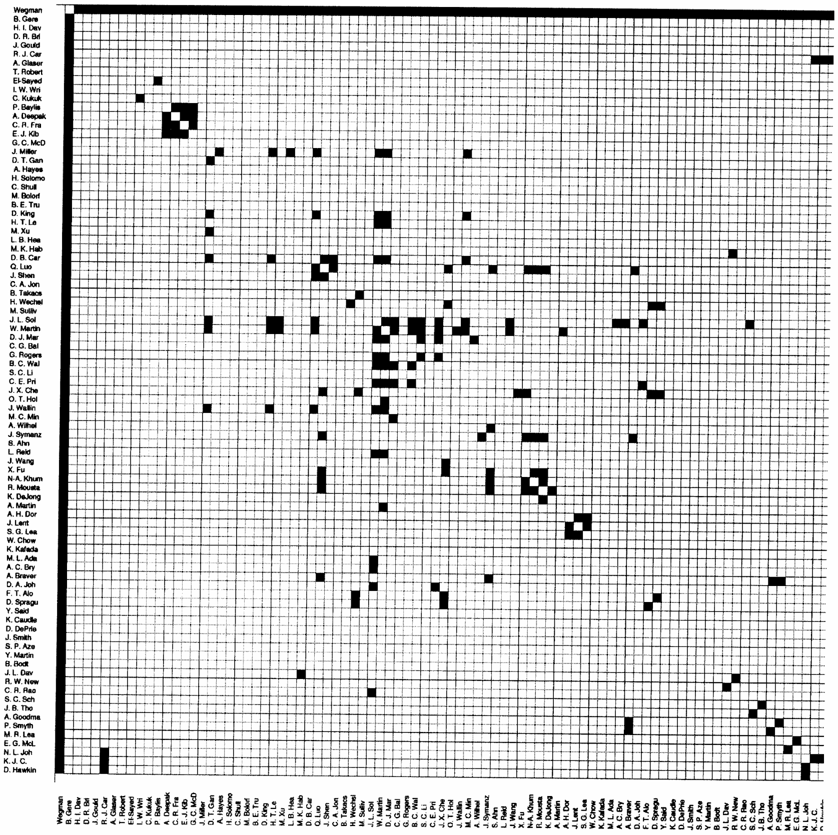


Figure 22: The adjacency matrix. Each black square indicates a coauthor relation.

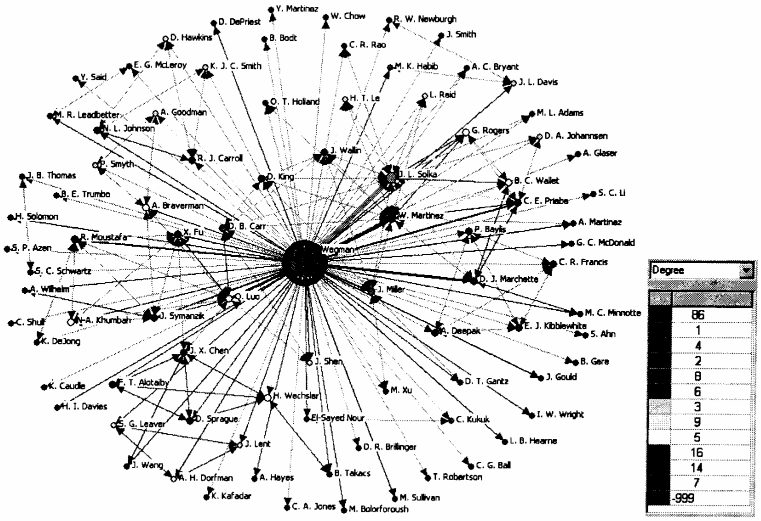


Figure 23: Wegman's network without the two clumps emphasizing node degree and tie strength.

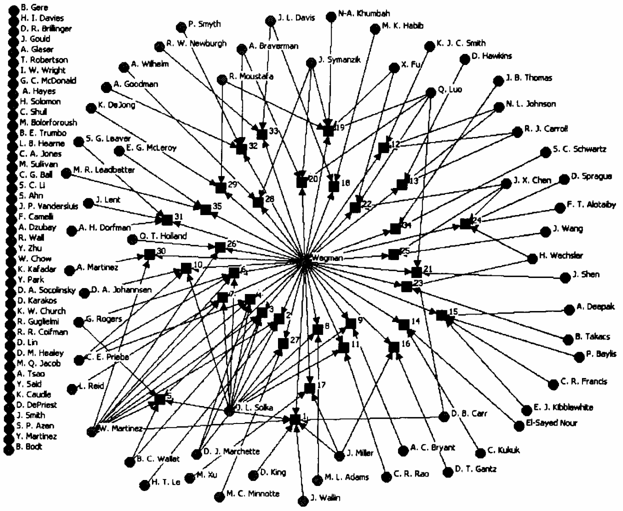


Figure 24: The 35 clique-sets.

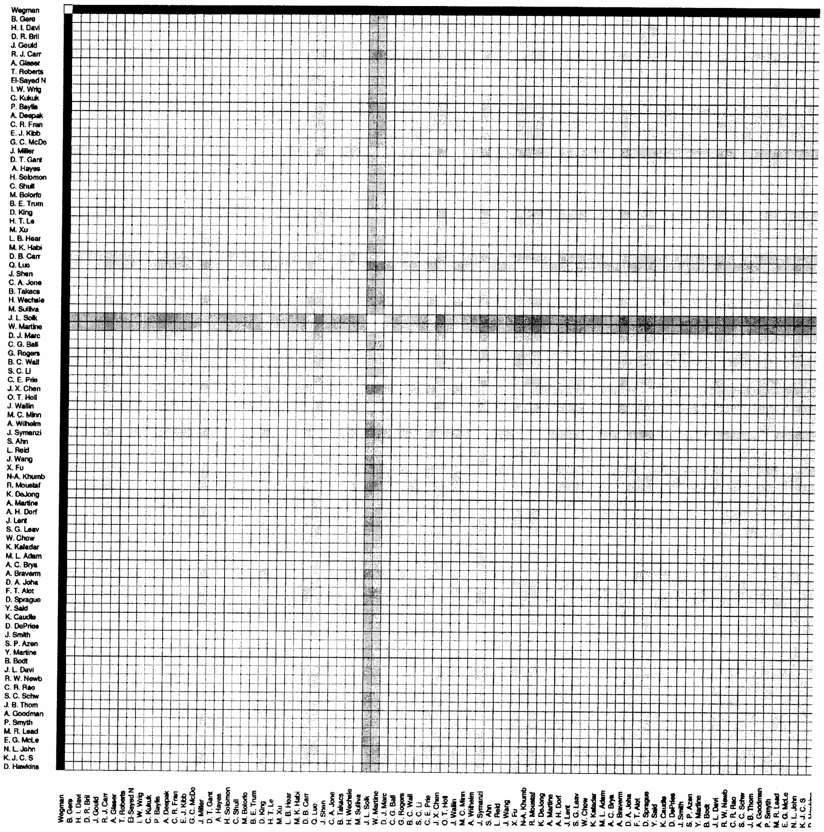


Figure 25: The proximity matrix in grey scale; the darker the color the higher the frequency.

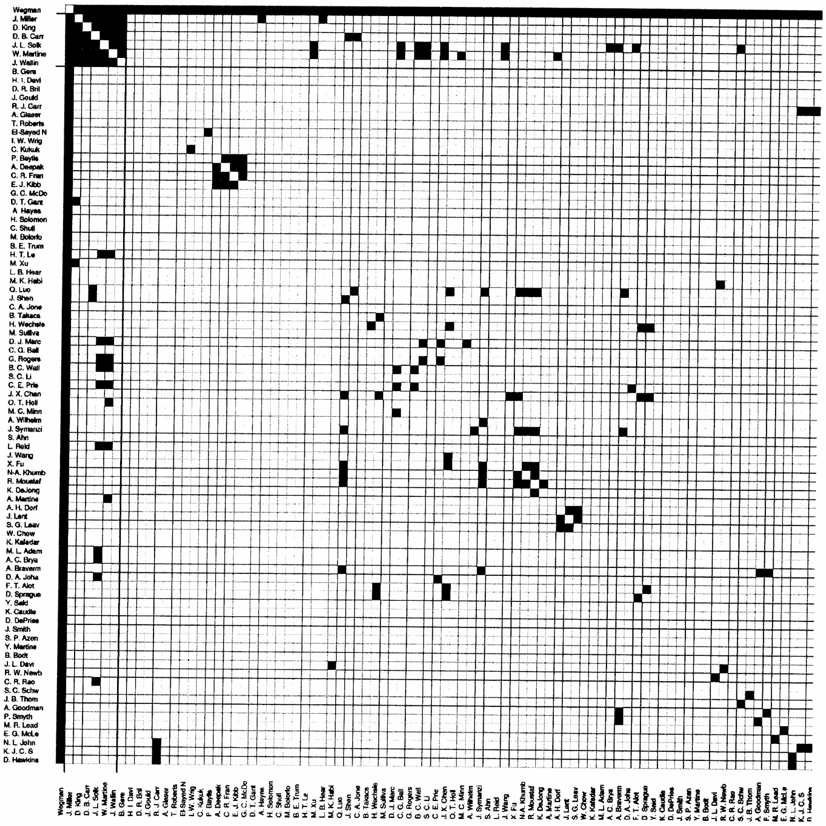


Figure 26: Random start blockmodel with three clusters using structural equivalence.

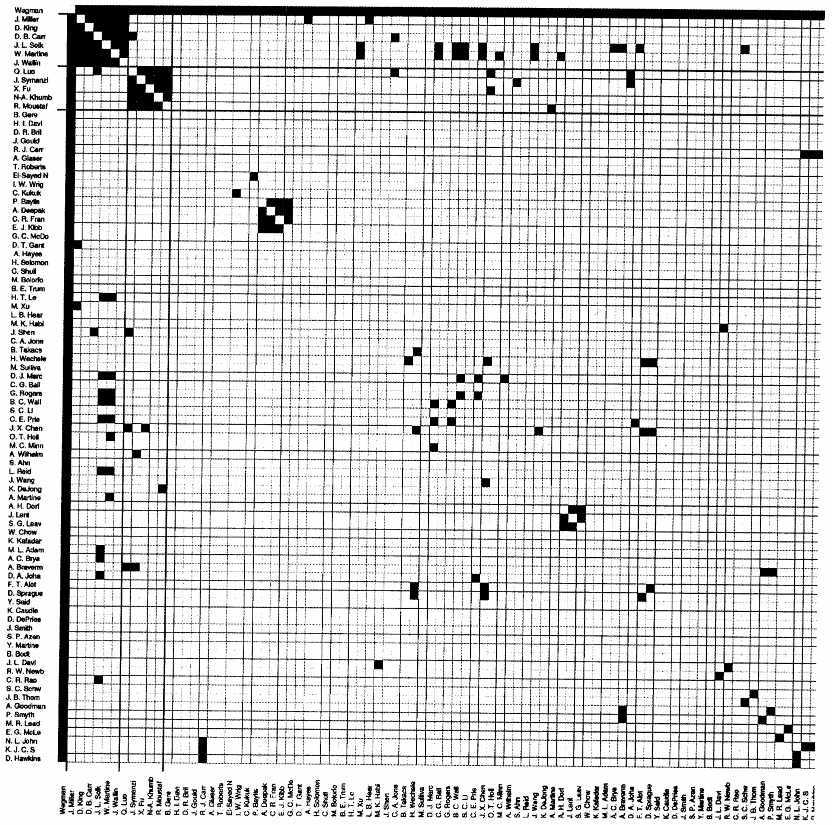


Figure 27: Random start blockmodel with four clusters using structural equivalence.

6 Conclusion

There is a potential "elite" group consisting of (Wegman, Solka, W. Martinez, Marchette, and Priebe). Members of this important group are high in degree, closeness and tie strength, removing these vertices will jeopardize the status and connectivity of the network. Structural holes are yet another characteristic of this ego network.

The analysis presented in this paper suggests that Wegman operates a "mentor" network with most of the coauthors being younger than him. Most of them are individuals who worked with Wegman to establish their future academic/industrial career and then left. The exception is the elite group, which were already established and have maintained communication up to the present with Wegman.

We can also argue on the quality versus quantity of the publications. Wegman favored quality of the work rather than quantity. This observation can be concluded by the many coauthors connected to him with few publications.

Investigating the second level coauthorship network covering at least the elite group is of importance, it will provide a more clear and accurate picture of who-wrote-with-who and which coauthors are critical to the status of the network. The network is expected to expand and fold into itself. The second-level analysis of Wegman's network will be the next phase.

Acknowledgement

I would like to express deep gratitude to my advisor Dr. E. Wegman, Dr. M. Tsvetovat and Dr. Y. Said whose guidance, support and advice were very helpful for the successful completion of this project.

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